Proceedings of the Royal Society of Edinburgh

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PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

EDINBURGH.

VOL. VII.

NOVEMBER 1869 TO JUNE 1872.

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VII. 1869–70. No. 80.

EIGHTY-SEVENTH SESSION.

Monday, 22d November 1869.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Council were elected:—

President.
PROFESSOR CHRISTISON, M.D.

Honorary Vice-President.
HIS GRACE THE DUKE OF ARGYLL.

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Dr LYON PLAYFAIR, C.B.
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Professor KELLAND.
The Hon. Lord NEAVES.
Professor Sir WILLIAM THOMSON.
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Professor TAIT.
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Dr HANDYSIDE.
ARCHIBALD GEIKIE, Esq.
Professor A. CRUM BROWN.
Principal Sir A. GRANT, Bart.
Rev. Dr W. LINDSAY ALEXANDER.

Monday, 6th December 1869.

The Hon. Lord Neaves, Vice-President, read the following Address:—

I have been deputed by your President to address you to-night from this chair, and so to attempt a task which would have been much better performed by one who possesses all the requisite scientific acquirements which I want, and without which, I fear, justice can only be imperfectly done to the work which I have undertaken.

It is usual at this meeting to give some notice of those of our Members who have died during the preceding year, and the list on this occasion contains so many, and some of them such distinguished names, that it will leave me no space for touching on other topics.

I cannot mention the name of Dr James Begbie to an audience like the present without feeling that it recalls to them pleasing remembrances and painful regrets connected with one who was so highly esteemed among us as an eminent physician and an excellent man, and who, but a little while ago, seemed likely for some years to continue his course of usefulness and success.

To myself the subject is specially calculated to communicate such feelings. Dr Begbie was my early school-fellow and friend, and in that relation, and also in my resort to him as a medical attendant in whose anxiety and skill I had the utmost confidence, there were many years, more than half a century, of cordial intercourse between us.

Dr Begbie was born in Edinburgh in October 1789. He was educated at the High School and at the University of Edinburgh, and early betook himself to medical studies. According to the system then established, but now I understand wholly or almost wholly discontinued, he became an apprentice with Dr Abercrombie, and was afterwards his assistant; in which capacity he had excellent opportunities of learning his profession, and of practically applying

his natural talents and theoretical studies. At this period, too, he showed those kindly and amiable qualities for which he was afterwards distinguished, and which gained him the affection both of his principal and of the pupils of Dr Abercrombie, with whom he was brought in contact, and who in a great measure were placed under his guidance and professional instruction. Dr Begbie in his turn became, under the system already noticed, the master of apprentices of his own, who regarded him with the same feelings, and among whom were some of the most esteemed medical men now among us.

Dr Begbie, on relinquishing his connection with Dr Abercrombie, became engaged in an extensive practice as a family medical attendant, and continued in that branch of the profession till about twenty years ago, when he confined himself entirely to the functions of a consulting physician, in which he was eminently successful, his assistance being extensively resorted to both by his brethren in Edinburgh and by practitioners throughout the country, who had confidence in his skill, and in his solicitude to do his duty to the utmost.

It is perhaps a remarkable circumstance that Dr Begbie, although he had hospital experience during his studies, never acted as an Hospital Physician. It is not a little creditable to him that he should have been able otherwise to supply the want of those opportunities from which he was thus excluded, and we should by no means be tempted to recommend a similar experiment in the ordinary case. Dr Begbie, however, was specially enabled to supply any deficiency in this part of his professional career by the very extensive means of observation which were within his reach as the assistant of Dr Abercrombie, for whom, to a great extent, he conducted those post-mortem examinations and pathological inquiries which were so intimately connected with Dr Abercrombie's reputation and success, particularly in certain classes of diseases.

We are inclined to think that in some respects Dr Begbie did not do himself full justice. He worked too hard and perhaps too exclusively at his own profession; he allowed himself scarcely any time for relaxation, although he thoroughly enjoyed the too short intervals which he occasionally employed in this manner. He was

fond of natural scenery, and particularly attached to the English Lake country, and it would have been better if he had indulged his taste more in that direction. We think, too, that in another respect he denied himself some enjoyments which might have done him good. A certain quietness, if not shyness, of disposition seemed to indispose him to much social intercourse, and he seems not to have betaken himself with any degree of interest to extra professional pursuits. We hold that every hard-working man is the better for a considerable amount of social recreation, and for that relaxation which arises from the prosecution of collateral pursuits.

Though not much known as a scientific man beyond the limits of his profession, Dr Begbie distinguished himself, we believe, by several excellent essays, both of a pathological and of a therapeutical kind. We must, of course, on this subject speak entirely from hearsay; but we understand it is generally considered that his volume of "Contributions to Practical Medicine" contains much that is valuable and original. His essays on Fatty Degeneration of the Heart, and on Anæmia and its consequences, have been specially mentioned to me as having excited great attention, and obtained much praise.

In one position which he occupied Dr Begbie was very prominently useful, and deserves to be specially pointed out for general imitation. I refer to the office which for nearly forty years he held as medical adviser to the Scottish Widows' Fund Assurance Office. In saying this, I do not wish to give him any preference over his brethren who, among ourselves, hold similar situations. That would not only be invidious, but utterly unjust; for I know that all the Edinburgh offices, and I have no doubt the Scottish offices generally, are in this respect aided by advisers of the greatest skill, assiduity, and conscientiousness. But the Scottish Widows' Fund is, I believe, our oldest Edinburgh office, and certainly one of our most prosperous, and I cannot resist this opportunity of saying, without disparaging the merits and services of officers of another class in such institutions, that the character and conduct of their medical adviser must always be of the utmost importance to their prosperity. Some recent occurrences have opened our eyes to a danger that we were apt to forget, that those who profess to

give security to others, may not be themselves secure. As the epigram says,

> "Payment of premiums will but make you poorer,
> Unless you're very sure of your insurer."

And certainly there can be no disappointment more cruel, no injustice more culpable, than that which takes from hard-working men of business a share of their annual earnings on the faith of providing for their families, and then at the end leaves those families unprovided for.

Now, one of the best guarantees for the success and solvency of an insurance office is to be found in the skill and fidelity of the medical officer. It is by testing carefully the value of the lives proposed for insurance that the office is enabled to meet its engagements and realise its profits; for one great source of profit must be that the lives insured are in one sense picked lives, so that they shall not be more hazardous, but rather less so, than the average rate of life on which the tables are framed; and that if any extra hazard is run, it shall be compensated by a corresponding extra payment. The medical duty thus to be discharged is not an easy one, and is beset by many difficulties and snares. It is not always easy to detect the seeds of latent disease, even when the person insured is presented to the medical officer; and it is still more difficult when the judgment is to be formed at second-hand from information that may be careless, inaccurate, or even treacherous, and where the utmost vigilance and acuteness are required in order to detect any concealed flaw. On the other hand, it is not right that lives, even of a doubtful kind, should altogether be excluded from the benefit of insurance, and still less that the medical officer should reject any from ignorance or rashness.

The task thus devolving on Dr Begbie for the important Society to which he was attached was discharged by him in a manner highly satisfactory to his constituents, and tending, there is no doubt, to aid in achieving for that society the great and growing success which has attended it. Dr Begbie's septennial papers on the causes of death in the records of that society were extremely interesting, and, I believe, very instructive. It is a great satisfaction to his friends, and to those interested in that institution,

that his place is now filled by a son who is every way worthy to succeed him.

I shall note here some dates of the principal incidents of Dr Begbie's professional life, and add also from the "Edinburgh Medical Journal" some account of his last illness.

Dr Begbie graduated in medicine in 1821 in the University of Edinburgh. In 1822 he was elected Fellow of the College of Surgeons, and at this time entered on the duties of private medical practice. In 1847, having become much engaged in consulting practice, he joined the College of Physicians as a Fellow. Of that College he was President in 1854–56, and discharged the duties of the office with ability, dignity, and grace. For a few years after the institution of the office, he acted as one of the Examiners in Medicine in the University. During 1850–52 he was President of the Medico-Chirurgical Society. For several years he was Physician in Ordinary to the Queen in Scotland.

The illness which led to his death began in the end of 1868 from exposure to cold, which gave rise to an attack of pneumonia. This was got under, but he returned too soon to his duties, and again became ill from some long journeys which he made. It was then seen that his health was seriously impaired. He suffered much from breathlessness, and the action of the heart became embarrassed. A change of air and scene was tried without success, and on his returning home his symptoms became more violent, and his strength declined. The immediate cause of his death was pulmonary congestion. But he remained conscious and collected to the last, enduring much suffering with great patience, and looking forward to his end without fear and with a well-founded religious confidence. He died on the 26th of August 1869.

William Brand, another of our departed members, was born in 1807, in the parish of Peterhead, and received his early education in that parish. After serving an apprenticeship in Peterhead with the respectable gentlemen who were factors for the Merchant Maiden Hospital of Edinburgh in that place, he came to this city, about the year 1829, and served a second apprenticeship with Messrs Scott, Findlay, and Balderston, W.S., of which firm, after himself entering as a Writer to the Signet, he became a partner.

He was an excellent man of business, of great intelligence, accuracy, and integrity; and his high character in this respect led to his appointment, in 1846, to the secretaryship of the Union Bank of Scotland, a situation which he filled with great usefulness and universal approbation until his death. His knowledge of financial affairs, his readiness to oblige and assist wherever his services were desired, and his great courtesy and frankness, made him most acceptable to his constituents and their customers, as well as to all who came in contact with him.

Mr Brand's love of science early took the direction of a decided taste for botany, and he was one of the original members who founded the Botanical Society of Edinburgh. Of that Society he continued all along to be a most valuable member, contributing many excellent communications to it, and enriching its herbarium with many thousand specimens of interesting plants, collected by him and by his friends in the course of their numerous botanical excursions, on which he always entered with great enthusiasm, and for which he was admirably adapted by his active habits and buoyant spirits, and by his readiness to bear, and even enjoy, the little hardships and inconveniences which such excursions sometimes involve. The spoils with which these excursionists returned were given to the Society, partly for distribution, partly for preservation, and were of no small importance in fostering and diffusing a taste for botany and a knowledge of the Scottish flora.

Some months before his death Mr Brand's health began to fail; and although at first no serious alarm was felt as to his case, he at last sank rapidly and unexpectedly, and died on the 18th October last, having completed his sixty-second year.

Mr Brand was well known as an active member of the Episcopal Church of Scotland. He died deeply lamented by his relatives and friends, and amidst the general respect and regret of the community, for his excellent qualities and exemplary character.

Dr Allen Dalzell, an able and amiable member of our Society, was born in 1821 at Madras, where his father held the position of Postmaster-General. Like most children of European parents, he early came to this country and resided with his mother in Dum-

fries, where his preliminary education was mainly carried on. He served for some years, first in the navy and then in the army, and saw a good deal of actual warfare; but in 1846 he resolved to change his profession, and, having commenced with great ardour the study of medicine, he took the degree of Doctor of Medicine at the University here with high distinction. While yet a student he had rendered great assistance to Professor William Gregory in his researches as to creatine and the products obtained from uric acid, and he received from that eminent chemist a special certificate of having exhibited much original research, while he obtained at the same time from the Senatus a remission of one Annus Medicus of the usual medical curriculum. In 1853, at the time of his graduation, he obtained the gold medal of the University of Edinburgh for a series of extended researches on physiology, and in December of that year he was appointed by Professor Gregory his class and laboratory assistant, with the duty of teaching the class of Practical Chemistry. During the winter preceding the Professor's death, when he was laid aside by illness, Dr Dalzell supplied his place in the chemical class, and was afterwards appointed by Dr Lyon Playfair, Dr Gregory's successor, to the same duties of conducting the practical laboratory which he had formerly discharged. His connection with the University continued to the last, with these additional labours, that in 1859 he delivered in the New College, Edinburgh, a six months' course on Natural Science, and succeeded the late Dr George Wilson in the Chair of Chemistry and Materia Medica in the Royal Veterinary College, which office he filled for many years with credit to himself and benefit to his pupils. He was also in much request, and much esteemed as a popular lecturer on scientific subjects in various institutions in England as well as in Scotland. He was possessed of decided talents, and, with much professional information, he had great refinement and elevation of character; and his frank, affectionate, and generous disposition secured the attachment of all who knew him. With his quick feelings and impulsive disposition, it is possible that his health, already affected by overwork, may have been further injured by an unpleasant lawsuit in connection with his official position in the Veterinary College. An erroneous verdict was returned against him, but which, on

an appeal to the Court, was set aside, and a verdict in his favour unanimously given by a second jury.

His health was for some time delicate, and it was found that he had severe disease of the heart. He died on the 29th July 1869, after an illness of much suffering, borne with pious and exemplary patience. His removal, thus occurring in the prime of life, was felt as a great loss and a severe affliction by his relatives and friends.

Dr Robert Dyce was the eldest son of the late Dr William Dyce, an eminent physician in Aberdeen. He was born in November 1798, and was the eldest of a family of sixteen, of whom the late eminent artist, Mr William Dyce, was one. He took his degree of M.A. at Marischal College in 1816, and afterwards studied medicine at Aberdeen, Edinburgh, and London. After being for some time attached to the Military Hospital at Chatham, he went out, in 1821, on a staff appointment to the Mauritius. There he became extremely popular with the English residents, from whom he declined to take fees for medical attendance, but who eagerly showed their gratitude by valuable presents. He was afterwards transferred to the Cape, where he remained for five years, and married the daughter of a gentleman holding a high official position there. He returned to England in 1833, and spent a winter in Aberdeen, after which he accepted a staff appointment at Maidstone; but in 1836, on the death of his father, he was induced to settle in his native town, where he succeeded to an extensive practice and to valuable appointments. In 1860, on the union of the two Colleges at Aberdeen into one University, he was appointed to the Professorship of Midwifery, then established, having previously held a college lectureship on that branch of science for nearly twenty years.

Both as a lecturer and as a practitioner in his special department he was looked up to as a high authority; and to his students, as well as to all who came in contact with him, he recommended himself by his kind and courteous manners, and his high principles and honourable feelings, which were in every respect those of a thorough gentleman. His medical assistance to the poor was given gratuitously, with unremitting and unostentatious liberality. He was an accomplished man, well acquainted with several import-

ant branches of natural history, which he had had peculiar opportunities of studying at the Mauritius and at the Cape; and he had made extensive collections of specimens, some of which were of great value. Though not an artist, like his distinguished brother, he had a great love of art, and a fine and critical taste in painting.

He had been ailing for some little time before his death, but had not felt any serious alarm about his case. At last, however, he came to Edinburgh for medical advice, when it was found that he had acute inflammation of the lungs. It was hoped that it might easily be subdued; but the disease suddenly took an unfavourable turn, and he died in Edinburgh, 11th January 1869, in his seventy-first year.

Among our Honorary Members whom we have lost I have to notice the eminent physiologist M. Flourens, lately deceased. He is well known among us, both by his reputation and by his works; and notices of the principal events of his life are to be found in the usual books of contemporary biography. I am sorry that I have been unable to ascertain any particulars as to the cause or circumstances of his death, a matter which, in his case, and in connection with his own speculations, might be thought to possess a special interest.

He was born in the district of Herault, in France, in 1794, and early devoted himself to medical science, and particularly to physiology and biology. He made various researches and experiments on the nervous system, and on the several functions of the great sources of nervous power; and his countrymen consider that the disclosures thus made by him, preceding, as they did, the promulgation of the discoveries of Sir Charles Bell, entitle him to high praise, and form the best foundation of his scientific reputation. He published a variety of works on other cognate subjects from time to time, one of the most remarkable of these being upon "Longevity, and the amount of life diffused over the globe," in which he vindicated for man the period of 100 years as the normal duration of his existence under favourable circumstances. He was elected a member of the Academy of Sciences, of which he afterwards became one of the secretaries. He was also afterwards elected a member of the Académie Française, and had numerous other honours conferred upon him, both scientific and

political. But he seems to have valued his scientific position above all adventitious dignities. At his death he had attained his seventy-fifth year, which might be generally thought a pretty fair allowance of life; but from our ignorance of facts above alluded to, we are unable to say whether this, in his view, a *premature* termination of his existence, is or is not a confirmation of his own theory on the subject.

There is no member of the Royal Society of whom we have occasion to lament the death, and to cherish the memory, more than Principal FORBES, who was for so long a period our faithful and efficient Secretary. It will not be easy to do justice to the merits of one who had so many claims upon our gratitude and regard, and who reflected so much honour on every public institution with which he was connected.

James David Forbes was born at Edinburgh, on the 20th of April 1809, and was the son of Sir William Forbes, of Pitsligo, Bart. The death of his mother in the year after his birth, and the delicacy of constitution which proved fatal to her, made his father feel anxious about the boy's health; and as he grew up, his slender frame, and almost premature intellectual development, seemed to indicate that his education should be conducted with caution, and limited, in the first instance, to the simplest and most essential subjects. It is remarkable, that it was thought necessary, on this ground, to prohibit strictly his study of mathematics; and it was only at spare moments, and almost by stealth, that he acquired a branch of knowledge so intimately connected with the pursuits in which he was afterwards destined to excel. His preliminary education was chiefly domestic, but in due time he attended several of the classes of the Edinburgh University. On leaving it, he has told us that geology, meteorology, and physics were his favourite pursuits; and he then began those excursions at home and abroad which were to him all his life so great a source of pleasure and scientific improvement. While he was still a youth his father had occasion to spend two successive winters in Italy, whither he took his son with him; and young Forbes's natural taste for investigation led him to make frequent visits to Vesuvius and the celebrated Pillars of Serapis. His mind was strongly moved by what he

there saw; and in 1827, when eighteen years of age, his first scientific papers appeared in Dr Brewster's Journal, but without his name. Two other papers from him, on the natural features of the same region, appeared in the same journal, also anonymously, but with the signature "Delta;" and from that time forward he continued to be a regular contributor to that publication in communications which were avowed.

In 1830, in compliance with his father's wishes, Mr Forbes passed advocate at the Scottish bar, and walked the boards for a short time; but his heart was not there, and it would have been vain to confine his buoyant spirit and active frame to the close discipline of that profession, when it was in his power to indulge his tastes and faculties in the pursuit of physical science and geological exploration. He soon afterwards resolved to quit the law, and rejoiced in the change he had thus made. At this time he visited Switzerland, and imbibed that interest in the subject of the glacier formations which afterwards stimulated so much of his exertions, both as an explorer and as a scientific author.

In 1833, on returning from the Continent, he found that the Chair of Natural Philosophy had become vacant by the death of Professor Leslie, and that Forbes's friends had put him in nomination as a candidate. It was a painful position for him to occupy when his competitor was Sir David, then Dr Brewster, who had been among his earliest scientific friends, and who had fostered and encouraged his talents by the kindest sympathy and assistance. It was a keen contest, and the friends of Brewster might naturally feel indignant that so young a man should be preferred to one of such high eminence and long standing as Brewster had attained to. This preference was imputed entirely to political feeling or local influence, and these undoubtedly entered largely into the question. But the supporters of Forbes were no false prophets when they predicted for their candidate a long career of ardent exertion and eminent success, not only as a scientific inquirer, but as a lecturer and teacher; and as to his youth, it was pointed out that Maclaurin, Dugald Stewart, and other eminent professors, were appointed at as early an age, or earlier. The appointment, ultimately, had all the justification which the event could supply. Professor Forbes occupied the Chair of Natural Philosophy for more than a quarter

of a century, with the utmost honour to himself and the University to which he belonged. It is creditable to both parties, and more especially so to Sir David Brewster, that the contest which thus terminated did not dissolve their friendship, or prevent their cordial co-operation in everything that could promote the interests of science.

For a long series of summers Professor Forbes resorted to Switzerland and to other districts of alpine scenery in Europe, and thus matured those profound and important views which he promulgated on geological and other questions—in particular, on the subject of glaciers. It is quite unnecessary, and would be very presumptuous on my part, to attempt any account or criticism of his works or researches, and indeed everything that could be desired has in this respect, so far as geology is concerned, been excellently done by our friend Mr Geikie, in the minute and kindly memoir of Principal Forbes which he lately read to the Geological Society. Appended to that memoir will be found a correct and complete list, as I believe, of Principal Forbes' scientific writings, and the catalogue of our own library will supply similar information. I may shortly say, that Principal Forbes was an ardent geologist—that from an early period he had been imbued with the enthusiasm for that branch of science which prevailed among scientific men in Edinburgh in the first quarter of the present century, and that he earnestly desired to see a school of geology fully revived and established among us.

Principal Forbes, it is somewhat singular to observe, had on the motion of Dr Brewster been admitted a member of the Royal Society before he had attained his twenty-first year. The Keith Prize was twice awarded to him by the Council. In 1846, on the death of Sir John Robison, he was appointed to the office of Secretary of this Society, and for about twenty years thereafter he discharged the duties of the appointment with the most efficient assiduity and the most conscientious diligence. His desire to maintain the usefulness and the dignity of the Society, and to preserve its ranks and its discussions free from anything that was unworthy of a scientific body, and the pains that he took in procuring and preparing for publication the compositions which constitute its "Transactions," and on which its character and reputation will in a great measure permanently depend, were beyond all

praise, and were both proved and rewarded by the condition in which he maintained the Society while he was Secretary, and in which he left it when he resigned that office

On occasion of his giving up the office of Secretary, the Royal Society recorded the expression of their sense of his valuable services in the following resolution:—"That the Royal Society deeply laments that a necessity has arisen for the retirement of Principal Forbes from office as General Secretary. That it desires now to record in its minutes its grateful sense of the obligation under which it lies to Principal Forbes for the zeal and ability with which he has acted as its Secretary for the last twenty years, for the many important discoveries and inquiries in science which he has brought before its meetings, and for the eminent degree in which his exertions and example have contributed to its present prosperity; and that, as a mark of the regard in which he has been long held, alike as an office-bearer and as a cultivator of physical science, he be requested to sit to an eminent artist for his portrait, to be hung in the Society's apartments."

On the removal of Sir David Brewster to the headship of the University of Edinburgh, Professor Forbes was chosen Principal of the United College of St Salvator and St Leonard in the University of St Andrews. His failing health, which, there can be little doubt, had suffered much from excessive exertions in his mountain excursions, and perhaps also from overstrained labour in some of his scientific researches, made the retreat thus offered to him a welcome refuge from the task of daily lectures to which he had become quite unequal. For a time after his removal to the retirement of St Andrews, he seemed to be rallying in strength, with the assistance of his annual residence in the pure air and amidst the interesting scenery of Perthshire, but the improvement did not continue, and his old ailment of hæmorrhage from the lungs returned with alarming violence. He left St Andrews and removed to a milder climate, stopping ultimately at Clifton, where he died on the 31st of December 1868. We are told that "whilst his body was reduced to the last stage of weakness, his mind remained self-controlled, unclouded, and peaceful to the end." His activity and usefulness in his office of Principal of St Andrews University have been borne witness to, and a truthful and touching tribute paid to

his memory, in the address lately delivered by his excellent and accomplished successor Principal Shairp.

Principal Forbes had a certain reserve and apparent dryness of manner, but he had a kind and noble heart, an unremitting zeal for the promotion of science, a conscientious desire to discharge every duty, an ardent love of truth, and a strong detestation of injustice. He was not unmindful of what he felt to be his own claims, but he also fought many a battle in vindication of what he considered to be due to others.

The late Master of the Mint will be readily enrolled by all who knew him, or who know what he has done, as another among the great names that Scotland can boast of in chemical science.

Thomas Graham was born at Glasgow, on the 21st December 1805, and after passing through the usual course of preliminary study in that city, he entered the University of Glasgow in 1819. He early showed a strong taste for science, and a decided bias for chemistry as a pursuit. His father, it is believed, wished him to enter the Scotch Church; but Graham felt that his true vocation lay in another direction, and his desire of penetrating the secrets of natural knowledge was too strong to be repressed. Thomas Thomson was then Professor of Chemistry in Glasgow University, and it cannot be doubted that from his instruction Graham derived great benefit, and received a strong confirmation of his natural tastes in that direction. After graduating at Glasgow, he repaired to Edinburgh, and studied for two years under Dr Hope, who, if not distinguished by powers of original discovery, was an able and elegant expositor of the discoveries of others, and most successful in conducting the experiments by which his lectures were illustrated. Graham at this time also made the acquaintance of Professor Leslie, a man of undoubted originality and of most diversified knowledge, and with whom it was impossible to associate without being stimulated to intellectual exertion and scientific inquiry.

It is probable that, during the time when he was engaged in his University studies, both in Glasgow and Edinburgh, he was subjected to much anxiety as to his prospects, and as to the probability of his being able to justify, by success, the choice which he had made of a position in life, which could scarcely be said to

amount to a profession, and which, at that time in particular, promised few and scanty rewards for the efforts and sacrifices which it involved. In these trials it would appear that Graham was comforted and supported by the sympathy and affection of an excellent mother, with whom, when he was absent, he regularly corresponded, and to whom he confided his most intimate and anxious feelings.

In such circumstances, it must have been a source of pride and satisfaction to him that, in 1829, when scarcely twenty-four years of age, he was appointed Lecturer on Chemistry at the Mechanics' Institution, Glasgow, and in 1830 Professor of Chemistry at the Andersonian Institution, an event of which his mother just survived to hear.

In 1837 he was appointed Professor of Chemistry in the London University, and remained in that appointment till the year 1855. During the five and twenty years for which he thus occupied a professorial chair, first in Glasgow and then in London, Graham found himself in that position which was the one he would himself probably have selected as the best for carrying on his favourite plans of scientific investigation; and that long period was accordingly devoted to the assiduous prosecution of his great object, in the course of which his enthusiastic researches were rewarded by numerous important discoveries, which are not only in themselves valuable, but which must ever deserve the attention of chemical students, as examples of that assiduous application and persevering inquiry by which alone the hidden truths of nature can be brought to light.

It is quite beyond my power to give any detailed account of Mr Graham's discoveries, or to make a just estimate of their value in a science with which, in its rapidly advancing and ever expanding state, I am so imperfectly acquainted; but I believe the statements on the subject which lately appeared in the new periodical, "Nature," may be relied on as accurate and just; and I have been furnished from a high authority with some materials as to these points, which I shall endeavour here to embody to the best of my ability.

Graham's tendency to the prosecution of scientific discovery showed itself while he was yet a pupil of Professor Thomson in Glasgow. He made some suggestions to that Professor as to

the possibility of water playing an important part in the constitution of acids and salts. The Professor was struck by the ideas of his young pupil, and encouraged him to continue his investigations on the subject. This ultimately led to his splendid researches in phosphoric acid, as to which he shows that its three varieties—common phosphoric acid, pyrophosphoric acid, and metaphosphoric acid—differed only by containing a different number of atoms of water, chemically combined with the an-hydride. He followed this inquiry up by researches on water in salts, and showed that in a salt the different numbers are held with different degrees of tenacity. His attention was early attracted to the diffusion of gases. The manner in which gases mix with each other, and the permanence with which the intermixture is maintained, are remarkably different from what is experienced in the case of liquids; and it is probably to this fact that we owe the stability of the proportions in which the ingredients of the atmosphere are maintained, a uniformity which is so essential to organic life. The laws also according to which gaseous diffusion takes place were found by Graham to be based upon mathematical relations between their density and their velocity of diffusion, which were at once interesting and unexpected. The laws as to the effusion of gases into a vacuum, and their transpiration through narrow tubes, were also traced by him with indefatigable diligence and complete success; and it is a fact of which we may be proud, that his first paper on that subject was read before this Society. The importance of these investigations, particularly in connection with the phenomena of osmosis, will probably be seen, in its full extent, in the clue which they seem to give to some of the most remarkable facts in physiology. The discoveries of Dr Graham were due mainly, it may be said, to his close adherence to any subject on which he once entered. He never quitted it until, by steadfast attention, deliberate consideration, and varied experiment, he had extracted out of it every atom of scientific truth which it was capable of yielding. The secret of his success in this respect was probably not different from what may be seen in other eminent discoverers. Newton ascribed his achievements not to genius, but to earnest and unremitting attention; and it must be manifest how much more likely it is that a

new truth should dawn upon the mind which has been long and intently occupied with a subject than that it should be the fruit of a casual and transient consideration. It was by this habit and faculty of perseverance that Graham was enabled to do what he did; it was to this that we owe all that he has taught us as to the diffusion of gases and liquids, as well as his last and crowning discovery as to the nature of hydrogen, of which, perhaps, the full effect is not yet fully seen or recognised.

At an early stage of his inquiries as to hydrogen, he had seen that it was isomeric with some of the metals, but his later experiments went further still towards establishing the metallic character of that gas. He showed that certain metals—palladium, platinum, and iron—can, under certain circumstances, absorb considerable quantities of hydrogen gas. This he termed the "Occlusion of Hydrogen Gas." Latterly, his investigations were made almost exclusively with palladium, which absorbs a much larger proportion of hydrogen than any other metal. The method he pursued was to decompose water by a galvanic battery, the negative electrode, at which the hydrogen is liberated, being formed of a plate or wire of palladium. In this arrangement, when the decomposition takes place, oxygen is given off copiously at the positive electrode, but no hydrogen, or very little, appears at the negative in the first instance, the avidity of the palladium for oxygen requiring that it should first be saturated with that substance, after which the hydrogen begins to be given off. In this way Graham succeeded in charging palladium with a quantity of hydrogen, which, in the form of gas, would occupy 900 times the volume of palladium. The palladium so charged retains its metallic appearance, and differs from pure palladium, very much as a metal containing a small quantity of metallic alloy differs from the pure metal. From these facts, Graham inferred that hydrogen in its solid state was truly metallic, and to this substance, according to the usual analysis of chemical nomenclature, the name of hydrogenium was given, and a medal of palladium and hydrogenium in the alloyed state was struck in honour of the discovery. Another of his recent discoveries is said to have been that, while the gas shut up in terrestrial iron is carbonic oxide, the gas contained in meteoric iron is hydrogen.

Prior, I believe, to the year 1850 the Mastership of the Mint had for a long time been a political office, the occupant of which was removable with the ministry with whom he was associated. The individual who held it was, in this way, not a man of science, but a statesman of general intelligence and business habits, whose duty it was to superintend and keep to their tasks the permanent officials by whom the work was understood and performed. In 1850 a change was made in this respect, and apparently a change for the better. It was determined that the office should be held by a man of science, not connected or removable with the ministry of the day, but who should give his talents and time to the actual working of the department. The office, as thus remodelled, was conferred upon Sir John Herschel, in acknowledgment of the high eminence which he had attained in so many branches of science. He held the office till 1855, when he resigned it from bad health, and Dr Graham was then appointed. He continued to hold the office and discharge its duties till his death with the utmost diligence and efficiency.*

All who knew Graham concur in bearing testimony to the purity and simplicity of his nature, and to the justice, generosity, and kindness of his conduct. He was physically too weak, and perhaps too much engrossed with scientific objects, to enter much into society; and he had no ambition for display, but was solely bent upon the discovery of scientific truth for its own sake, and for the advancement of scientific objects. He has been cut off in the midst of a noble and useful career, when it might have been hoped that some years of active investigation would still be allowed him, and from which it is not easy to estimate what results might have followed. The loss which science has thus sustained can only be repaired by similar exertions made in a similar spirit by those who possess the natural qualifications that are essential to scientific inquiry.

Dr Graham, for some time previous to his last illness, had occasionally gone to Malvern for a day or two at the end of a week, and derived much benefit from the change. On the last

* If any further change be contemplated in this department, it is to be hoped that it will not tend to deprive men of science of what is at once a fair reward and a fitting sphere of usefulness.

occasion, however, of his being there, he had over-fatigued himself by walking, and caught a chill from falling asleep near an open window. The result was an attack of inflammation in one of the lungs. He returned immediately to London, where his medical advisers from the first took an unfavourable view of his case, either in its immediate or ulterior consequences. He died on 16th September, after ten days' illness, having been assiduously attended by his sister and one of his nieces. His remains were brought to Glasgow, and interred in the family burying-ground attached to the Cathedral, where two months before he had erected a tombstone to the memory of his parents and other members of the family, space being left merely for his own name and that of his only surviving sister.

Charles Frederick Philip von Martius, the greatest, perhaps, and most celebrated botanist of the present day, was born at Erlangen, in Bavaria, in the year 1794. His family are said to have been of Italian origin, but they had been for some time settled in Bavaria, where his father had a medical appointment in connection with the court. Young Martius received, in the first instance, the usual medical education, but when about eighteen years of age resolved to devote himself to botany, and shortly afterwards was appointed to a subordinate position in the Botanic Garden at Munich. His diligence in that situation, and the merit of some treatises which he then published, attracted the notice of Maximilian Joseph I., who was an ardent lover of plants, and a frequent visitor to the garden. In 1816, when the joint expedition was concerted by Austria and Bavaria to explore the natural history of Brazil, Martius was named by the king as the Bavarian botanist, though then little more than twenty-two years of age. He immediately set out on this enterprise, and was absent for a period of four years, having returned to Munich on the 8th of December 1820. The explorations made by the two Bavarian travellers, Spix and Martius, who proceeded in a separate direction, and over a wider field than their Austrian associates, were on a scale much larger and more comprehensive than any that had previously been attempted. The expedition, we are told, irrespective of the sea voyage, extended over nearly 1400 geographical miles, and for months led through the

most inhospitable and dangerous regions of the New World. Both explorers, however, escaped without any important disaster on the road, and they had the rare good fortune to preserve and bring home their collections, complete and uninjured, through all the perils to which they were exposed. These collections, finer and richer than all previous and most subsequent ones from Brazil, were made over to the Academy.

The task thus successfully achieved established Martius's reputation, and settled for life the special destination of his studies. He received from his sovereign distinguished honours, and was recognised by men of science as worthy of a high place among them. The publication of the narrative of this Brazilian journey, which appeared in 1823–31, and which, in consequence of the early death of Spix, was chiefly prepared by Martius, carried the admiration of his talents to a very high pitch. There was here seen a worthy rival of Alexander Humboldt; and readers were at a loss whether to admire most the copiousness of the information furnished, or the beauty of the diction, and the poetical and yet truthful power of the colouring, in which were presented all the characteristic features of those wonderful regions, with their productions and their inhabitants. A relative work at the same time was commenced, and continued in a magnificent series of volumes, exhibiting to scientific eyes the minute representation and description of the natural objects, whether plants or animals, with which the expedition had made the travellers familiar. The esteem in which these works were held procured for Martius the distinguished honour of being elected a member of the French Institute. He was enrolled in nearly all the other learned bodies in Europe; he was appointed an Honorary Member of our own Society in the year 1855.

After the accession of Louis I. to the Bavarian throne, Martius was appointed Professor of Botany in the University of Munich, and subsequently was promoted to be Chief Conservator of the Botanic Garden.

In 1823, Martius began his celebrated Monograph upon Palms, which was completed in three folio volumes in 1845. It is considered one of the finest monuments of modern botany, and is said to contain a description of 582 different species of Palm, while Linnæus had only given 15, and Humboldt 99. It was to

this work that his friends specially alluded when, in 1864, on the jubilee of his graduation at the Academy, a medal was struck in his honour, dedicated "PALMARUM PATRI," with the motto "TU PALMIS RESURGES;" and the same idea was followed when, four years afterwards, on 13th December 1868, his bier was bedecked with palm leaves, and a similar motto inscribed on his tomb.

The last great work in which Martius was engaged is the "Flora Brasiliensis," which was continued, from time to time, upon a scale worthy of the subject, and at his death had reached its forty-sixth part. It is to be hoped that it will be continued in the same spirit in which it was begun.

Martius was a most popular lecturer, and in every way a superior man. His general intellectual powers were very great, and his readiness to communicate his knowledge was unfailing. His hospitality was liberal, and his best recreation, after the labours of each day, was the reception in his house of scholars, travellers, and men of science, and more especially of young inquirers after knowledge, whose projects and aspirations he delighted to encourage and direct. He died in his seventy-fifth year; but I regret that I am unable to state any particulars as to that event, or his last illness.

Among those members whom we have this year lost by death is the late venerable and excellent pastor of St Stephen's Church, in this city. He took no prominent part as a man of science, but he felt an interest in its progress, and watched its rapid advance; and though not mixing actively in the proceedings or debates of this Society, he strongly approved of its objects and recognised its benefits. It is an honour to have such men enrolled among us, and when they are removed in the course of nature, they should not be deprived of the just tribute to which their virtues and talents are entitled.

Dr WILLIAM MUIR was a native of Glasgow, the son of a Glasgow merchant. He was a distinguished student at Glasgow University, and having chosen the Church for his profession, he was ordained in the year 1812. It is said that his own predilection originally was for the Church of England, and that he entered the Scotch Church in deference to his father's wish. However this may be, the choice then made by him was fully ratified by his ultimate convictions.

He was first assistant, and afterwards minister, of St George's, Glasgow, and was about the year 1822 removed to the New Greyfriars' Church, Edinburgh. On the erection of the parish of St Stephen's in 1828, he was appointed to that charge, which he continued to hold till his death on 23d June last.

In every situation in which Dr Muir was placed as a minister he discharged his parochial duties in the most exemplary and efficient manner; in particular in St Stephen's parish, of which he was the pastor for forty years, not only his ministrations in the pulpit, but his diligence in personal attention to his flock, his care of the young, his kindness to the sick and suffering, and his organisation for the promotion of education, and the diffusion of sound Christian faith and active Christian practice, were such as to call forth the strongest feelings of gratitude and admiration in his congregation and parishioners. His elders, embracing among them some of the most eminent and respectable of our citizens, concurred in looking upon his pastoral services as invaluable, and omitted no opportunity of testifying their confidence in his character and their sense of his worth. Documents have been placed in my hands, by some of their number, which enable me to make these statements with a perfect conviction that they are in no respect exaggerated, and that Dr Muir was, in all his parochial relations, the model of a Christian minister. I have read with peculiar interest the proceedings of his congregation in 1862, when, on occasion of his completing the fiftieth year of his ministry, they placed at his disposal the fruits of a liberal subscription among them, but which he declined to receive personally, and insisted on forming into a sinking fund, of which the proceeds were to be annually applied to pious and charitable uses, parochial or congregational. I have also read, with a perfect persuasion of its sincerity and truth, the address which the late excellent Dr Hunter delivered in 1864, on occasion of Dr Muir being compelled to withdraw from active duty in consequence of a failure of eye-sight, with which he was visited. That address was obviously from the heart of the speaker, as it must have gone to the hearts of those who heard him, and bears unequivocal testimony to the high character of the man who was the subject of it.

This is not the place to speak of Dr Muir's career or opinions,

either on religious or on ecclesiastical questions. I may venture, however, to make one or two observations in connection with these matters.

1. Dr Muir, from an early period of his ministrations, came to occupy a somewhat peculiar position as a minister. He belonged to what was called the Moderate party in the Church, having no sympathy with the strong views either of popular rights or of spiritual independence, which characterised the High Church Presbyterians. But the Moderate party had also the reputation, whether well or ill founded, of being rather *too moderate* in their doctrinal views; and, in this respect, Dr Muir's opinions and style of preaching were more decidedly and prominently evangelical, as it was called, than was generally the case with his political friends.

2. Dr Muir's opinions were always listened to in the Church Courts with respect and deference; but he was not altogether adapted to the position of a party leader, which, in other respects, he might have well attained. He had a fault, or what will be considered such by some men; but it was that fault which a delightful poet has ascribed to the greatest man of his own age—he was

"Too fond of the *right* to pursue the *expedient*."

It has been well observed to me, by one who knew him well, that it is a rare thing, and anything but a disparagement, when all that can be said against a man is, that he followed conscience exclusively, and valued integrity and independence too high for any price to tempt him even to the semblance of a surrender.

Perhaps his most marked characteristic was this high-minded conscientiousness of disposition. His habit of making conscience of everything made him appear stiff and unbending to those from whom he differed in opinion, and many may think that he took the alarm too soon and too sensitively when he thought that even the outworks of principle were in danger. His steadfastness certainly to what he held the truth never quailed; his independence was unshaken by what to others might even seem legitimate feelings. His superiority to all selfish motives had in it the essence of chivalry. Though to strangers his manner was reserved, those who had the privilege of familiar intercourse with him knew that beneath the surface there lay a native geniality of temper which

could break forth and sparkle into its natural gleams, and a heart as warm as ever beat in human bosom.

Dr Muir was an accomplished scholar, and all along kept himself abreast of the literature and science of the day. He was well read in the classics, and had a more than usual acquaintance with the literature of his own profession. Suffering for a year or two before his death under nearly total blindness, he had a reader always with him, to read to him his favourite authors, not in English merely, but in Latin and Greek, and even Hebrew.

Dr Frederick Penney, Professor of Chemistry in Anderson's Institution, Glasgow, was born in London in 1817. He was brought up as a professional chemist, having early shown a predilection for that branch of science. He studied under Mr Hennel of London; and it has been stated that he was present when his instructor was killed, while conducting some experiments, by an explosion of fulminating powder. Dr Penney recommended himself very early by important experiments and communications on chemical subjects; and in 1839, while only twenty-two years of age, when the Chair of Chemistry, which he ultimately held, became vacant, he was recommended for the office by the late Professor Graham, and unanimously appointed by the patrons. Dr Penney was a man of great talent, quickness, and intelligence, and an excellent chemist, both theoretical and practical. As a chemical analyst, he enjoyed a high reputation for his fidelity and accuracy, and, I should suppose, must have derived a considerable income from that source. In one department, that of a scientific witness, I can bear personal testimony to his ability and excellence. His evidence in the witness-box was always clear, ready, explicit, and consistent; and he had one qualification essential to every good scientific witness, but which is certainly not possessed by all who place themselves in that position,—he underwent the operation of cross-examination with perfect composure and good temper, and showed himself as ready to speak to any fact that seemed to bear against the side adducing him as he had been to give evidence in its favour. This demeanour, which every scientific witness should at least assume, made his testimony very influential and valuable. In his private relations, Dr Penney appears to have

been an amiable and agreeable man, with strong feelings of affection to his friends, and much kindly consideration for the feelings of others. He was well informed and highly accomplished. He was fond of travelling when he could command a holiday, and his skill as an amateur artist enabled him the better to enjoy and perpetuate the beauties of the scenery which he visited.

His frame was never robust, and for some time past he suffered from a complication of ailments, which terminated his life on the 2d November 1869, at the age of fifty-two.

His funeral was attended by many scientific friends and respectable citizens of Glasgow, as well as by the chief office-bearers of Anderson's Institution, and the students of that seminary joined the procession and proceeded with it to the burying-ground.

Dr William Seller, an eminent member of the medical profession, and long an esteemed Fellow of this Society, was born in Peterhead, Aberdeenshire, in 1798, the son of a respectable merchant, who died while his family were children, leaving them under the charge of a widow, who was herself still young, and who found that, in consequence of losses arising from misplaced confidence in others, she must depend on her own exertions for the family's support. She came to Edinburgh as a better field, both for earning a livelihood and educating her children, and here her son William had the advantage of the excellent education which the High School and the University afforded. He was distinguished at both of these seminaries, and latterly was enabled to assist his mother by his creditable exertions in private tuition.

He became at the University a member of the Dialectic Society, where he formed many pleasing and permanent friendships with several of his contemporaries, including, among others, Lord Deas, Dr Aitken, for many years the Minister of Minto, and Dr Cumming, Government Inspector of Free Church schools. With these gentlemen he maintained a life-long friendship, as well as with many of those whom he had attended as private tutor, and who had learned to respect his learning and his virtues. Ultimately he made choice of medicine as his profession, and took the degree of M.D. in August 1821.

Prudential considerations led him soon afterwards to make his

knowledge and abilities available in a form which generally brings to those who adopt it less honour than its usefulness and its intrinsic merit truly deserve. He opened a house for the reception of medical students as boarders during the College session, and instituted classes for preparing such students for their examination. It is not impossible that the department thus chosen by him formed some impediment to his success as a medical practitioner; but no one who knew Dr Seller, or watched his conduct, could fail to see, both in his choice and in the manner in which he followed it out, proofs of his manly independence, and of his earnest desire to promote medical science and maintain the dignity of his profession. His lectures and lessons, we believe, were admirably adapted for that purpose, delivered in the most kindly and conciliatory tone, and skilfully framed to lead his pupils by easy gradients to the most commanding views of medical knowledge. His general learning and accomplishments were at the same time suited in an eminent degree to illustrate and adorn medical studies. He was an excellent classical scholar; he was profoundly acquainted with the intellectual and moral sciences, for which he had early shown a strong predilection; and he was a proficient in those physical sciences which were most closely connected with his own professional subjects. The extent and accuracy of his information were only equalled by his readiness in communicating it and his modest estimate of his own acquirements.

His last book, which he published in conjunction with Mr Henry Stephens, on "Physiology at the Farm," will illustrate at once, to those who are capable of appreciating it, the extent and variety of his scientific knowledge, and some defects at the same time which attended his mode of conveying instruction in this form.

In that volume there is a marvellous exposition of all the most important facts and principles connected with the subject of animal growth and nutrition, particularly as applicable to the rearing and feeding of stock; and the ground there travelled over in physiology, anatomy, chemistry, and botany is so extensive, that no one who was not thoroughly master of all these subjects could do them the justice which has there been dealt to them. The only fault in his dissertations is that they are too profound, and that it may be necessary to find an interpreter to stand between the man of science

and the practical farmer. From this fountain, however, all instructors desirous of communicating to those concerned a familiar and available view of the truth on these subjects will be able to draw the most important and reliable materials. In the preparation of this book, Mr Stephens, in a pleasing letter addressed to me, bears testimony to the assiduity, readiness, and disinterested zeal of Dr Seller, who declined all remuneration for his labours, though offered from a high quarter, and was with difficulty persuaded to let his own name stand first on the title-page before that of his excellent associate, who in the scientific department of the book felt how great a claim Dr Seller had to the commendations due to the work.

I am not personally acquainted with his other productions, and should be ill qualified to form an estimate of their worth; but a full account of these will be found in the notice of Dr Seller contained in the "Edinburgh Medical Journal" for May 1869. That memoir is, I believe, from the pen of Dr Alexander Wood, who was on the most intimate terms with him, and who has shown his ability both to appreciate and to record the talents and virtues of his friend.

Mention is there made of the great merit of the lectures on Mental Diseases which he annually delivered, under the Morrison Endowment, in the College of Physicians. "We have called them wonderful," Dr Wood says; "they were truly so, whether we have respect to the learning they displayed, to the acuteness and originality of the views which they enforced, or to the power of mental analysis which they exhibited. But," he adds, "if ever published, they will require some gifted and loving hand to popularise the style, and let the whole matter down to the comprehension of the busy workers of our every-day world."

The same memoir contains a full account of the professional honours which he attained. Among the most distinguished of these was his appointment as President of the Royal College of Physicians from 1848 to 1850. He was also the librarian of that College and a councillor for twenty years. A few years ago they did him the honour to request him to sit for his portrait, to be hung in the new hall, and the picture thus painted was among the last works of the late Sir John Watson Gordon. Dr Wood thus speaks of his personal character with equal kindness and truth:—

"His moral qualities reached almost higher than his intellectual, and were the secret of the influence he possessed, and of the affection with which he was regarded. His courtesy of manner and delicacy of feeling marked him as a true gentleman in all that he did. In him sterling integrity, firmness of principle, unswerving rectitude, and thorough persuasion in his own mind, were combined with a breadth of view, and a tolerance for the opinions, ay, even for the weaknesses, of others, as pleasing as it is rare. Guileless as a child, he was yet sagacious beyond most men; while the delicate susceptibilities of his kind heart prevented him from saying or doing anything that could by possibility wound the feelings of another."

In society Dr Seller's manners were most genial and agreeable, and he had the power of attaching to himself all who made his acquaintance. Mr Stephens, his "collaborateur" in the "Physiology of the Farm," and who came to know him only through their union in that work, writes to me of him—"I never made so dear a friend on so short a notice."

Until about the year 1865 Dr Seller enjoyed a fair amount of good health, and retained his active habits; but shortly after that time his constitution gave way; and when, after some interval, he sought medical advice, a complication of disorders was discovered to exist, including disease of the heart.

Under the care of Mr Archibald W. Dickson, assisted by other eminent medical friends, the worst symptoms were kept in check for a time, but at last resisted the remedies applied to them, and made it apparent that his end was approaching. He bore the sufferings incident to his illness with the fortitude of a philosopher and the resignation of a Christian. He discussed with his medical attendants every symptom of his malady, and its probable termination, with the same calmness as if the patient had been a stranger. He retained his courtesy and kindness to all around him to the very last. His death occurred on the 11th April 1869, at the age of seventy-one. The great respect with which he was regarded was shown by the number of those who, unbidden, were present at his funeral. The College of Physicians, who had long considered him an honour to their body, attended in their official robes, preceding the coffin to the grave, and surrounding it while the last rites were

performed. It will be long before we see supplied the place of one who had so many high attainments and so amiable a character—so many solid and so many agreeable qualities.

James Wardrop, one of our oldest members, and long known as a very eminent surgeon, was born, in August 1782, at Torbanehill, a small property which had been long in his family, and which has since earned a marked reputation in a mineral and chemical as well as a forensic point of view. He commenced the study of medicine under the care of his uncle, Dr Andrew Wardrop, an eminent surgeon in Edinburgh. He became assistant to Dr Barclay, the celebrated anatomist, and was for some time house-surgeon in the Royal Infirmary here. He afterwards went to London, to prosecute his studies in the lecture-rooms and hospitals of the metropolis; and afterwards passed over to Paris, though by this time the peace of Amiens had been broken off, and war had recommenced between France and England. Had he been known as an Englishman, he would have been detained as a prisoner; but he contrived to elude the vigilance of the police whilst he remained in Paris, and ultimately succeeded in effecting his transit from France into Germany. He attended various lectures at Vienna, and had there his attention specially directed to the diseases of the eye, for the treatment of which he afterwards attained so high a reputation. On returning to Edinburgh, he commenced the practice of his profession, and very soon selected surgery as his department. After practising here for four or five years, Mr Wardrop left Edinburgh, and settled in London as a surgeon. Instead of attending, however, the public hospitals there established, he preferred to institute a surgical hospital of his own, the wards of which were thrown open to the profession gratuitously, and where he had a weekly concourse of visitors, when medical topics were made the subject of conversation. This hospital he continued to superintend for about eight years, when he found the labour that it involved was more than he could undertake consistently with his other avocations. In this manner, and from surgical lectures which he delivered in London, Mr Wardrop's reputation became well established. In 1818, he was appointed Surgeon Extraordinary to the Prince Regent; and when the Prince, after his accession to the throne, visited Scotland, Mr

Wardrop attended him. He is understood to have been a great favourite with the king; but, towards the last days of that monarch, a misunderstanding at Court arose which excluded Mr Wardrop from attendance, in consequence, it was thought, of his having complied too frankly with the king's urgent inquiry as to the nature and probable termination of his disease. There can be no doubt that Wardrop was right in the opinion he formed, though whether the communication he made was consistent with the rules of courtly etiquette is not easy to determine. It is, however, believed that, from some of those who had been instrumental in excluding him from the royal death-bed, Mr Wardrop ultimately received an ample apology. Mr Wardrop, though an excellent surgeon in all respects, soon showed a special familiarity with ophthalmic surgery, and attained the highest reputation in that department, both by his writings and his practice. In 1813, Mr Wardrop published the well-known case of James Mitchell, the boy born blind and deaf, who, I believe, only died in the present year. The case excited a great deal of interest both among metaphysicians and physiologists. Mr Wardrop's account of it is extremely interesting and curious. He had partially succeeded in admitting light to the boy's eye by operating for cataract, and the sight was thereby improved, so as to afford the patient the delight that colours could convey, and which he keenly enjoyed, though his vision still remained too imperfect to become a source by which practical information of external objects could be introduced. Mr Wardrop was a man of very varied tastes and talents. He had a great love and appreciation of art. He was very fond of horses, and frequented the hunting-field till a comparatively late age; and it was with great satisfaction that he wrote his essay on the diseases of the eye of that animal, which obtained a prize from the Board of Agriculture. It has been said that he operated with success on several valuable race-horses and hunters by couching them for cataract, to the great gratification of their owners; but whether the animals when so treated required a pair of spectacles or an artificial lens to supply the place of the extirpated humour, I am unable to tell.

I shall not here attempt any account of Mr Wardrop's works, which must be well known to medical men, who are most likely to

feel an interest in the subject. An enumeration of them is given in Pettigrew's "Medical Portrait Gallery," where also the incidents of his life are fully narrated. I believe that he enjoyed a peaceful and cheerful old age, and attained his eighty-seventh year, without much suffering. I have heard that he latterly discontinued the use of wine, and attributed to that circumstance mainly his continued enjoyment of health. He had always been a temperate man, his favourite beverage being tea. Not very long before his death he had the misfortune to lose his wife, who also attained a great age, and latterly his eyesight failed him completely. This he felt as a great privation, but he bore it with patience, and never murmured. He sank into a state of great weakness, which gradually led to his death without any struggle. He was much loved and respected by all who knew him, and his reputation as a good man and as an excellent surgeon, and especially as a distinguished and scientific oculist, ought not soon to be forgotten in his profession.

It is said that he has left behind him a manuscript record of his recollections, which, if published, would in all probability, coming from a man of his ability, observation, humour, and experience, be highly interesting, not only to the profession, but to the public.

The following statement respecting the Members of the Society was read by the Chairman :—

I. Honorary Fellows—		
Royal Personage,	1	
British subjects,	19	
Foreign „	33	
Total Honorary Fellows,		53
II. Non-Resident Member under the Old Laws,		1
III. Ordinary Fellows :—		
Ordinary Fellows at November 1868,		289
New Fellows, 1868–69.—Robert Henry Bow, Alexander Buchan, Rev. H. Calderwood, James Dewar, Professor A. Dickson, William Dickson, George Elder, Principal Sir Alexander Grant, Bart., Sir Charles Hartley, Isaac Anderson-Henry, Alexander Howe, Professor Fleeming Jenkin,		
Carry forward,		289

	Brought forward,	289
Dr John W. Johnston, Maurice Lothian, David MacGibbon, Dr R. Craig Maclagan, Dr W. C. M'Intosh, John Maclaren, Dr Henry Marshall, O. G. Miller, John Pender, Rev. T. M. Raven, Dr W. Rutherford, J. L. Douglas Stewart, Viscount Walden, Capt. T. P. White,		26
		315
Deduct Deceased.—Dr Begbie, William Brand, Dr Dalzell, Professor Dyce, Principal Forbes, Rev. Dr Muir, Dr Penney, Dr Seller, James Wardrop,	9	
James Anstruther (formerly noticed),	1	
Resigned.—Dr A. E. Mackay, Bishop Morell,	2	
		12
Total Number of Ordinary Fellows at November 1869,		303
Add Honorary and Non-Resident Fellows,		54
	Total,	357

Monday, 20th December 1869.

Professor KELLAND, Vice-President, in the Chair.

The Keith Prize for the Biennial Period ending May 1869, having been awarded by the Council to Professor P. G. Tait, for his paper "on the Rotation of a Rigid Body about a fixed point," which has been published in the Transactions, the Medal was delivered to him by the President at the commencement of the Meeting.

The following Communications were read:—

1. On the Geological Structure of some Alpine Lake-Basins. By Archibald Geikie, Esq., F.R.S.

In this paper the author reviewed the arguments by which the geologists of Switzerland endeavour to prove that the so-called "orographic" lakes are essential parts of the architecture of the Alps. He showed from detailed sections of one or two lakes, particularly of the Lake of the Four Cantons, that the amount of denudation, which the surrounding rocks had suffered, demonstrated that

the lakes must be greatly younger than the plication of the strata of the Alpine chain; that from the known effects of subaerial denudatoni, the lakes must be, in a geological sense, quite modern; and that the Alpine lakes possessed no distinctive features which entitled them to be considered apart from the numerous lakes which are scattered over northern Europe and America. He regarded the enormous development of lakes at the present period in northern latitudes as a fact which could not be explained by reference to subterranean movements. Such movements must have taken place in a late geological period, otherwise the lakes would have been filled up with sediment, as is going on every day. He could not but think that the formation of such lake-basins was connected in some way with the action of the denuding forces, and he believed that the theory proposed by Professor Ramsay—that the rock-basins had been hollowed out by the ice of the glacial period—fulfilled all the geological conditions of the problem, and would eventually come to be accepted even by the geologists of Switzerland.

2. Preliminary Notice of the Great Fin Whale, recently stranded at Longniddry. By Professor Turner.

This communication was preliminary to a more extended memoir which the author hopes to lay before the Society during the Session.

The colour, general form, and dimensions of the animal, were taken when the whale was lying on the shore at Longniddry. The observations on its internal structure were made whilst it was undergoing the operation of flensing at Kirkcaldy, or on specimens which were brought over to the Anatomical Museum of the University. These specimens it was his intention to preserve in the Museum. In conducting the examination he had been ably and willingly seconded by the thoroughly cordial and enthusiastic co-operation of his assistant Mr Stirling, and his pupils Mr Millen Coughtrey, and Mr James Foulis.

Most of the Fin Whales which had been subjected to examination by British and Continental anatomists had been found floating dead on the surface of the sea, and had then been towed ashore; but the Longniddry whale had got entangled, whilst living, amongst

the rocks and shoals, where it was left as the tide receded. The length of the animal, measured from the tip of the lower jaw to the end of the tail, 78 feet 9 inches. The girth of the body immediately behind the flipper was 45 feet. Its girth in line with the anal orifice was 28 feet, whilst around the root of the tail it was only 7 feet 6 inches. The inner surface of the lower jaw, close to its upper and outer border, was concave, and sloped inwards so as to admit the edge of the upper jaw within it. The lower jaw projected at the tip 1½ foot beyond the upper. The length from the angle of the mouth to the tip of the lower jaw, along the upper curved border, was 21 feet 8 inches. The dorsum of the upper jaw was not arched in the antero-posterior direction. It sloped gently upwards and backwards to the blow holes, from which a low but readily recognised median ridge passed forwards on the beak, gradually subsiding some distance behind its tip. On each side of this ridge was a shallow concavity. Immediately in front of the blow holes the ridge bifurcated, and the forks passed backwards, enclosing the nostrils, and then subsided. The outer borders of the upper jaw were not straight, but extended forward from the angle of the mouth for some distance in a gentle curve, and then rapidly converging in front, formed a somewhat pointed tip. Their rounded palatal edge fitted within the arch of the lower jaw. The transverse diameter of the upper jaw over its dorsum, between the angles of the mouth, was 13 feet 3 inches. From the blow holes the outline of the back, curved upwards and backwards, was uniformly smooth and rounded, and for a considerable distance presented no dorsal mesial ridge. From the tip of the lower jaw to the anterior border of the dorsal fin the measurement was 59 feet 3 inches. This fin had a falcate posterior border. Behind the dorsal fin the sides of the animal sloped rapidly downwards to the ventral surface, so that the dorsal and ventral mesial lines were clearly marked, and the sides tapered off to the tail. The ventral surface of the throat, and the sides and ventral surface of the chest and belly, were marked by numerous longitudinal ridges and furrows. When he first saw the animal, the furrows separating the ridges were not more than from ¼ to ¾ inch broad, whilst the ridges themselves were in many places 4 inches in breadth, but as the body began to swell by the formation of gas

from decomposition, the furrows were opened up, became wider and shallower, and the ridges underwent a corresponding diminution in breadth. At the same time a considerable change took place in the contour of the body in the thoracic and abdominal regions, which presented a huge lateral bulging, giving a greater girth than when it first came ashore.

The flipper, which measured 12 feet 3 inches from root to tip along its anterior convex border, projected from the side of the body 31 feet 4 inches behind the tip of the lower jaw, and 14 feet behind the angle of the mouth. It curved outwards and backwards, terminating in a free pointed end. The distance between the two flippers, measured over the back between the anterior borders of their roots, was 18 feet 6 inches.

On the dorsum of the beak and of the cranium, on the back of the body, and for some distance down its sides, the colour was dark steel grey, amounting in some lights almost to black. On a line with the pectoral flipper the sides were mottled with white, and on the ventral surface irregular, and in some cases large patches of a silvery grey or whitish colour were seen. An experienced whaling seaman, Mr Walter Roddam, who had repeatedly seen this kind of whale in the northern seas, told him that it was known to the whalers by the name of "silver bottom." The dorsal fin was steel grey or black, except near its posterior border, where it was a shade lighter and streaked with black lines. The anterior margin of the lobes of the tail, its upper surface near the root and for the anterior two-thirds, were black, whilst the posterior third of the same surface and the interlobular notch were lighter in tint. The upper surface of the flipper was steel grey, mottled with white at the root, at the tip, along its posterior or internal border, and on the under surface; white patches were seen on the upper surface near the tip, and here they were streaked with black lines running in the long axis of the flipper. White patches also extended from the root of the flipper to the adjacent parts of the sides of the animal. The outside of the lower jaw was black, whilst the inside was streaked with grey. The tongue of the whale was of enormous size. The dorsum was comparatively smooth in front, but at the posterior part it was elevated into hillocks which were separated by deep furrows. The baleen had a deep black colour,

and consisted on each side of plates which projected from the palate into the cavity of the mouth. The plates were arranged in rows — 370 were counted on each side — which lay somewhat obliquely across the palate, extending from near the base of the great mesial palatal ridge to the outer edge of the palate. The plates diminished in size so much, that at the tip, where the two sets of baleen became continuous, they were merely stiff bristles. The blubber varied much in thickness. Mr Tait, by whom the whale was purchased, and to whom the author was indebted for the opportunity of examining the animal during the flensing operation, stated that he had obtained from the blubber, and from the inside fat, 19 tons 12 cwt. of oil; whilst the skeleton, including the lower jaw, weighed 9 tons 12 cwt., and the baleen, including the gum, about one ton; the weight of flesh, intestines, and other refuse, was estimated at about 50 tons.

The author believed the whale to be an example of the whale called Steypireyðr by the Icelanders, a description of which by Professor Reinhardt has recently appeared in the Annals of Natural History (Nov. 1868). The Steypireyðr has been identified with the *Balænoptera Sibbaldii* or *Physalus Sibbaldii* of Gray. The Longniddry whale differed from the *Balænoptera musculus* (*Physalus antiquorum*, Gray), or common Razor-back, in having a broader and more rounded beak, in the flipper being longer in proportion to the length of the body, in the baleen plates, fringes, and palatal mucous membrane, being deep black, in the plates being longer and broader, in the belly possessing a more silvery grey colour, and in the blubber being thicker, so that the animal is commercially more valuable.

The whale was with calf, but the fœtus, a male, had been displaced, and thrown out of the abdominal cavity into a space between the outer surface of the right ribs and the blubber. The displacement had probably occurred whilst she was being towed by the tail across the firth from Longniddry to Kirkcaldy. The whale may have entered the firth in order to give birth to her calf, as there seems reason to think that whales do frequent arms of the sea for that purpose. Although nothing definite seemed to be known of the period of gestation of the Fin whales, yet, from the length of the calf—amounting to nearly 20 feet, or about one fourth the length of the mother—he thought it was probable that

the whale was at or about her full time. Several square feet of the fœtal membranes were examined. The outer surface of the chorion was thickly studded with villi, which over large areas had no special mode of arrangement; but in some localities they formed an irregular network, in others they were seated on long ridge-like elevations of the chorion, and in other cases conical folds of that membrane, 5 or 6 inches long, were closely covered with villi. The placenta was diffused, but with a tendency to aggregation of the villi where the chorion was raised into ridge-like and conical folds.

The paper contained an account of the vessels, the pharynx, laryngeal pouch, the omentum, the intervertebral discs, the cylindriform fibrous mass which supports the lower jaw, and a description of the atlas, axis, hyoid bone, sternum and pelvis. The sternum was shown to be not a rudimentary bone, but of considerable size, consisting of three large lobes with a posterior pointed process. The dissection of the fœtus proved that the opinion entertained by anatomists, that in the baleen whales the sternum is a single bone developed from one ossific centre, is not correct for all the species. For in this *Balænoptera* the fœtal sternum consisted of two distinct masses of cartilage, one of which corresponded to the posterior pointed process, the other to the larger 3-lobed anterior portion. The pelvic bones were also described. In the fœtus they were still cartilaginous, but had the same general form as in the adult, which proved that in the process of ossification no important change took place in their external configuration, and that the pelvis of the male differs in no essential feature from that of the female. From the appearance presented by the skeleton generally, the large whale was obviously in the stage of growth which Mr Flower has termed "adolescent."

The paper was illustrated by photographs, drawings, and specimens.

3. Note on Aggregation in the Dublin Lying-in Hospital. By Dr Matthews Duncan.

In this paper it is pointed out that deliveries are a better means of arriving at an estimate of the healthiness of an hospital than amputations; that the deliveries in the Dublin Hospital are remarkably valuable because of their great number (nearly 200,000),

and of the length of time of the hospital's operation (above 100 years); and that the evidence derivable from them relative to the danger of confinement, as regulated by the amount of aggregation, or number brought together at the same time, has never been properly taken.

It has been asserted by Dr Evory Kennedy and others, that the mortality is in direct proportion to the aggregation. But an analysis of the whole data indisputably shows that in the Dublin Hospital the mortality does not increase with the increased number of the inmates, and does not rise with the aggregation. The mortality of this hospital is neither in the direct nor in the inverse ratio of the aggregation.

The data, indeed, seem to favour the view that the mortality diminishes when the aggregation is increased. Certainly a smaller proportional number die when there were many in the hospital than when there were fewer.

The following Gentlemen were elected Fellows of the Society:—

St John Vincent Day, Esq., C.E.
David Munn, Esq.
Robert R. Tatlock, Esq.

Monday, 3d January, 1870.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. On a Method of Economising our Currency. By Andrew Coventry, Esq.

In the outset, it was stated that the currency consisted mainly of a large mass of paper, whose convertibility had been provided for by Sir Robert Peel's Bank Bill of 1844–45, with which paper, and the gold set aside for it, the author did not propose to meddle. But alongside of the paper there circulated a large quantity of gold, and the object of his paper was to economise it. Now, gold having only three uses—as currency, in the arts, and to discharge debts abroad—it was desirable that some arrangement should be thought

of which might relieve it of the first mentioned service, in which it suffers much waste, and set it free for the two others.

The plan proposed was to disqualify gold, under legal penalties, for currency or barter within the island, upon which it would flow into the Bank, to be kept there for the security of the notes which would take its place, and for the arts and foreign trade. The gold currency being shown to amount to 80 millions, it was next explained that, agreeably to an article in the "Economist" of 3d July last, the saving thereby effected (in tear and wear, coining and recoining) to the country would be fully L.56,000 a year, or rather L.60,000 a year, as L.4000 might be added for loss by fire and shipwreck. As to the expense, again, of the paper which would be needed to represent the 80 millions of gold brought in by the disqualification, the author proposed to provide for it in the following way:—Let the Bank have to itself two of the 80 millions of gold, and yet be allowed to issue paper to the full amount of 80. The uncovered part of the issue would be a slight extension of the 14 or 15 millions already privileged by statute, and such an extension has been often proposed, and by able men. In return for the two millions of gold, the Bank might very fairly be expected to provide the paper currency and pay the State L.25,700 a year. These figures are arrived at by the terms of the arrangement between the Bank and Government as to the 14 millions being adopted for the two millions now. Farther, a return to the use of small notes in England was recommended, as the experience of Scotland showed that certain improvements in engraving were complete preventives against forgery; and he advocated also gold bars, a suggestion of the late Mr Ricardo, instead of coins.

The result of gain on the whole would be, to the State L.60,000 and L.25,700, besides L.18,000 of profit to the Bank *after* defraying the paper currency—or, in all, L.103,700 a year, which, capitalised, would be three millions.

Such was Mr Coventry's proposal. But he added that some might reasonably be inclined to go further, and to take the whole or part of the remaining eight of the 78 millions, making some compensation to the Bank, of course, seeing that a reserve of 78 of gold against 80 of paper, large at any time, would be extravagant when gold fell to be disused for currency. Even if we were to

assume the cost of 80 millions of paper to be not far short of the cost of maintaining a gold currency of like amount, the scheme proposed would have this merit, that it would bring 80 millions of gold into the bank, of which 70 millions would be an ample reserve against 80 of paper—*thus effecting a gain of Ten Millions.* Mr Coventry showed, too, that bullion was seldom required to be sent abroad to any very great amount by the exchanges, and instanced the year 1864, when the trade of the country amounted to nearly 500 millions, and the balance only to 4½ millions, or a trifle more.

2. On the old River Terraces of the Earn and Teith, viewed in connection with certain Geological Arguments for the Antiquity of Man. By the Rev. Thomas Brown, Edinburgh.

The author described the circumstances which led him, in 1863, to begin the investigation of these terraces, and showed he had traced their course along the Earn from Loch Earn to where they meet the tide. He had also examined the valley of the Teith, and had found the same deposits from the head of Loch Lubnaig to near Stirling. There are three different levels on which the terraces lie at different heights above the river bed. The lowest consists of the present banks of the stream and haughs or meadows; above this there is an intermediate terrace, which, in its turn, is surmounted by the highest. Owing to the effects of denudation, one or other of these levels is frequently interrupted or obstructed, but they are ever again found recurring, and the whole three present themselves so frequently as to show that this threefold terrace system is the true key to these valley deposits. It was shown that they were neither sea-beaches, as some geologists have held, nor lake-margins, as has been maintained by others, but must have been formed by the river itself, at some former age, when its floods had the power of rising to the requisite height. All the three terraces are found varying in height at different points according to the width of the valley, the strength of the current, and other circumstances. The lowest, which consists of the present banks, &c., varies from 3 to 10 feet, according to the locality; the second, from 15 to 24; while the third is from 35 to 60, or

even more above the river bed. Numerous examples were given of their outward form and inward structure to illustrate these views.

The author next proceeded to describe the exact geological position of these deposits. As the time of the kames or escars belonged to the close of the glacial epoch, so the formation of these terraces followed the time of the kames, and they were constructed by river floods out of the pre-existing collections of gravel, &c. The fossil remains of the flora of Strathearn, which they enclose, show that the climate of the period must have been as mild as the present.

Certain geological arguments for the antiquity of man were referred to, especially these deduced from the gravel deposits of the Somme in France and the Brixham cave in England. From the height at which these deposits with flint weapons had been found above the present river courses, it had been held that the human period must be extended so as to leave time for the erosion of the valleys. The author adduced evidence to show conclusively that the Scottish valleys had been eroded down to their present depth previously to the formation of these old gravel deposits, which are found at so great a height above the rivers. If, therefore, the analogy of the Scottish valleys and streams could apply to those of France and England, the time needed for the erosion of the valleys must be thrown out of the account. It was vain to attempt to dissociate the formation of the valley system of France and England from that of Scotland, as if they were not analogous. He had no doubt that these views would be established; but, in the meantime, it was at least right that men should suspend their judgment till the question thus raised had been thoroughly investigated.

The following Gentlemen were elected Fellows of the Society:—

Alexander Russel, Esq.
James Crichton Browne. M.D.
John Duncan, M.D., F.R.C.S.E.
W. Burns Thomson, F.R.C.S.E.
Dr W. R. Sanders, Professor of Pathology.
Rev. Andrew Thomson, D.D.
Joseph Lister, Professor of Clinical Surgery.
William Anderson, LL.D.

Monday, 17th January 1870.

GEORGE ROBERTSON, Esq., Councillor, in the Chair.

The following Communications were read:—

1. Experiments on the Colorific Properties of Lichens. By W. Lauder Lindsay, M.D., F.R.S.E., F.L.S.

The author's paper consists mainly of a *Table* exhibiting certain of the positive results of many hundred experiments on the colouring matters contained in or educible from Lichens. The experiments in question are partly a repetition, and partly an extension on a more systematic and complete scale, of a series of researches made by the author between 1852 and 1855, the results of which were originally submitted to the Botanical Society of Edinburgh. The present series of experiments includes the whole family of the Lichens. The Table represents mainly the effects of chemical reagents on solutions of the lichen colouring-matters, or colorific principles, in boiling alcohol or water. The nomenclature of the Colour-reactions is that of Werner and Syme. As the subjects of his experiments, the author confined himself in great measure to the lichens contained in published Fasciculi; so that comparative experiments may hereafter be made on authentic specimens of the *same species* and varieties by other observers in other countries. The author's results are submitted as a mere pioneer contribution to a subject, which has been as yet most imperfectly worked out, viz., the Chemistry of the lichen colouring-matters; but he trusts they may furnish a partial basis for a future more exhaustive series of researches to be undertaken *conjointly by Chemists and Lichenologists.*

The present Table illustrates *pro tanto*—

I. The kinds of colour producible from lichens: those, viz.—

(*a*) Which exist ready formed in the thallus—for the most part green, yellow, or brown,—and which are of little practical utility; and

(*b*) The colourless colorific principles, which, under the action of ammonia and atmospheric oxygen, yield red or purple

dyes of the class of which Orchil, Cudbear, and Litmus are the familiar types.

II. The families, genera, or species that possess practical colorific value; as well as the relative values of colorific species or varieties.

III. The irregularities or uncertainties of colour-development, according to

(*a*) The condition of the lichen operated on;

(*b*) The condition of the reagent; or

(*c*) The circumstances of experiment.

There is thus a rough indication, on the one hand, of the so-called *Dye-lichens;* and, on the other, of species and genera that are practically useless to the colour-maker.

The present series of experiments, moreover, has a direct practical bearing on

I. The recent introduction of *Colour-tests as Specific Characters* in Lichens;

II. The modern manufacture from Lichens (*e.g.*, in France) of *fast dyes*, capable of competing successfully with the brilliant coal-tar colours and other dyes of recent introduction; and

III. The use, which still lingers in certain parts of Scotland, and probably also of Wales and Ireland, of lichens as *Domestic dye-stuffs*.

2. On the Principles of Scientific Interpretation in Myths, with Special Reference to Greek Mythology. By Professor Blackie.

Professor Blackie commenced by saying that, of all the branches of interesting and curious learning, there was none which had been so systematically neglected in this country by English scholars as mythology—a subject closely connected both with theology and philosophy, and on which those grand intellectual pioneers and architects, the Germans, had expended a vast amount of profitable and unprofitable labour. The consequence of this neglect was, that of the few British books we had on the subject, the most noticeable were not free from the dear seduction of favourite ideas which possessed the minds of the writers as by a juggling witch-

craft, and prevented them from looking on a rich and various subject with that many-sided sympathy and catholic receptiveness which it required. In fact, some of our most recent writers on this subject have not advanced a single step, in respect of scientific method, beyond Jacob Bryant, unquestionably the most learned and original speculator on mythology of the last century; but whose great work, nevertheless, can only be compared to a grand chase in the dark, with a few bright flashes of discovery, and happy gleams of suggestion by the way. For these reasons, and to make a necessary protest against certain ingenious aberrations of Max Müller, Gladstone, Inman, and Cox in the method of mythological interpretation, he had undertaken to read the present paper; which, if it possessed only the negative virtue of warning people to be sober-minded and cautious when entering on a path of inquiry, full of bogs below and clouds above, could not be deemed impertinent at the present moment.

One great fact as to the origin of Polytheism may be considered as firmly established, and by general consent admitted—viz., that the great physical shows and forces by which man finds himself surrounded and conditioned, assuming, under the influence of reverence and imagination, various anthropomorphic disguises, constituted the original council of the great gods. When we say physical, however, we do not mean physical in the material and mechanical modern sense of the word; but we mean physical in a sort of pantheistic sense, in which nature is regarded as everywhere interpenetrated, inspired, and fashioned by spirit. This being so and ascertained, be it noted, by an overwhelming array of strictly inductive evidence, there can be no difficulty in predicating, *à priori*, what the great gods of the Greeks, to whom I shall confine myself in this paper, must have been originally in their elemental significance. They must have been those powers of Nature and of the human soul, or of Nature considered as animated by a human soul, whose display was most striking, and whose influence was most felt by primeval man. Those powers are—The sky, the earth, the sun, the moon, the stars, the sea and rivers, the atmosphere and winds, the subterranean forces, the underground world, and the unseen powers of darkness beyond the grave, the vegetative or generative principle, the fervid domain of moral emotions,

and the sovereign sway of intellect. For I do not believe in any period when man was merely a brute, or a nondescript creature, half emergent from the primeval man-monkey or monkey-man. Individual tribes of a low type, such as those whom my ingenious, acute, and learned friend, Mr M'Lennan, calls by the undignified name of *Totems*, may always have existed; but in a general Totem-state of an embryo and embruted humanity I do not believe. Hypotheses of this kind are the conceit of speculative scicence, not historical fact. Starting from this base of operations, our first business is to look our gods fairly in the face, and by a reverential and poetic study of their forms, attitude, dress, badges, and symbols, to recreate the anthropomorphised power in its original elemental significance. And this must be done in an extremely cautious and careful way, so as to make legitimate our inductive conclusions, after the method of which such admirable examples are given by Ottfried Müller in his "Prolegomena"—a small book in respect of bulk, but a truly great book in respect of significance; and to the principles laid down in which it would be well if some of our recent mythological speculators would seriously recur. Mr Ruskin's method of interpreting the Greek gods without such a careful scholarly preparation, is mere brilliant trifling; and all excursions into the realms of comparative mythology and philology, after the fashion of Creuzer and Bryant, without first taking sober counsel from home materials, can result only in floating conjecture, not in stable knowledge. Now, to give an example of what I mean: if we take three of the principal gods of the Hellenic Olympus—Zeus, Poseidon, and Apollo—and peruse them carefully, I defy any man who has a common amount of classical reading, and who, like Wordsworth, can put himself into the position of the original creators of mythology, to form any other conclusion than that these personages are mere anthropomorphic disguises of the heavens, the ocean, and the sun; and towards forming this conclusion, with a man who is entitled to have a judgment on such subjects, not a single shred of Hebrew or Sanscrit, or any foreign organon of interpretation, is required. It may be interesting to know that Ζεύς in its Sanscrit form means *bright* or *shining;* but it is not necessary towards a well-grounded scientific induction of the original significance of the god.

But there are other persons in the Pantheon whose significance is anything but plain; and in their case, unquestionably, recourse may be had with advantage to etymology, first, in the native language, of course, and then in the kindred languages, in some one of which the original form of the sacred title may have been preserved. A striking example of the utility of native etymology in fixing the significance of the Greek mythological personages is presented in the familiar case of the Harpies, whose whole character and actions, taken along with the open evidence of their Greek names in Hesiod, prove, beyond all doubt, that they are the impersonated forms of such sudden gusts and squalls of wind as come down fuequently on the Black Sea or the Highland lochs. But etymology, though a safe guide in such instances, is, in less obvious cases, of all guides the most fallacious. And this is what my distinguished friend Max Müller, and some who follow in his train, seem at the present moment somewhat apt to forget. An etymology, though not caught up in the arbitrary fashion of Bryant and Inman, but traced with the most cautious application of Grimm's laws, is, after all, only a conjecture. It is a conjecture not in the teeth of all philological analogy. It implies a possible, or, as the case may be, a probable identity. But alone, and without extrinsic and real, as opposed to verbal indications, it affords no ground for a legitimate induction. Nothing is more common than accidental coincidences in mythological names—such as the Latin Hercules and the Greek Heracles—which, as scholars know, have not the most remote connection. Besides, even if the true etymology of any Greek god could be found in Sanscrit or any other language, the signification of the original name affords no sure clue to the character of the accomplished god. Our dictionaries are full of words whose ultimate signification has travelled so far away from its original, that the original meaning could supply no key to the modern usage. Πορφύρεος, for instance, means *dark* in Homer, but in Horace *brilliant* or *shining*. Usage alone can inform us of this perversion or inversion of the original meaning of words. But if this be true with regard to mere philology, it is much more true with regard to mythology. The root of a word, like the stock of a tree, may remain stiff enough for centuries; but the human imagination, when employed in the forming of myths, is a kaleido-

scope whose changes are incalculable, and whose results are so transmuted from the original type as to be unrecognisable. On these grounds, I feel myself bound to protest in the strongest manner against the fashion recently introduced by Max Müller and Mr Cox, of giving a new interpretation of Hellenic gods, founded on no firmer basis than slippery Sanscrit etymologies, and a few ingenious conjectures. After reading the distinguished German's lucubrations on Hermes, and Athena, and Erinnys, I stand as unconvinced as before the portentous array of Protean "Radicals," in the first volume of Bryant; it is only another turn of the mythological kaleidoscope from the hand of a man who combines the erudition, the speculation, and the subtlety of his people, with an eloquence and a taste seldom surpassed by the best Englishmen writing their own language in the best way—a man whose character I respect, and whose instructive intercourse I have enjoyed now for a long series of years; but, with regard to whose speculations on curious points of Greek mythology, I can only say, *Amicus Plato sed magis amica veritas.* And etymology is not the only point on which I am forced to dissent from Max Müller and that large school of German thinkers of whom he is the spokesman in this century. A long familiarity with the writings of German scholars has convinced me that there is a particular idiosyncrasy in their minds which, when applied without qualification in mythological research, is peculiarly apt to mislead. This idiosyncrasy leads them to believe in no facts that they are not able to construct from certain favourite presupposed ideas. Now, I believe in facts as having an independent value, and a right to be recognised altogether independent of any favourite ideas which an interpreter of facts may bring to explain them. I believe that one domain of myths is to be explained by ideas; but I believe also in a class of myths, of which the main root and stem are historical, and only the outer limbs and flourishes mythical. I see no presumption whatsoever that the Trojan War represents a conflict between the powers of light and darkness; that Achilles is a degraded solar god, as Müller would indicate, or a water god, as is the fashionable idea of most Germans. The most improbable thing in the world is that a nation should have drawn a brush over all its human memories, and left nothing but myths of the Dawn and the Dark

in the shape of European peers and Asiatic princes. I refuse, therefore, on the faith of a few specious etymologies, to see anything mythical in the main action of the "Iliad;" and I deem it a waste of brain to seek the interpretation of a stout old Thessalian thane, from a Sanscrit epithet of the sun. But India is not the only country to which adventurous scholars have travelled in search of a key to unlock the mysteries of the Hellenic Pantheon. Mr Gladstone, as it is well known, has reverted to the expedient—a favourite one with our old theological giants—of explaining Greek gods through the medium of a primitive sacred tradition. There might be no objection to this if the Hebrews had possessed any original quarry of theologic material from which an Apollo or an Athena could be built up; but the only idea that the Hebrews could have supplied to the Greeks was that of the one Supreme God, whom no doubt we have in Zeus, but unaccompanied with any special Hebrew character by which he might be identified. The same distinguished scholar's most recent excursion into far Eastern lands has not brought back, in my opinion, any more valuable booty. That Aphrodite and Hercules were of Phœnician extraction, at least contained a strong admixture of Phœnician elements, was known long ago; and few facts in early Hellenic history can be considered more certain; but beyond this, all propositions with regard to early Phœnician influence on the persons of the Greek Pantheon, seem to me to stand on too slight a basis of ingenious conjecture to possess any scientific value.

Having made these protests against the brilliant, but, so far as Greece is concerned, in my opinion barren excursions of recent writers into the regions of comparative mythology, I have only to say in conclusion, that the only safe method in the present state of the science of mythology, is to confine our attention to the actual forms and attitudes and symbols of the gods as they present themselves before us in their accomplished impersonation. By tracing Hermes, for instance, to the breeze of the early Dawn, nothing is gained, even it be true; it were only a pretty fancy of the infant Aryan mind on the banks of the Indus, with which a pastoral Greek on Mount Cyllene had nothing to do. The Hermes of the Greeks, is plainly, in the first place, a pastoral god of increase, then a god of gain, when the shepherd became a merchant, and

then generally a god of commerce, and the adroitness which commerce demands. Athena, in the same way, the daughter of the dark-clouded Jove, is the flashing-eyed maiden, because she represents the feminine aspect of the sky, of which her sire represents the masculine. Juno, again, by many manifest signs, is certainly the earth anthropomorphised out of the physical γῆ, just as Ζεύς was out of οὐρανός. Then, again, if Apollo be the sun, Artemis, his sister, without going further, must be the moon; and Dionysus, the wine god, whose Oriental origin and late introduction is certified, stands by virtue of the phallic symbol manifestly an Oriental god of the generative virtue, just as Hermes was in Arcadia by the same symbol proclaimed the patron of breeding to the sheep-farmers of the Pelasgic peninsula. Then, by the same process of looking at what is before me, apart from German theories and Sanscrit etymologies, I reserve a considerable domain in the mythological land for exaggerated and metamorphic history; not at all concerned that I may be looked on by the winged Germans as a dull, prosaic fellow, or a disciple of the atheistic Euhemerus—for Euhemerus also was not altogether wrong, and the worship of human ideals as, at least, one element in many mythologies, is one of the most accredited facts in the history of the human race. And if I seem to have achieved a very small thing when I keep myself within these bounds, I have at least kept myself clear of nonsense, which in mythological science is as common as sunk rocks in the Shetland seas. To Max Müller, and other Sanscrit scholars, I hope I shall always be grateful for any happy illustrations which they may supply of the general character of Aryan myths, and of occasional coincidences of the Hellenic mode of imagining with the Indian; and I think the somewhat cold and unimaginative race of English scholars are under no small obligations to him for having taught them to recognise poetical significance and religious value in some legends, which passed in their nomenclature for silly fables or worthless facts; but I profess to have been unable to derive any sure clue from the far East to the most difficult questions of Greek mythology; nor do I expect that, when every obsolete word in the Rig Veda shall have been thoroughly sifted and shaken, a single ray of intelligible light will thence flow on the Athena of the Parthenon or the Hermes of the Cyllenian slopes. I believe

that in the region of mythology they will ultimately be found to be the wisest, who are at present content to know the least; that while some mythological fables are too trifling to deserve interpretation, others are too tangled to admit of it; and that the man who, at the present day, shall attempt to interpret the Greek gods from the transliteration of Sanscrit or Hebrew words, will be found, like Ixion, to have embraced a cloud for a goddess, and to have fathered a magnificent lie from the fruitful womb of his own conceit. There is no more dangerous passion than that which an ingenious mind conceives for the fine fancies which it begets.

The following Gentlemen were admitted Fellows of the Society:—

Dr G. H. B. Macleod, Professor of Surgery in the University of Glasgow.
Dr Thomas A. G. Balfour, F.R.C.P.E.

The following Gentlemen were admitted Honorary Fellows of the Society:—

1. *Foreign.*

Hugo von Mohl, M.D., Ph.D., Member of the Imperial Academy Naturæ Curiosorum, and Professor of Botany in the University of Tubingen.
Claude Bernard, Member of the Institute of France, Professor of Physiology in the College of France.

2. *British.*

Thomas Andrews, M.D., F.R.S., M.R.I.A., Vice-President and Professor of Chemistry in Queen's College, Belfast.

ERRATUM.

Index, vol. vi. p. 608, Professor Tait's Paper, line 4 from bottom, second column, *for* Parts *read* Roots.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VII. 1869–70. No. 81.

EIGHTY-SEVENTH SESSION.

Monday, 7th February 1870.

DR CHRISTISON, President, in the Chair.

The following Communications were read:—

1. On Reciprocal Figures, Frames, and Diagrams of Forces. By J. Clerk Maxwell, Esq., F.R.SS. L. & E.

The reciprocal figures treated of in this paper are plane rectilinear figures, such that every line in one figure is perpendicular to the corresponding line in the other, and lines which meet in a point in one figure correspond to lines which form a closed polygon in the other.

By turning one of the figures round 90°, the corresponding lines become parallel, and are more easily recognised. The practical use of these figures depends on the proposition known as the "Polygon of Forces." If we suppose one of the reciprocal figures to represent a system of points acted on by tensions or pressures along the lines of the figure, then, if the forces which act along these lines are represented in magnitude, as they are in direction, by the corresponding lines of the other reciprocal figure, every point of the first figure will be in equilibrium. For the forces which act at that point are parallel and proportional to the sides of a polygon formed by the corresponding lines in the other figure.

In all cases, therefore, in which one of the figures represents a frame, or the skeleton of a structure which is in equilibrium under

the action of pressures and tensions in its several pieces, the other figure represents a system of forces which would keep the frame in equilibrium; and, if the known data are sufficient to determine these forces, the reciprocal figure may be drawn so as to represent, on a selected scale, the actual values of all these forces.

In this way a practical method of determining the tensions and pressures in structures has been developed. The "polygon of forces" has been long known. The application to polygonal frames, with a system of forces acting on the angles, and to several other cases, was made by Professor Rankine in his Applied Mechanics. Mr W. P. Taylor, a practical draughtsman, has independently worked out more extensive applications of the method. Starting from Professor Rankine's examples, I taught the method to the class of Applied Mechanics in King's College, London, and published a short account of it in the "Philosophical Magazine" for April 1864. Professor Fleeming Jenkin, in a paper recently presented to the Society, has fully explained the application of the method to the most important cases occurring in practice, and I believe that it has been found to have three important practical advantages. It is easily taught to any person who can use a ruler and scale. It is quite sufficiently accurate for all ordinary calculations, and is much more rapid than the trigonometrical method. When the figure is drawn the whole process remains visible, so that the accuracy of the drawing of any single line can be afterwards tested; and if any mistake has been made, the figure cannot be completed. Hence the verification of the process is much easier than that of a long series of arithmetical operations, including the use of trigonometric tables.

In the present paper I have endeavoured to develope the idea of reciprocal figures, to show its connection with the idea of reciprocal polars as given in pure mathematics, and to extend it to figures in three dimensions, and to cases in which the stresses, instead of being along certain lines only, are distributed continuously throughout the interior of a solid body. In making this extension of the theory of reciprocal figures, I have been led to see the connection of this theory with that of the very important function introduced into the theory of stress in two dimensions by Mr Airy, in his paper "On the Strains in the Interior of Beams" (Phil. Trans. 1863).

If a plane sheet is in equilibrium under the action of internal stress of any kind, then a quantity, which we shall call Airy's Function of Stress, can always be found, which has the following properties.

At each point of the sheet let a perpendicular be erected proportional to the function of stress at that point, so that the extremities of such perpendiculars lie in a certain surface, which we may call the surface of stress. In the case of a plane frame the surface of stress is a plane-faced polyhedron, of which the frame is the projection. On another plane, parallel to the sheet, let a perpendicular be erected of height unity, and from the extremity of this perpendicular let a line be drawn normal to the tangent plane at a point of the surface of stress, and meeting the plane at a certain point.

Thus, if points be taken in the plane sheet, corresponding points may be found by this process in the other plane, and if both points are supposed to move, two corresponding lines will be drawn, which have the following property:—The resultant of the whole stress exerted by the part of the sheet on the right hand side of the line on the left hand side, is represented in direction and magnitude by the line joining the extremities of the corresponding line in the other figure. In the case of a plane frame, the corresponding figure is the reciprocal diagram described above.

From this property the whole theory of the distribution of stress in equilibrium in two dimensions may be deduced.

In the most general case of three dimensions, we must use three such functions, and the method becomes cumbrous. I have, however, used these functions in forming equations of equilibrium of elastic solids, in which the stresses are considered as the quantities to be determined, instead of the displacements, as in the ordinary form.

These equations are especially useful in the cases in which we wish to determine the stresses in uniform beams. The distribution of stress in such cases is determined, as in all other cases, by the elastic yielding of the material; but if this yielding is small and the beam uniform, the stress at any point will be the same, whatever be the actual value of the elasticity of the substance.

Hence the coefficients of elasticity disappear from the ultimate values of the stresses.

In this way I have obtained values for the stresses in a beam

supported in a specified way, which differ only by small quantities from the values obtained by Mr Airy, by a method involving certain assumptions, which were introduced in order to avoid the consideration of elastic yielding.

2. On the Extension of Brouncker's Method. By Edward Sang, Esq.

The operation in use by the ancient geometers for finding the numerical expression for the ratio of two quantities, was to repeat each of them until some multiple of the one agreed with a multiple of the other; the numbers of the repetitions being inversely proportional to the magnitudes.

The modern process, introduced by Lord Brouncker, under the name of continued fractions, is to seek for that submultiple of the one which may be contained exactly in the other; the numbers being then directly proportional to the quantities compared.

On applying this method to the roots of quadratic equations, the integer parts of the denominators were found to recur in periods; and Lagrange showed that, while all irrational roots of quadratics give recurring chain-fractions, every recurring chain-fraction expresses the root of a quadratic; and hence it was argued that this phenomenon of recurrence is exhibited by quadratic equations alone.

The author of this paper had supplemented Lagrange's proposition, by showing that when the progression of fractions converging to one root of a quadratic is continued backwards, the convergence is toward the other root. The singularity of this exclusive property of quadratic equations led him to consider whether some analogous property may not be possessed by equations of higher degrees. Putting aside the idea of the chain-fraction as being merely accidental to the subject, and attending to the series of converging fractions, he came upon a kind of recurrence which extends to equations of all orders; and which proceeds by operating on two, three, or more contiguous terms according to the rank of the equation. In this way a ready means of approximating to the greatest and to the least root of any equation was obtained.

The following cases were cited:—

If we begin with the terms $\frac{1}{0}$, $\frac{1}{1}$, and form a progression by

adding the respective members of the preceding term to the doubles of those of the last, thus—

$$\frac{1}{0}, \frac{1}{1}, \frac{3}{2}, \frac{7}{5}, \frac{17}{12}, \frac{41}{29}, \frac{99}{70}, \frac{239}{169}, \text{\&c.}$$

we form the well-known series converging to the ratio of the diagonal of a square to the side.

Beginning with the terms 0, 1, if we add together the last two, thus—

0, 1, 1, 2, 3, 5, 8, 13, 21, 34, 55, 89, &c.,

each term bears to the succeeding one a ratio approaching to that of the side of a regular pentagon to the diagonal thereof.

If we assume the three terms 0, 0, 1, and continue the progression by adding to the double of the last term, the difference of the two preceding ones, thus—

0, 0, 1, 2, 5, 11, 25, 56, 126, 283, 636, 1429, &c.,

the ratio of each term to the following approaches to that of the side to the greater diagonal of a regular heptagon.

Or again, beginning with the same three terms, if we form a progression by deducting the antepenult from the triple of the last term, thus—

0, 0, 1, 3, 9, 26, 75, 216, 622, 1791, 5157, &c.,

we obtain an approximation to the ratio of the side to the longest diagonal of a regular enneagon.

From these examples it would appear that important results may be expected from the study of this branch of Logistics. Now, the roots of quadratics were reached by the comparison of two magnitudes, wherefore those of cubics may result from the comparison of three incommensurables; and analogously for equations of higher degrees. The comparison of several magnitudes thus forms the subject of the paper.

Assuming three homogeneous quantities, A, B, C, arranged in the order of their magnitudes, we take the second B as often as possible from the greatest A, and obtain a remainder less than B; this remainder may or may not be greater than C. If it be greater, we take C as often as possible from it, and obtain a remainder D less than C, the least of the three quantities. B, C, D may now be

treated in the same way, and thus we form a series of equations—

$$A = p_1 B + q_1 C + D$$
$$B = p_2 C + q_2 D + E$$
$$C = p_3 D + q_3 E + F, \text{ \&c.},$$

in which p can never be zero, while q may be so.

In order to compute, by help of these quotients, the approximate ratios of A, B, C, we may put A_1, A_2, A_3, &c.; B_1, B_2, B_3, &c.; C_1, C_2, C_3, &c., for the corresponding successive values, and then we obtain the equations—

$$A_{n+1} = p_{n+1} A_n + q_n A_{n-1} + A_{n-2},$$
$$B_{n+1} = p_{n+1} B_n + q_n B_{n-1} + B_{n-2},$$
$$C_{n+1} = p_{n+1} C_n + q_n C_{n-1} + C_{n-2},$$

which indicate a very simple arrangement, best studied from an example. Thus, if the successive equations were—

$$A = 2.B + 1.C + D$$
$$B = 3.C + 2.D + E$$
$$C = 2.D + 0.E + F$$
$$D = 3.E + 1.F + G$$
$$E = 2.F + 2.G + H$$
$$F = 3.G + 0.H + I$$
$$G = 2.H + 1.I + K$$
$$H = 3.I + 2.K + L, \text{ \&c.}$$

we should write the values of p, q, 1 in horizontal lines as in the accompanying scheme; and the successive approximate values of A, B, C in lines below them. Unit being written as the first value of A under p_1, which in this case is 2, we multiply this by 2, and

	1	1	1	1	1	1	1	1	1	
2	1	2	0	1	2	0	1	2	0	
p	2	3	2	3	2	3	2	3	2	
A	1	2	7	19	59	144	569	1197	4304	11571
B	...	1	3	8	25	61	241	507	1823	4901
C	...	...	1	2	6	15	59	124	446	1199
D	...	...	...	1	3	7	28	59	212	570
E	...	...	...	...	1	2	8	17	61	164
F	...	...	...	...	...	1	3	6	22	59
G	...	...	...	...	...	...	1	2	7	19
H	...	...	...	...	...	...	...	1	3	8
I	...	...	...	...	...	...	...	...	1	2
K	...	...	...	...	...	...	...	...	...	1

write the product in the column containing p_2, q_2. We then multiply the newly found A by the p above it; the preceding A by its q, that is in this case 3.2 and 1.1, and write the sum 7 as the third value of A. Again, taking the sum of the products p_3A_3, q_2A_2, and, as we may call it for generality's sake, r_1A_1, we have $2.7 + 2.2 + 1.1 = 19$ for A_4. In this way we obtain the successive values of A.

The values of B are found in the same way, observing that $B_1 = 0$, $B_2 = 1$. So also are the values of C, and if it be wished, those of D, E, F, &c., obtained, the first effective term being delayed a step, as shown in the scheme.

This method was applied to the three irrational quantities, log 5, log 3, and log 2; and the results were used in explaining the doctrine of musical temperaments.

When two quantities only are compared, it is well known that the cross products of the adjoining fractions differ by unit, or that, taking three contiguous terms, such as—

$$\frac{A_3}{B_3}, \frac{A_4}{B_4}, \frac{A_5}{B_5}, \text{ we have the equation,}$$

$$A_3B_4 - A_4B_3 = - A_4B_5 + A_5B_4,$$

which may be expressed, according to Cayley's notation of determinants—

$$\begin{vmatrix} A_3 & A_4 \\ B_3 & B_4 \end{vmatrix} = - \begin{vmatrix} A_4 & A_5 \\ B_4 & B_5 \end{vmatrix} = \pm 1 .$$

In the very same way, when three magnitudes are compared, we have the equation—

$$\begin{vmatrix} A_3 & A_4 & A_5 \\ B_3 & B_4 & B_5 \\ C_3 & C_4 & C_5 \end{vmatrix} = + \begin{vmatrix} A_4 & A_5 & A_6 \\ B_4 & B_5 & B_6 \\ C_4 & C_5 & C_6 \end{vmatrix} = + 1 .$$

that is to say, this determinant is unit throughout.

The extension of this method to more than three quantities is easy. In conclusion, an opinion was expressed, that as the Brounckerian process applied to two magnitudes has already thrown great light on the doctrine of squares, this extension of it may be expected to do as much for the still higher departments of the theory of numbers.

3. On the Forces experienced by Solids immersed in a Moving Liquid. By Sir William Thomson.

Cyclic irrotational motion,* [§ 60 (*z*)] once established through an aperture or apertures, in a movable solid immersed in a liquid, continues for ever after with circulation or circulations unchanged, [§ 60 (*a*)] however the solid be moved, or bent, and whatever influences experienced from other bodies. The solid, if rigid and left at rest, must clearly continue at rest relatively to the fluid surrounding it to an infinite distance, provided there be no other solid within an infinite distance from it. But if there be any other solid or solids at rest within any finite distance from the first, there will be mutual forces between them, which, if not balanced by proper application of force, will cause them to move. The theory of the equilibrium of rigid bodies in these circumstances might be called Kinetico-statics; but it is in reality a branch of physical statics simply. For we know of no case of true statics in which some if not all of the forces are not due to motion; whether as in the case of the hydrostatics of gases, thanks to Clausius and Maxwell, we perfectly understand the character of the motion, or, as in the statics of liquids and elastic solids, we only know that some kind of molecular motion is essentially concerned. The theorems which I now propose to bring before the Royal Society regarding the forces experienced by bodies mutually influencing one another through the mediation of a moving liquid, though they are but theorems of abstract hydrokinetics, are of some interest in physics as illustrating the great question of the 18th and 19th centuries:—Is action at a distance a reality, or is gravitation to be explained, as we now believe magnetic and electric forces must be, by action of intervening matter?

I. (Proposition.) Consider first a single fixed body with one or more apertures through it; as a particular example, a piece of straight tube open at each end. Let there be irrotational circulation of the fluid through one or more such apertures. It is readily

* The references §§ without farther title are to the author's paper on Vortex Motion, recently published in the Transactions (1869), which contains definitions of all the new terms used in the present article. Proofs of such of the propositions now enunciated as require proof are to be found in a continuation of that paper.

proved [from § 63 *Exam.* (2.)]* that the velocity of the fluid at any point in the neighbourhood agrees in magnitude and direction with the resultant electro-magnetic force, at the corresponding point, in the neighbourhood of an electro-magnet replacing the solid, constructed according to the following specification. The "core," on which the "wire" is wound, is to be of any material having infinite diamagnetic inductive capacity,† and is to be of the same size and shape as the solid immersed in the fluid. The wire is to form an infinitely thin layer or layers, with one circuit going round each aperture. The whole strength of current in each circuit, reckoned in absolute electro-magnetic measure, is to be equal to the circulation of the fluid through that aperture divided by $\sqrt{4\pi}$. The resultant electro-magnetic force at any point will be numerically equal to the resultant fluid velocity at the corresponding point in the hydrokinetic system, multiplied by $\sqrt{4\pi}$.

Thus, considering, for example, the particular case of a straight tube open at each end, let the diameter be infinitely small in comparison with the length. The "circulation" will exceed by but an infinitely small quantity the product of the velocity within the tube into the length. In the neighbourhood of each end, at distances from it great in comparison with the diameter of the tube and short in comparison with the length, the stream lines will be straight lines radiating from the end. The velocity, outwards from one end and inwards towards the other, will therefore be inversely as the square of the distance from the end. Generally at all considerable distances from the ends, the distribution of fluid velocity will be the same as that of the magnetic force in the neighbourhood of an infinitely thin bar longitudinally magnetised uniformly from end to end.

Merely as regards the comparison between fluid velocity and resultant magnetic forces, Euler's fanciful theory of magnetism is thus curiously illustrated. This comparison, which has been long known as part of the correlation between the mathematical theories of elec-

* Or from Helmholtz's original integration of the hydrokinetic equations.

† Real diamagnetic substances are, according to Faraday's very expressive language, relatively to lines of magnetic force, *worse conductors* than air.

The ideal substance of infinite diamagnetic inductive capacity is a substance which completely *sheds off* lines of magnetic force, or which is perfectly impervious to *magnetic force.*

tricity, magnetism, conduction of heat, and hydrokinetics, is merely kinematical, not dynamical. When we pass, as we presently shall, to a strictly dynamical comparison relatively to the mutual force between two hard steel magnets, we shall find the same law of mutual action between two tubes, with liquid flowing through each, but with this remarkable difference, that the forces are opposite in the two cases; unlike poles attracting and like poles repelling in the magnetic system, while in the hydrokinetic there is attraction between like ends and repulsion between unlike ends.

II. (Proposition.) Consider two or more fixed bodies, such as the one described in Prop. I. The mutual actions of two of these bodies are equal, but in opposite directions, to those between the corresponding electro-magnets. The particular instance referred to above shows us the remarkable result, that through fluid pressure we can have a system of mutual action, in which like attracts like with force varying inversely as the square of the distance. Thus, if the exit ends of tubes, open at each end with fluid flowing through them, be placed in the neighbourhood of one another, and the entering ends be at infinite distances, the mutual forces resulting will be simply attractions according to this law. The lengths of the tubes on this supposition are infinitely great, and therefore, as is easily proved from the conservation of energy, the quantities flowing out per unit of time are but infinitesimally affected by the mutual influence.

III. Proposition II. holds, even if one of the bodies considered be merely a solid, with or without apertures; if with apertures, having no circulation through them. In such a case as this the corresponding magnetic system consists of a magnet or electro-magnet, and a merely diamagnetic body, not itself a magnet, but disturbing the distribution of magnetic force around it by its diamagnetic influence. Thus, for example, a spherical solid at rest in the field of motion surrounding a fixed body, through apertures in which there is cyclic irrotational motion, will experience from fluid pressure a resultant force through its centre equal and opposite to that experienced by a sphere of infinite diamagnetic capacity, similarly situated in the neighbourhood of the corresponding electro-magnet. Therefore, according to Faraday's law for the latter, and the comparison asserted in Prop. I., it would experience a force from places of less towards places of greater fluid velocity,

irrespectively of the direction of the stream lines in its neighbourhood; a result easily deduced from the elementary formula for fluid pressure in hydrokinetics.

I have long ago shown that an elongated diamagnetic body in a uniform magnetic field tends, as tends an elongated ferromagnetic body, to place its length along the lines of force. Hence a long solid, pivoted on a fixed axis through its middle in a uniform stream of liquid, tends to place its length perpendicularly across the direction of motion; a known result (Thomson & Tait's "Natural Philosophy," § 335). Again, two globes held in a uniform stream with the lines joining their centres, require force to prevent them from mutually approaching one another. In the magnetic analogue, two spheres of diamagnetic or ferromagnetic inductive capacity repel one another when held in a line at right angles to the lines of force. A hydrokinetic result similar to this for the case of two equal globes, is to be found in Thomson and Tait's "Natural Philosophy," § 332.

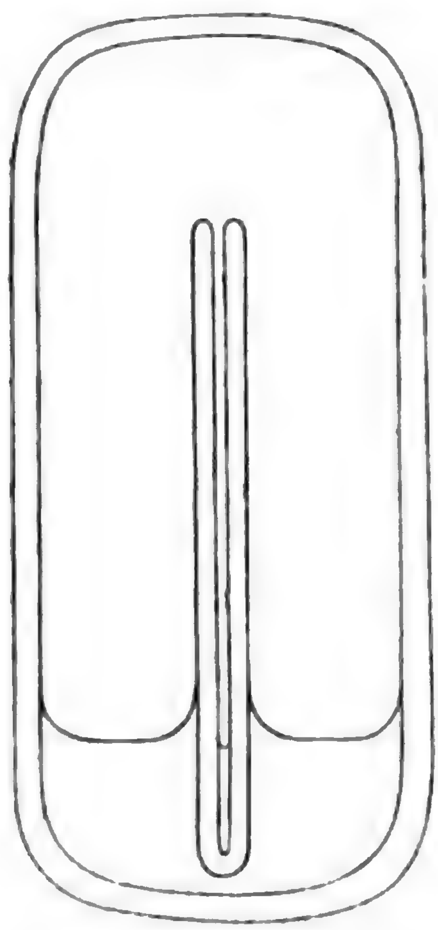
IV. (Proposition.) If the second body considered in § III., that is to say, a body either having no apertures, or, if perforated, having no circulation through the apertures, be acted on by one system of forces applied so as always to balance the resultant of the fluid pressure, calculated for it according to II. and III. for whatever position it may come to at any time, and if it be influenced, besides, by any other system of applied forces, superimposed on the former, it will move just as it would move, under the influence of the latter system of forces alone, were the fluid at rest, except in so far as compelled to move by the body's own motion through it. A particular case of this proposition was first published many years ago, by Professor James Thomson, on account of which he gave the name of "vortex of free mobility" to the cyclic irrotational motion symmetrical round a straight axis.

4. On the Equilibrium of Vapour at a Curved Surface of Liquid. By Sir William Thomson.

In a closed vessel containing only a liquid and its vapour, all at one temperature, the liquid rests, with its free surface raised or depressed in capillary tubes and in the neighbourhood of the solid boundary, in permanent equilibrium according to the same law of

relation between curvature and pressure as in vessels open to the air. The permanence of this equilibrium implies physical equilibrium between the liquid and the vapour in contact with it at all parts of its surface. But the pressure of the vapour at different levels differs according to hydrostatic law. Hence the pressure of saturated vapour in contact with a liquid differs according to the curvature of the bounding surface, being less when the liquid is concave, and greater when it is convex. And detached portions of the liquid in separate vessels all enclosed in one containing vessel, cannot remain permanently with their free surfaces in any other relative positions than those they would occupy if there were hydrostatic communication of pressure between the portions of liquid in the several vessels. There must be evaporation from those surfaces which are too high, and condensation into the liquid at those surfaces which are too low—a process which goes on until hydrostatic equilibrium, as if with free communication of pressure from vessel to vessel, is attained. Thus, for example, if there are two large open vessels of water, one considerably above the other in level, and if the temperature of the surrounding matter is kept rigorously constant, the liquid in the higher vessel will gradually evaporate until it is all gone and condensed into the lower vessel. Or if, as illustrated by the annexed diagram, a capillary tube, with a small quantity of liquid occupying it from its bottom up to a certain level, be placed in the neighbourhood of a quantity of the same liquid with a wide free surface, vapour will gradually become condensed into the liquid in the capillary tube until the level of the liquid in it is the same as it would be were the lower end of the tube in hydrostatic communication with the large mass of liquid. Whether air be present above the free surface of the liquid in the several vessels or not, the condition of ultimate equilibrium is the same; but the processes of evaporation and condensation through which equilibrium is approached will be very much retarded by the presence of air. The experiments of Graham, and the kinetic theory of Clausius and Maxwell, scarcely yet afford us sufficient data for estimating the rapidity with which the vapour proceeding from one of the liquids will diffuse itself through the air and reach the surface of another liquid at a lower level. With air at anything approaching to ordinary atmospheric

density to resist the process, it is probable it would be too slow to show any results unless in very long continued experiments. But if the air be removed as perfectly as can be done by well-known practical methods, it is probable that the process will be very rapid: it would, indeed, be instantaneous, were it not for the cold of evaporation in one vessel and the heat of condensation in the other. Practically, then, the rapidity of the process towards hydrostatic equilibrium through vapour between detached liquids, depends on the rate of the conduction of heat between the several surfaces through intervening solids and liquids. Without having



made either the experiment, or any calculations on the rate of conduction of heat in the circumstances, I feel convinced that in a very short time water would visibly rise in the capillary tube indicated in the diagram, and that, provided care is taken to maintain equality of temperature all over the surface of the hermetically sealed vessel, the liquid in the capillary tube would soon take very nearly the same level as it would have were its lower end open; sinking to this level if the capillary tube were in the beginning filled too full, or rising to it if (as indicated in the diagram) there is not enough of liquid in it at first to fulfil the condition of equilibrium.

The following formulæ show precisely the relations between curvatures, differences of level, and differences of pressure, with which we are concerned.

Let ρ be the density of the liquid, and σ that of the vapour; and let T be the cohesive tension of the free surface, per unit of breadth, in terms of weight of unit mass, as unit of force. Let h denote the height of any point, P, of the free surface above a certain plane of reference, which I shall call for brevity the plane level of the free surface. This will be sensibly the actual level of the free surface in regions, if there are any, with no part of the edge (or bounding line of the free surface where liquid ends and solid begins) at a less distance than several centimetres. Lastly, let r and r' be the principal radii of curvature of the surface at P. By Laplace's well-known law, we have, as the equation of equilibrium,

$$(\rho - \sigma)h = \mathrm{T}\left(\frac{1}{r} + \frac{1}{r'}\right) \quad . \quad . \quad . \quad (1).$$

Now, in the space occupied by vapour, the pressure is less at the higher than at the lower of two points whose difference of levels is h, by a difference equal to σh. And there is permanent equilibrium between vapour and liquid at all points of the free surface. Hence the pressure of vapour in equilibrium is less at a concave than at a plane surface of liquid, and less at a plane surface than at a convex surface, by differences amounting to $\frac{\mathrm{T}\sigma}{\rho - \sigma}$ per unit difference of curvature. That is to say, if ϖ denote the pressure of vapour in equilibrium at a plane surface of liquid, and p the pressure of vapour of the same liquid at the same temperature presenting a curved surface to the vapour, we have

$$p = \varpi - \frac{\mathrm{T}\sigma}{\rho - \sigma}\left(\frac{1}{r} + \frac{1}{r'}\right) \quad . \quad . \quad (2),$$

$\frac{1}{r}$ and $\frac{1}{r'}$ being the curvatures in the principal sections of the surface bounding liquid and vapour, reckoned positive when concave towards the vapour.

In strictness, the value of σ to be used in these equations, (1) and (2), ought to be the mean density of a vertical column of vapour, extending through the height h from the plane of reference.

But in all cases to which we can practically apply the formulæ, according to present knowledge of the properties of matter, the difference of densities in this column is very small, and may be neglected. Hence, if H denote the height of an imaginary homogeneous fluid above the plane of reference, which, if of the same density as the vapour at that plane, would produce by its weight the actual pressure ϖ, we have

$$\sigma = \frac{\varpi}{\mathrm{H}} .$$

Hence by (1) and (2)

$$p = \varpi \left(1 - \frac{h}{\mathrm{H}}\right) \quad . \quad . \quad . \quad (3).$$

For vapour of water at ordinary atmospheric temperatures, H is about 1,300,000 centimetres. Hence, in a capillary tube which would keep water up to a height of 13 metres above the plane level, the curved surface of the water is in equilibrium with the vapour in contact with it, when the pressure of the vapour is less by about $\frac{1}{1000}$th of its own amount than the pressure of vapour in equilibrium at a plane surface of water at the same temperature.

For water the value of T at ordinary temperatures is about ·08 of a gramme weight per centimetre; and ρ, being the mean of a cubic centimetre, in grammes, is unity. The value of σ for vapour of water, at any atmospheric temperature, is so small that we may neglect it altogether in equation (1). In a capillary tube thoroughly wet with water, the free surface is sensibly hemispherical, and therefore r and r' are each equal to the radius of the inner surface of the liquid film lining the tube above the free liquid surface; we have, therefore,

$$h = \cdot 08 \times \frac{2}{r} .$$

Hence, if $h = 1300$ centimetres, $r = \cdot 00012$ centimetres. There can be no doubt but that Laplace's theory is applicable without serious modification even to a case in which the curvature is so great (or radius of curvature so small) as this. But in the present state of our knowledge we are not entitled to push it much further. The molecular forces assumed in Laplace's theory to be "insensible at sensible distances," are certainly but little, if at all, sensible at distances equal to or exceeding the wave lengths of ordinary light. This is directly proved by the most cursory observation of soap

bubbles. But the appearances presented by the black spot which abruptly ends the series of colours at places where the bubble is thinnest before it breaks, make it quite certain that the action of those forces becomes sensible at distances not much less than a half wave length, or $\frac{1}{40000}$ of a centimetre. There is, indeed, much and multifarious evidence that in ordinary solids and liquids, not merely the distances of sensible inter-molecular action, but the linear dimensions of the molecules themselves, and the average distance from centre to nearest centre,* are but very moderately small in comparison with the wave lengths of light. Some approach to a definite estimate of the dimensions of molecules is deducible from Clausius' theory of the average spaces travelled without collision by molecules of gases, and Maxwell's theory and experiments regarding the viscosity of gases. Having perfect confidence in the substantial reality of the views which these grand investigations have opened to us, I find it scarcely possible to admit that there can be as many as 10^{27} molecules in a cubic centimetre of liquid carbonic acid or of water. This makes the average distance from centre to nearest centre in the liquids exceed a thousand-millionth of a centimetre!

We cannot, then, admit that the *formulæ* which I have given above are applicable to express the law of equilibrium between the moisture retained by vegetable substances, such as cotton cloth or oatmeal, or wheat-flour biscuits, at temperatures far above the dew point of the surrounding atmosphere. But although the energy of the attraction of some of these substances for vapour of water (when, for example, oatmeal, previously dried at a high temperature, has been used, as in the original experiment of Sir J. Leslie, to produce the freezing of water under the receiver of an air-pump), is so great that it might almost claim recognition from chemists as due to a "chemical affinity," and resulting in a "chemical combination," I believe that the absorption of vapour into fibrous and cellular organic structures is a property of matter continuous with the absorption of vapour into a capillary tube demonstrated above.

* By "average distance from centre to nearest centre," I mean the side of the cube in a cubic arrangement of a number of points equal to the number of real molecules in any space.

5. On a Bow seen on the Surface of Ice. By J. Clerk Maxwell, Esq., F.R.SS. L. & E.

On the 26th of January, about noon, I observed the appearance of a coloured bow on the frozen surface of the ditch which surrounds S. John's College, Cambridge. Its appearance and position seemed to correspond with those of an ordinary primary rainbow. I at once made a rough measurement of the angle on the board of a book which I had with me, and then borrowed from Dr Parkinson, President of S. John's College, a sextant, with which I found that the angle between the bright red and the shadow of the large mirror was 41° 50′, and that for bright blue 40° 30′. The angle for the extreme red of the primary bow, as given in Parkinson's Optics, is 42° 20′, and that for violet 40° 32′. The bows formed by ice crystals are seen on the same side as the sun, and not on the opposite side. I suppose the bow which I saw to be formed by small drops of water lying on the ice. If the lower part of each drop were flattened, so as to bring the point at which the reflexion takes place nearer to the points of incidence and emergence, the effect would be of the same kind as that of a diminution of the index of refraction—that is, the angle of the bow would be increased. How a drop of water can lie upon ice without wetting it, and losing its shape altogether, I do not profess to explain.

Only a small part of the ice presented this appearance. It was best seen when the incident and emergent rays were nearly equally inclined to the horizontal. The ice was very thin, and I was not able to get near enough to the place where the bow appeared to see if the supposed water drops really existed.

The following Gentlemen were admitted Fellows of the Society:—

W. E. Heathfield, Esq., F.R.G.S., F.C.S.
Edward James Shearman, M.D., F.R.C.S.L.
Patrick D. Swan, Esq.
Dr H. Alleyne Nicholson.

A ballot also took place for the Rev. Dr Hodson, who resigned the Fellowship of the Society in 1867. Dr Hodson was re-admitted.

Monday, 21*st February* 1870.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Note on the Atomic Volume of Solid Substances. By James Dewar, Lecturer on Chemistry, Veterinary College, Edinburgh.

The investigation of the volume retained by different elementary substances, when combined in the solid condition, has attracted the attention of many chemists. We have only to look at the laborious memoirs of Schröter, Kopp, Playfair and Joule, Boullay, Filhol, and others, to be convinced of the great amount of labour expended on the subject. Nor is it at all remarkable that so many workers should take to this field of research, when we remember the simplicity of the laws regulating the combining volumes of gaseous substances, and the probable extension of some such similar law to the solid condition of matter. Emboldened by analogy, the forementioned workers endeavoured to find some constant to which volumes of elements and compounds held the relation of some simple multiple, and thus extend the apparent simplicity of Prout's law of combining weights to combining volumes. The great object in view was evidently to extend the speculations and laws of Dalton and Gay Lussac to the volumes of solid substances, and thus to arrive at some general explanation of the results. However creditable the desire to reveal simplicity from out of the apparent chaos, no one, in examining the subject, can help arriving at the conclusion that the means employed to extract the seeming harmony from the results were purely arbitrary. It does not follow, however, that the results were fruitless, although no great generalisation was discovered. The solid state of matter is relatively far more complicated than either the liquid or gaseous conditions. The uniformity of expansion of gaseous matter, and the easy comparison of liquid substances under similar conditions, enable us to arrive at some satisfactory conclusions regarding the volume in these states: but, in examining solid matter, we have no guarantee

that the substances are under similar physical conditions. We cannot, therefore, expect the same uniformity in the results; but although, strictly speaking, we may entertain grave doubt on the real value of the results, yet, in some cases, we cannot help recognising some curious analogies, especially on comparing similar classes of compounds. It is not the object of this note either to criticise or discuss the labours and speculations of others, no originality being claimed in the subject matter itself, all that is original being merely the addition of a few new analogies.

The first important discovery in the subject of atomic volumes was made by Schröter. He observed that the equivalent volume of oxygen, obtained by subtracting the volume of metal in the free state from the volume of the oxide, gave, approximately, the same value of 5·2 in the oxides of copper, zinc, cadmium, lead, mercury, iron, cobalt, and titanium. In other words, the oxygen occupied the same volume in each combination. Other classes of oxides gave a volume of twice, or half the above number. In order to arrive at the volume of the oxygen, Schröter started with the premises that the metal in the combined state occupied the same volume as the uncombined metal. Granting, for the present, that oxygen has a definite volume in combination in the oxides, it is clear that the volume obtained by difference will vary with the volume of the combined metal. The same method applied to the oxides of the less dense metals would give a negative volume to the oxygen; and in these cases we must admit condensation to have taken place in the metal itself. We may have three cases, therefore, according as the volume of the combined metal differs from that of the uncombined. If it remains the same in combination, we obtain the real volume; if it condenses, the volume is a minimum; if it expands, a maximum. Seeing that the oxygen in the dense metals has the volume 5·2, we may regard the greater and smaller volume obtained from some oxides as the result of condensation or expansion of the metal. Supposing the above volume (5·2) to exist generally in the oxides, we would have a condensation in the less dense metals in combination, approaching very nearly, in the case of potassium, sodium, and aluminium, to one-third, and in calcium, magnesium, and strontium to nearly one-half, of the volume in the free state. Thus far,

then, this number would give a rough explanation in admitting condensation in many of the metals.

I have thought that it would be interesting to compare this volume with the volume of oxygen when it is combined with solid substances other than metallic, and to take a series of analogous combinations. For this purpose the chlorine family is well fitted in their respective combinations with potassium, and these with oxygen. The following table contains the best known density determinations and volumes of chloride, bromide, and iodide of potassium, compared with the densities of chlorate, bromate, and iodate.

	Sp. gr.	Volume.	Difference.	Mean vol. of O (=16)
KCl . .	1·977	37·6	15	5
$KClO_3$ . .	2·326	52·6		
KBr . .	2·69	44	7	2·3
$KBrO_3$ . .	3·271	51		
KI . . .	3·0	55·3	–1·6	–0·5
KIO_3 . .	3·979	53·7		

The total volume of the oxygen in chlorate of potash, on the supposition the chloride of potassium retains its original volume in combination, is 15; whereas it is only 7 in bromate of potash, if we allow that the bromide of potassium retains its original volume; and it appears to occupy no volume in iodate of potash, assuming that iodide of potassium maintains its original volume. The apparent disappearance of the volume of the oxygen, in changing iodide of potassium into iodate, is analogous to the apparent loss of volume of many salts in their water of hydration, the salt occupying the volume of the crystal water taken as ice, as pointed out many years ago by Playfair and Joule. It is clear that, in assuming the halogen compounds of potassium as retaining their primitive volume in their oxidised derivatives, we place these compound substances in the same position as the metals in the simple oxides. Now, we saw that in many oxides the volume of the oxygen varied, and that, in all probability, from metallic condensation taking place during the act of combination. The metals having the lowest density and the greatest atomic volume condense the most in combining. Generally speaking, if we examine the

volumes of the halogen salts in the above table, it is clear that the equivalent volumes increase, chloride of potassium being 37, bromide 44, and iodide 55·3, and their relative stability diminishes. The equivalent volumes of chlorine, bromine, and iodine are identical in the liquid state; and thus the formation of the respective potassium compounds is one of the results of unequal condensation, the co-efficient of contraction in the formation of chloride of potassium being 0·46, bromide 0·29, iodide 0·23 per unit volume. Their formation is attended with the evolution of very different amounts of heat. The following table contains some of the constants found with reference to combination and solution:—

Constants of Group.

	Contraction per Unit Volume.	Total heat.	Heat of solution.	Diffusion times (relative).	Co-efficient of expansion per equivalent volume.	Specific heat per atom.
KCl	0·46	97086	3874	74·5	0·001429	12·88
KBr	0·29	85666	4522	119	0·001848	13·47
KI	0·23	72721	4847	166	0·002358	13·60

Generally speaking, the number found for bromide of potassium is nearly the mean of those attached to chloride and iodide. A similar observation has recently been made by M. Valsen in examining the equivalent capillary constants of these bodies. Looking at the atomic thermal number, there is a far greater likelihood of condensation taking place in the bromide and iodide of potassium in the combined state, than in case of chloride, seeing that it would be relatively far more difficult to condense. But neither the chlorate, bromate, nor iodate can be produced through the direct addition of oxygen to the respective halogen salt. And the chlorate, it is well known, evolves heat on giving off its oxygen, and thus necessitates an absorption of heat during combination. It is just possible that the heat produced during the decomposition is the result of the necessary expansion of volume in the chloride of potassium in combining with oxygen, and its return to its normal volume on losing it. It makes no change in volume to suppose that, in the one case, the oxygen is added as a whole to the chloride of potassium, or, in the other, that it is between the

potassium and chlorine, each occupying its individual volume unchanged, but it would alter greatly the heat evolved in so doing. If oxygen combined with chloride of potassium as a whole, without any condensation taking place, the natural result would be an evolution of heat. But if the addition of the oxygen diminishes the co-efficient of contraction, as compared with that of the free compound, then we have a physical explanation of the evolution of heat on decomposition. In this case the actual work performed by the condensation of oxygen is retained in a potential form, and, therefore, reappears as heat on its decomposition. If, now, we examine the mode in which the oxygen is attached to the respective halogen compounds, we can trace, as a necessary consequence, the retention of varying amounts of energy. Chlorate, bromate, and iodate of potash are formed by a similar chemical reaction, according to the following formula of exchange, given in equivalents, the whole reaction supposed to take place in the presence of water:—

$$\underset{6(76238)}{6KO} + 6Cl = \underset{5(97086)}{5KCl} + KClO_6$$

$$\underset{6(76238)}{6KO} + 6Br = \underset{5(85666)}{5KBr} + KBrO_6$$

$$\underset{6(76238)}{6KO} + 6I = \underset{5(72721)}{5KI} + KIO_6$$

We have appended the thermal equivalents attending the formation of these bodies in a large volume of water. It will be obvious on comparing the formation of chlorate of potash, through the above reaction, that it may be the result of absorption of heat; whereas it is certain that the formation of iodate of potash must be attended with an evolution of heat, or else cold must be the result of their action. In special experiments, made with the object of determining the thermal action, neither absorption nor evolution of heat could be detected. Thus the formation of iodate of potash is attended with an evolution of heat. This would, then, accord with the easy transformation of the chlorates into iodates, or of chloric acid into iodic acid, and the easy transformation of the iodide of potassium into the iodate, through the action of permanganate of potash, seeing that we must have an evolution of heat.

The oxygen, therefore, may be assumed to be in a very different condition relatively to the other elements, or else we must suppose that it has not affected the co-efficient of contraction, certainly not to have diminished it. The author throws out this simply as a possible explanation; he is also well aware that many other explanations might be given, all, possibly, equally satisfactory. But a physical explanation, however far it may lie from the truth, seems to convey to us the clearest ideas of what may possibly take place.

There is one point connected with the subject of volumes that requires very careful attention. All bodies in combining do not unite with condensation; that is, the volume of the compound might exceed the volumes of the isolated constituents, and yet a large evolution of heat might take place during its formation. A well-known example is that of iodide of silver. Now, M. Fizeau has shown that iodide of silver contracts regularly with increase of temperature, and M. St Claire Deville has given an explanation of this anomaly. Deville believes that bodies combine at such a temperature as would be required to transform the volume of the compound to that of the sum of the volumes of its constituents in the free state. Applying this to iodide of silver, it is clear that contraction must take place, and in all similar cases where we have an increase of volume. One cannot help associating this increase of volume to a purely physical change of state, such as the change of water with expansion into ice. Now, as Sir William Thomson has proven that pressure lowers the freezing point of water, and Mousson has actually liquefied ice by enormous pressure, if the formation of a chemical compound is analogous to a physical change of state, we ought to be able by mere pressure to decompose a chemical compound, if the formation of that compound is attended with an increase of volume. No doubt, in order to get experimental proof of this fact, we must use a relatively weak chemical compound, one attended with the evolution of no great amount of heat; and the well-known experiments of Joule on the effect of pressure on amalgams, seems to confirm my anticipation. Joule has shown that the amalgams of zinc, lead, and tin are decomposed by pressure alone, and these are the amalgams produced with the least contraction of any. In order to get definite proof of

the expansion, it is, of course, necessary to use the specific gravity of mercury in the solid state. Now, Joule states, as the mean of his experiments, that mercury in the solid form has the specific gravity 15·19, whereas in the above amalgams it would have the density of only 14·1. The observations of Matthiessen on the specific gravity of alloys enables us to confirm Joule's results:—

Lead Series (A. Matthiessen).

	Sp. Gr.	Calculated Sp. Gr.	$\frac{V + V'}{v}$
Pb_2Hg, . . .	11·979	12·008	1·0024
PbHg, . . .	12·484	12·358	0·9899
$PbHg_2$, . . .	12·815	12·734	0·9937

The specific gravity of the mercury used in calculating the mean density was 13·573. Now, seeing that there is little or no contraction, and even in one case a slight expansion, in taking the above specific gravity of mercury, the higher density of mercury given by Joule as the result of his experiments would necessarily lead to an expansion in their formation. To illustrate the effect of pressure on the composition of an amalgam, let us take Joule's experiments on the tin amalgam. The composition of this amalgam was 100 of mercury to 51·01 of tin, and the specific gravity 10·518. The effect of 5400 lbs. pressure for thirty days, changed the amalgam, so that it had ultimately the composition 100 of mercury to 384 of tin. It is natural to believe, therefore, that the effect of pressure in this case is quite analogous to the inverse change of state, when a body that has expanded in changing its state has been subjected to its influence.

In the early part of this paper we saw that the volume of oxygen in some oxides, instead of being 5·2, was sometimes double this amount, or even more. It has also been remarked, that if the metal in combining was to expand, the volume of the oxygen would appear as a maximum. This apparently large volume of the oxygen seems to belong to sub-oxides, such as sub-oxides of mercury and copper, and oxide of silver. If we suppose, now, that this large increase of volume in the oxygen is the result of an expansion in the metal in combining with the normal oxide, it is

possible that mere pressure would decompose these oxides, at least in part, into metal and the higher oxide. The instability of a body of this type, such as sub-oxide of mercury, is well known, mere titration effecting the liberation of metal with formation of the higher oxide. In this way, therefore, it seems to support the argument adduced.

2. Note on Inverted Sugar. By James Dewar, Lecturer on Chemistry, Veterinary College, Edinburgh.

For some time past an animated discussion has been going on in the columns of the "Comptes Rendus de l'Académie des Sciences" between MM. Dubranfaut and Maumené regarding the nature of inverted sugar. M. Dubranfaut, many years ago, made many valuable additions to our knowledge concerning the composition and reactions of various sugars, especially in explaining the result of the action of dilute acids on cane sugar. He explained the levo-rotatory action of inverted sugar, and its rapidly varying power with the temperature, as the result of a molecule of water in reacting with a molecule of cane sugar, generating one molecule of glucose and one of lævulose. Dubranfaut believed that inverted sugar consisted of a mixture of glucose and lævulose in equal weights; and although he did not make a direct analysis of the product, yet he was justly entitled to assume that it was so constituted, seeing that, generally, it agreed with a mean of the properties of inulin sugar and dextrose.

In order to support the above view, he separated levo-glucose from the inverted sugar, through the insolubility of the lime compound, and compared its properties with pure lævulose. The decomposition would, according to Dubranfaut, be as follows :—

$$\underset{+73\cdot 8}{C_{12}H_{22}O_{11}} + H_2O = \underset{+56}{\overset{+}{C_6H_{12}O_6}} + \underset{-106}{\overset{-}{C_6H_{12}O_6}}$$
$$(-25)$$

So thoroughly had his facts and explanations been accepted by chemists generally, that, up till a recent date, no one discovered any flaw in his researches, and therefore no doubt was thrown on the validity of this theory. Recently, Maumené has reinvestigated

the composition of inverted sugar by analysis. He has attempted to separate the two sugars through the action of chloride of sodium. The dextro-glucose forms a well-defined crystalline compound with chloride of sodium, whereas the lævulose does not form any compound. The results obtained by this method differ greatly from theory. Instead of finding 50 per cent. of lævulose, he found 88 per cent. In repeating the experiments of Dubranfaut on the separation of levo-glucose by hydrate of lime, he has not met with any better results; in fact, his results are quite opposed to those of Dubranfaut.

Apart altogether from expressing an opinion on the merits of the views entertained by the different parties to this discussion, the author has thought some observations of the same subject might not be unworthy of notice at the present time.

Linneman, many years ago, applied the process of hydrogenation to the sugars that he had found so successful in treating the simple organic substances. In the way named he obtained mannite from inverted sugar, the following reaction taking place :—

$$C_6H_{12}O_6 + H_2 = C_6H_{14}O_6 .$$

Mannite had long been known to be the product of certain kinds of fermentation, and occurring as a secondary product in the vinous fermentation; but it was this elegant synthesis of Linneman that first clearly showed the connection. But although inverted sugar can be changed into mannite, the next point that demands a solution is the proving the inverted sugar to be composed of equal quantities of dextrose and lævulose. Are they both transformed by hydrogenation into mannite? or is only one of them, and which? Linneman seems to have directed his attention to the solution of this question. He states that it is only the lævulose that is so affected. The reasons why he entertains the above views are not given. In all likelihood he thought that, just as Berthelot had changed mannite by a peculiar fermentation into levo-glucose, so would the levo-glucose in inverted sugar be hydrogenised into mannite.

In repeating the action of sodium amalgam on inverted sugar, I have not seen any reason why the one sugar any more than the other should be supposed to generate the mannite. The following is a description of the mode by which the sugar was inverted and hydro-

genised:—Twenty grammes of cane sugar were dissolved in 150 grms. of water, and inverted through the action of 2 grms. of sulphuric acid, keeping the solution at the temperature of 70° C., afterwards adding pure carbonate of barium, filtering, and then adding one gramme of sodium in the form of a weak amalgam. The action took place without any evolution of hydrogen. If the amalgam was impure, from the presence of other metals, it evolved hydrogen at once, and the solution became brown; otherwise it remained perfectly clear. After one month the solution gave no trace of sugar with the alkaline copper solution. It was then carefully neutralised with dilute sulphuric acid, evaporated on the water bath, the greater part of the sulphate of sodium separated by crystallisation, and the residue treated with boiling 70 per cent. alcohol, the solution filtered, and allowed to crystallise. Sometimes the mannite did not crystallise until all the alcohol had evaporated, leaving a syrup that slowly assumed the crystalline form. The product had no rotatory power. In no case was the sugar entirely changed into mannite—a gummy substance was invariably left, that would not crystallise after exposure to the air for months. Mannitan, or some similar body, may be one of the products.

Dextro-glucose made from honey gave mannite when treated in the same way, having exactly the same melting point as ordinary mannite. In treating milk sugar with dilute sulphuric acid, changing into gallactose and hydrogenising, dulcite was not isolated; but I have not specially studied the reaction.

3. On the Flow of Electricity in Conducting Surfaces. By W. R. Smith, M.A., Assistant to the Professor of Natural Philosophy in the University of Edinburgh. Communicated by Professor Tait. (With a Plate.)

The conditions of a steady flow of electricity in a conducting surface are completely determined, if we know either the nature of the electrical distribution throughout the surface, or the direction and intensity of the flow at every point. On the first of these ways of considering the question, the problem is solved if we can express the potential v at any point as a function of the co-ordinates, and

the nature of the distribution will be indicated to the eye by forming the equipotential curves

$$v = \text{const.} \quad . \quad . \quad . \quad . \quad (1).$$

From the second point of view, we should endeavour to determine the lines of flow by equations of the form

$$u = \text{const.} \quad . \quad . \quad . \quad . \quad (2).$$

The curves determined by equations (1) and (2) are obviously orthogonal, and since

$$\frac{d^2v}{dx^2} + \frac{d^2v}{dy^2} = 0,$$

we know, by a theorem of Lamé and Stokes,* that

$$\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} = 0\,.$$

Kirchhoff, in the year 1845, took up the problem for plane surfaces† in the first of the two ways we have indicated. By an application of Ohm's law, he expressed analytically the conditions to be satisfied by v. When the electricity enters and issues by a number of individual points, he found (apparently by trial) that an integral of the form $\Sigma(a \log r)$, where r_1 r_2, &c., are the distances of the point (x, y) from the successive points of entrance and issue, satisfies these conditions when the plate is infinite. For a finite plate, it is necessary that the boundary of the plate should be orthogonal to the curves

$$\Sigma(a \log r) = \text{const.} \quad . \quad . \quad . \quad (3).$$

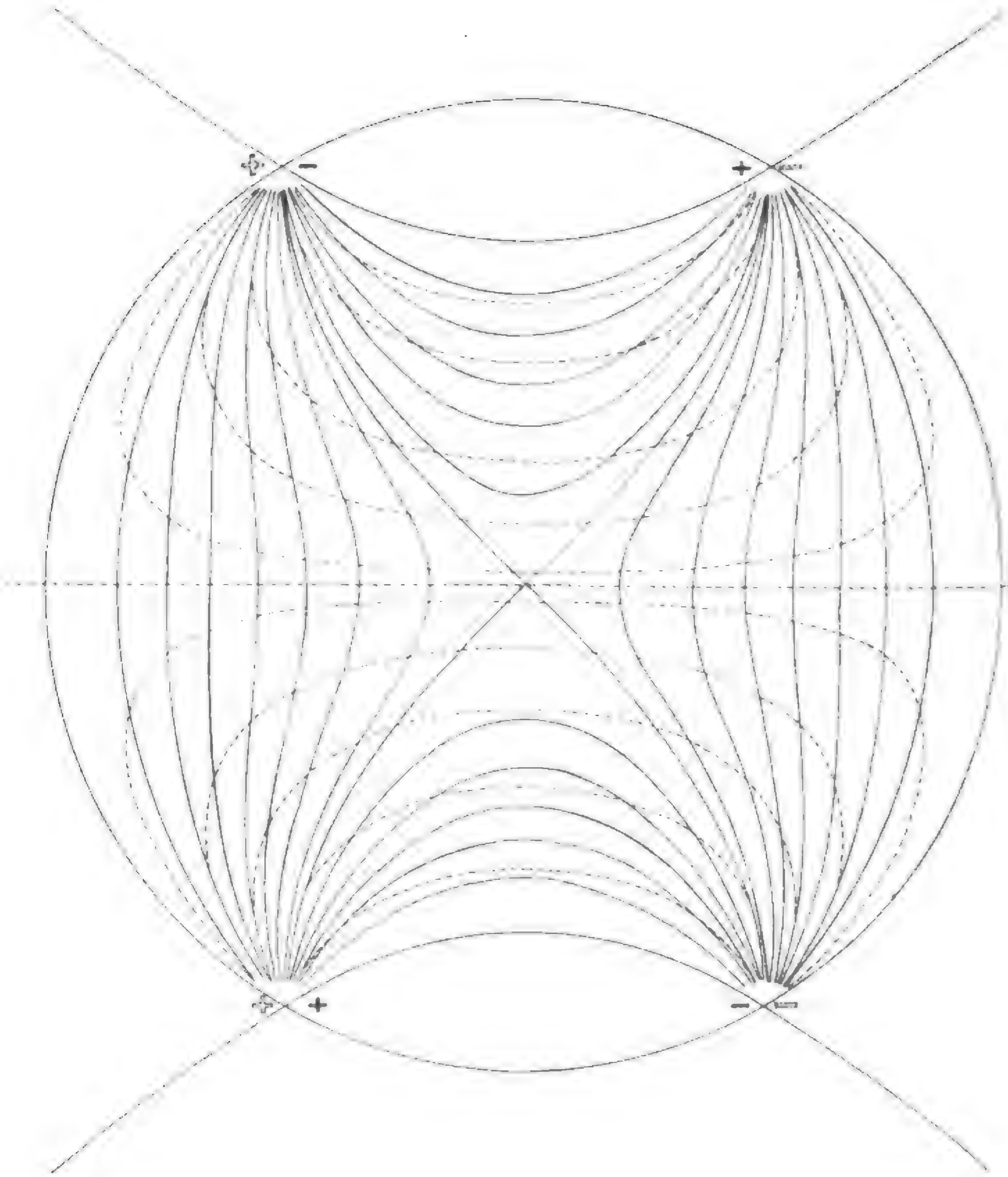
He was thus led to form the orthogonal curves, whose equation he gives in the form

$$\Sigma(a\,[r, \mathrm{R}]) = \text{const.} \quad . \quad . \quad . \quad (4),$$

where $[r, \mathrm{R}]$ is the angle between r and a fixed line R. These equations he applies to the case of a circular plate, completely determining the curves when there is one exit and one entrance point in the circumference, and showing that in any case a proper number of subsidiary points would make the equipotential lines determined by (3), cut the circumference at right angles. Kirch-

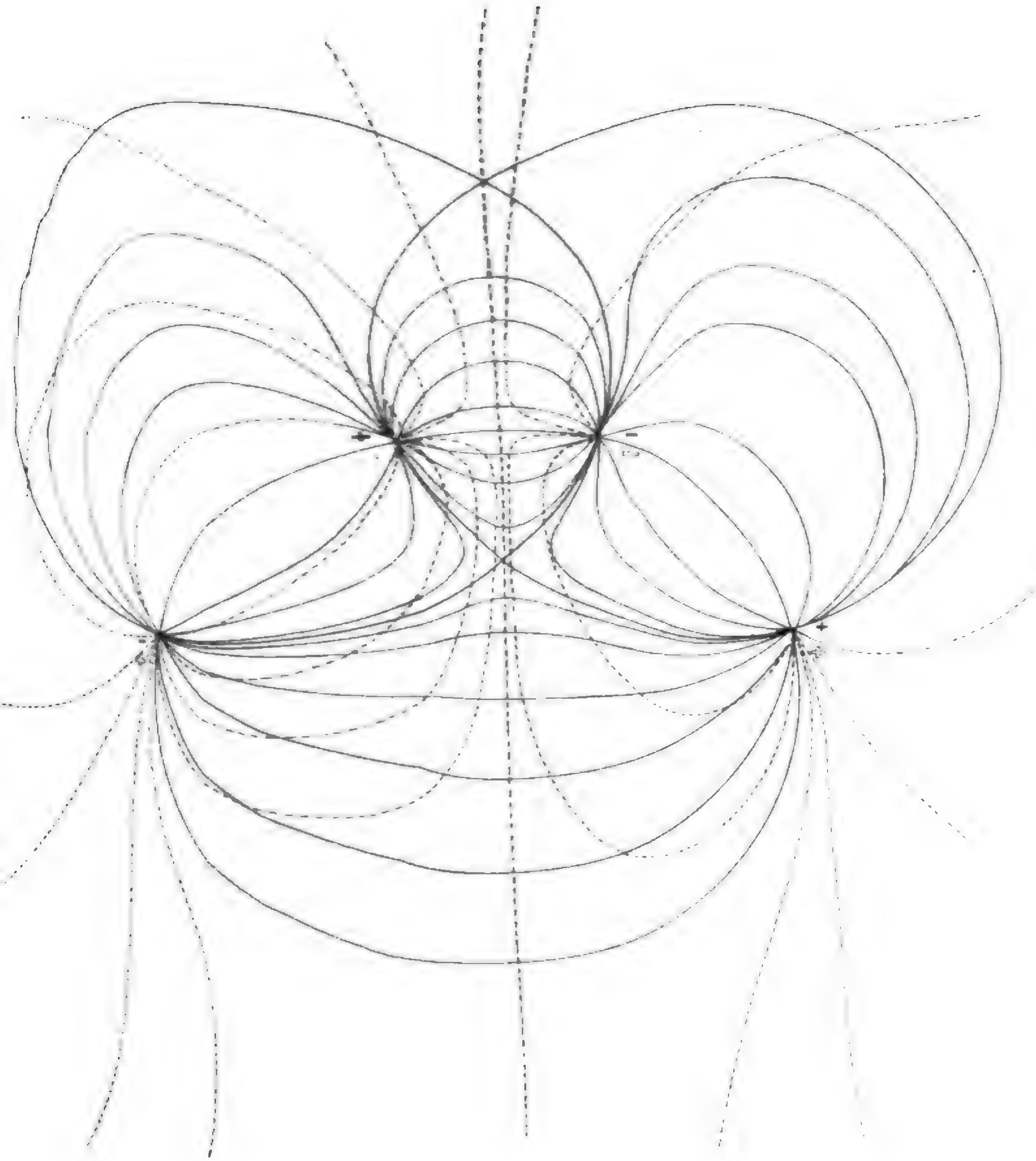
* See Thomson and Tait's Natural Philosophy, i. 542.

† Poggendorff's Annalen, Bd. lxiv.



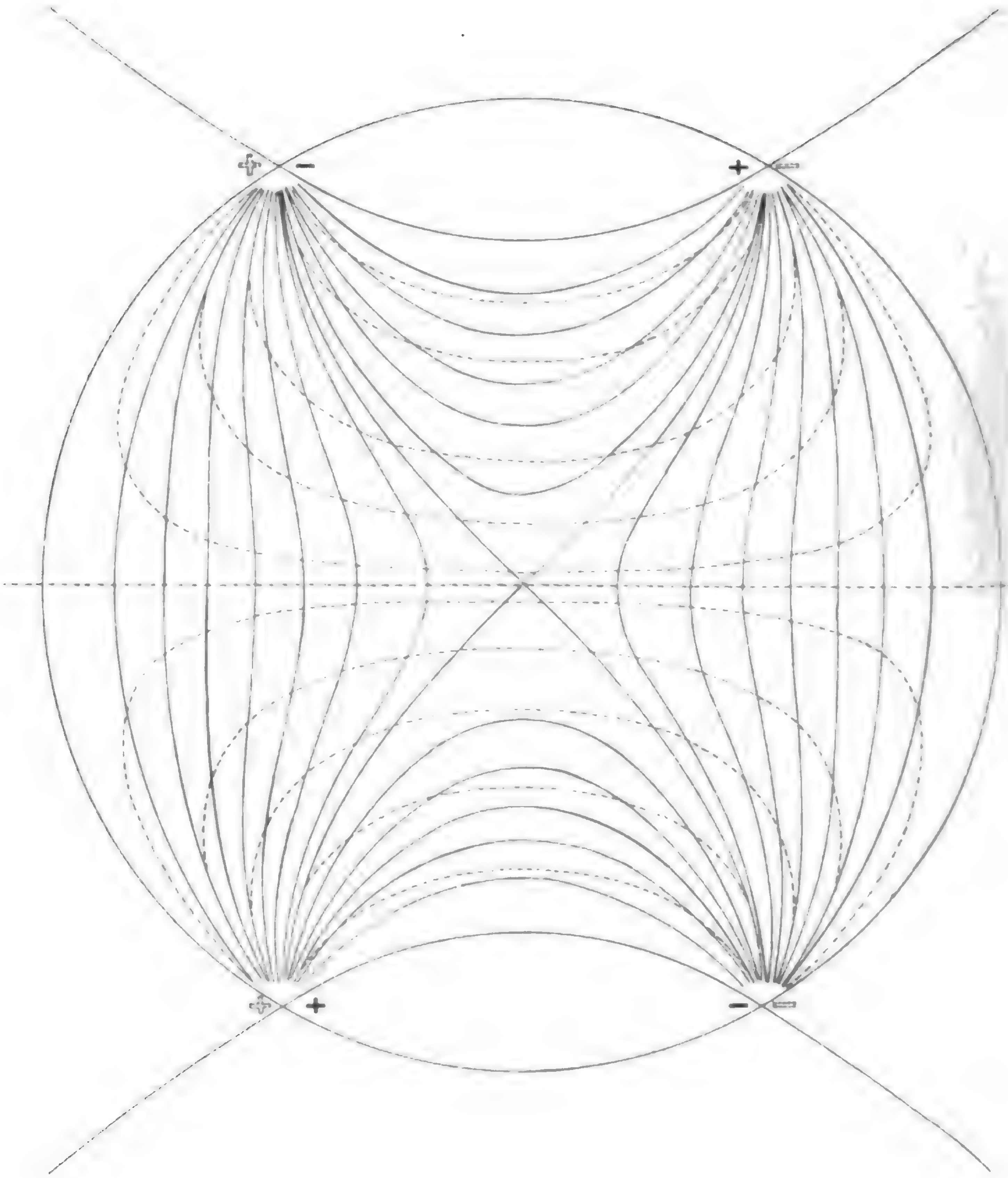
A

Lines of flow when the sources form a rectangular parallelogram, whose diagonals make an angle of $\frac{\pi}{3}$. The unbroken lines are lines of flow when sources of the same sign are diagonally opposite. One line sinks to a circle, another to a rectangular hyperbola, the rest are lemniscates. When a source and sink are transposed the circle is still part of a stream whose other branch is a straight line, but the lemniscates pass over into the dotted curves.



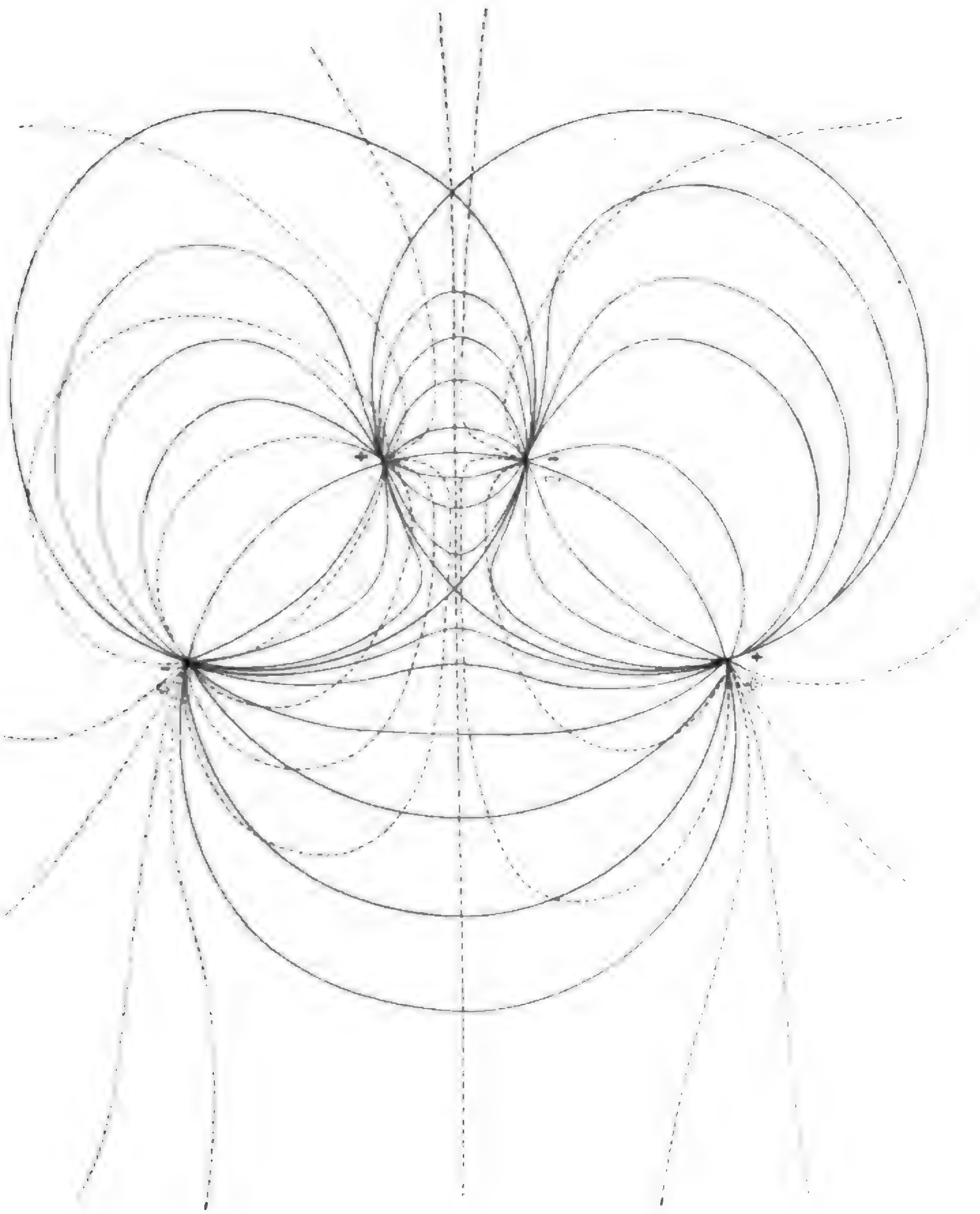
B

Case of two sources and two sinks giving three equal stream circles with two points of zero flow. When a source and sink are transposed the lower circle is still a line of flow. The other lines assume the dotted form.



A

Lines of flow when the sources form a rectangular parallelogram, whose diagonals mak[e] an angle of $\frac{\pi}{3}$ The unbroken lines are lines of flow when sources of the same sign are diagonally opposite. One line sinks to a circle, another to a rectangular hyper bola, the rest are lemniscates. When a source and sink are transposed the circle is still part of a stream whose other branch is a straight line, but the lemniscates pass over into the dotted curves.



B

Case of two sources and two sinks giving three equal stream circles with two points of zero flow. When a source and sink are transposed the lower circle is still a line of flow. The other lines assume the dotted form.

hoff's paper is throughout properly busied with the function v, and the stream lines are only dealt with incidentally. There is no attempt to give a physical meaning to the equation (4).

In 1846, Thomson drew attention to the orthogonal systems (3) and (4), as an example of Lamé's theorem.* He showed that the rings and brushes of biaxal crystals are a special case of these curves. They correspond, in fact, as we shall see, to the equipotential lines and lines of flow in an infinite plate with two equal sources of electricity.

Maxwell, in 1856, suggested the application to problems of electric currents of his beautiful theory of the motion of an immaterial incompressible fluid in a resisting medium, but does not appear to have developed the suggestion.†

The object of this paper is to show that, by regarding, in accordance with Maxwell's suggestion, every point of exit or issue as a source or sink, spreading or absorbing electricity, independently of all other sources, Kirchhoff's general equations may be deduced by easy geometrical processes, and extended to certain cases of flow in curved surfaces. We shall, by this method, be naturally led to look mainly at the function u, which in the analytical investigation is subordinated to v. The equation $u = 0$ will receive an obvious physical interpretation, and we shall then proceed to consider in detail the nature of the flow in certain special cases apparently not yet examined.

If a source P, in an infinite uniformly resisting plate, steadily give forth a quantity of electricity E per unit of time, the flow per second over the whole circumference of all circles with P as centre is equal. Hence the rate of flow at each point of the circumference of such a circle is inversely as the radius $= \frac{E}{2\pi r}$. The potential due to P satisfies the equation

$$\frac{dv}{dr} = - \frac{E}{2\pi r},$$

or,

$$v = C - \frac{E}{2\pi} \log r.$$

* Camb. and Dub. Math. Journ. vol. i. p. 124.

† Cambridge Phil. Trans. vol x.

The potential due to any number of sources P_1, P_2, and sinks P_1' P_2', &c., all of equal power, is got by simple superposition. If E be equal for all points,

$$u = C - \Sigma \frac{E}{2\pi} \log r + \Sigma \frac{E}{2\pi} \log r',$$

where r corresponds to a source, and r' to a sink. Hence the equipotential lines are

$$\frac{r_1 \, r_2 \, r_3 \ldots}{r_1' \, r_2' \, r_3' \ldots} = C \quad . \quad . \quad . \quad . \quad (5).$$

The equation of the lines of flow follows at once from the equation of continuity. Across any element ds of a stream line subtending angles $d\theta_1$ $d\theta_2$, &c., at the sources, and $d\theta_1'$ $d\theta_2'$, &c. at sinks, no fluid must flow. But the quantity of fluid per second reaching ds from P_n is $\frac{d\theta_n}{2\pi}$ E. The quantity withdrawn by P'_n is $\frac{d\theta'_n}{2\pi}$ E. Hence the differential equation of the stream-line is

$$\Sigma d\theta - \Sigma d\theta' = 0 .$$

Integrating, $$\Sigma\theta - \Sigma\theta' = \text{const.}$$

where θ and θ' are the angles between radii vectores and any fixed lines. If we agree to reckon θ in opposite directions for sources and sinks, the equation becomes

$$\Sigma\theta = a \quad . \quad . \quad . \quad . \quad (6).$$

The following are elementary consequences of this equation :—

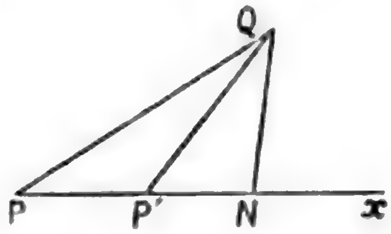
(*a.*) When we have one source P and one equal sink P′, the stream line through any point Q has for its equation

$$\Sigma\theta = QPP' + QP'P = \frac{\pi}{2} - PQP' = a.$$

Hence the locus of Q is a circle through P and P′, which is Kirchhoff's case. The orthogonals are circles whose centres (R) lie in PP′ produced, and whose radii $= \sqrt{PR.P'R}$.

(*b.*) If we have two equal sources and no sinks, or what is the

same thing, sinks at an infinite distance, the stream lines are rectangular hyperbolas. For in this case,

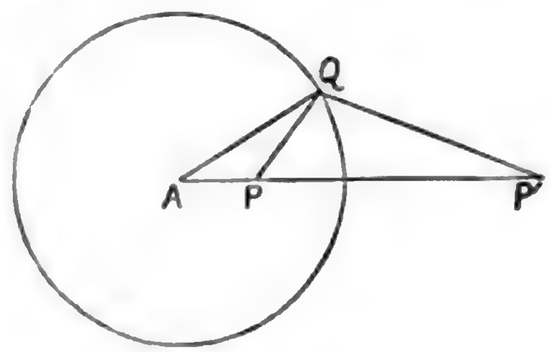


QPN + QP′N = a = QNx, if we make P′QN = QPN. Also QN touches the circle through PP′Q, therefore

$$QN^2 = NP' . NP$$

– the equation of a rectangular hyperbola through P and P′, whose centre is the middle point of PP′, and which is referred to conjugate diameters inclined at angle a. The orthogonal system in this case consists of the lemniscates $rr' = c$. One of the hyperbolas consists of the straight line PP′, and the line equidistant from P and P′. Dividing the plate along the latter line, we have the case of one source in a plate bounded in one direction by an infinite straight line, but otherwise unlimited or bounded by a lemniscate of infinite conductivity, having P and its image due to the boundary line for poles.

(*c.*) To find the image of any point in a circular boundary, *i.e.* to find the source which in combination with a source at the centre of the circle, and an equal sink at any other point, will make the circle a stream line.



Let A be the centre of the circle, and P the given sink. In AP take P′, so that AP.AP′ = AQ^2. Then PAQ and QAP′ are similar triangles, and QPA = AQP′.

Therefore QAP + QP′A + QPA = 2π, or (6) is satisfied for any point in the circle by assuming at P′ a sink = P.

(*d.*) Hence if there be within a circle m sources and n sinks, we

must assume the same number of sources and sinks without the circle, and $n-m$ sources at the centre.

(*e.*) The straight line equidistant from two equal sources of the same sign is clearly a stream line for these points. Hence the image of any point in a straight line is an equal point, which is its optical image.

I have constructed the equation

$$\Sigma\vartheta = a$$

on the assumption that all the sources are equal, because the degree of the stream line is equal to the number of equal sources (positive and negative) to which the system can be reduced. For if h, k be the co-ordinates of P, the equation becomes

$$\Sigma \pm \tan^{-1}\frac{y-k}{x-h} = \mathrm{C}, \quad . \quad . \quad . \quad (7).$$

If $\left(\frac{y-k}{h-x}\right)_m$ denote the sum of all the combinations of expressions $\pm\frac{y-k}{x-h}$, taken m at a time, we may write this

$$1 - \mathrm{C}\left(\frac{y-k}{x-h}\right)_1 - \left(\frac{y-k}{x-h}\right)_2 + \mathrm{C}\left(\frac{y-k}{x-h}\right)_3 + \left(\frac{y-k}{x-h}\right)_4 - \&\mathrm{c.} = 0 \quad (8),$$

an equation of the n^{th} degree if there be in all n sources.

The degree of the equipotential lines is also $= n$ if there be an equal number of sources and sinks. In general, if there be m sources of one sign, and $n-m$ of another, and $m > n-m$, $2m$ is the degree of the equipotential lines. This is one of many features which make it more convenient to work with stream lines.

It is obvious from equation (8), that every stream line must pass through all the sources. Thus, the circle in case (*c*), which passes through no source, is not a complete stream line, the other branch being the straight line APP′, which passes through all the sources. Distinct stream lines can intersect only at a source, for at no other point can $\Sigma\theta$ be indeterminate. Where two branches of the same stream line intersect the velocity is necessarily zero, changing sign in passing through the point. The physical meaning of a branch is that two streams impinge, and are thrown off with an abrupt change of direction.

The same result is easily found from the analytical condition for a singular point $\frac{du}{dx} = \frac{du}{dy} = 0$.

$$\text{For } -\frac{du}{dx} = \frac{dv}{dy} = \text{velocity parallel to axis of } y,$$

$$\frac{du}{dy} = \frac{dv}{dx} = \text{velocity parallel to axis of } x,$$

or directly by differentiation.

$$\left.\begin{aligned} \frac{du}{dx} &= \Sigma\left(\mp\frac{y-k}{r^2}\right) \\ \frac{du}{dy} &= \Sigma\left(\pm\frac{x-h}{r^2}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (9).$$

The nature of the intersection of the branches of a stream line at a multiple point is easily determined.

At an m-point, the angles at which the branches cut the axis of x are the roots of the equation—

$$\left(\frac{d}{dx} + \tan\phi\frac{d}{dy}\right)^m u = 0 \quad . \quad . \quad . \quad (10).$$

Where, since $\quad \frac{d^2u}{dx^2} = -\frac{d^2u}{dy^2}$

$$\frac{d^mu}{dx^m} = -\frac{d^mu}{dx^{m-2}dy^2} = \frac{d^mu}{dx^{m-4}dy^4} \;\&c.,$$

$$\frac{d^mu}{dx^{m-1}dy} = -\frac{d^mu}{dx^{m-3}dy^3} \;\&c.$$

Whence (10) becomes

$$\frac{d^mu}{dx^m}\left(1 - \frac{m\,.\,m-1}{1\,.\,2}\tan^2\phi + \&c.\right) +$$

$$\frac{d^mu}{dx^{m-1}dy}\left(m\tan\phi - \frac{m\,.\,m-1\,.\,m-2}{1\,.\,2\,.\,3}\tan^3\phi + \&c.\right) = 0.$$

We can choose the axes so that $\frac{d^mu}{dx^m} = 0$, and reduce the equation to

$$m\tan\phi - \frac{m\,.\,m-1\,.\,m-2}{1\,.\,2\,.\,3}\tan^3\phi + \ldots = 0 \quad . \quad (11),$$

or
$$\tan m\phi = 0 \quad . \quad . \quad . \quad (12),$$

$$\phi = \frac{l\pi}{m}, \text{ where } l \text{ is any integer from 1 to } m.$$

Thus the branches make equal angles with each other. This proposition depends solely on the relation $\nabla u = 0$. It is therefore true, also, for the equipotential lines, as is otherwise obvious.*

The general nature of the stream lines will be different, according as the number of sinks is or is not equal to the number of sources. In the former case, $\Sigma(\theta) = 0$ is satisfied at all points infinitely distant, the radii being all parallel, and the positive and negative angles equal in number. Hence one stream line has the straight line at infinity as a branch, or intersects the straight line at infinity at right angles, and therefore has an asymptote. This stream line will, in general, be of the $n-1^{\text{th}}$ degree. In some cases it may be of a lower degree; as, for example, when the conic at infinity is its other branch. A case of this sort will be given below. The other stream lines of the system cannot meet the line at infinity, and cannot have asymptotes. However far they run out, they must therefore loop and return.

When there are more sources than sinks, $\Sigma\theta$ becomes indeterminate at an infinite distance, as might have been anticipated from the fact, that in this case there is a constant flow of electricity outwards, implying a sink at an infinite distance. The line at infinity is not in this case a stream line, and will be cut by all the stream lines, which do not loop except at finite distances, and have all asymptotes.

The asymptotes, in this case, may be easily constructed by the aid of equations (6) and (8).

At the infinitely distant point of contact the velocities due to all sources are in the same direction, or the asymptote must be parallel to the radii.

If there are m sources and $n-m$ sinks, the stream line whose asymptote makes an angle α with the initial line is obviously

$$\Sigma\theta = (2m-n)\alpha = \tan^{-1} C \quad . \quad . \quad . \quad (13).$$

* I have since found that this result has been already proved for plane curves by Professor Rankine and Professor Stokes (Proc. R.S., 1867), and for spherical harmonics by Sir W. Thomson and Professor Tait, in their treatise on Natural Philosophy.

This equation has $2\ m-n$ roots.

$$\alpha_1,\ \alpha_1 + \frac{\pi}{2m-n},\ \alpha_1 + \frac{2\pi}{2m-n},\ \&c.$$

So that each stream line has $2m-n$ asymptotes equally inclined to one another.

Transforming to rectangular co-ordinates, and choosing the asymptote as axis of x, (8) reduces to

$$\left(\frac{y-k}{x-h}\right)_1 - \left(\frac{y-k}{x-h}\right)_2 + \quad . \quad . \quad = 0.$$

When $y = 0$, x has two roots $= \infty$ if

$$\Sigma(\pm k) = 0 \quad . \quad . \quad . \quad . \quad (14).$$

Hence the asymptote is such that the algebraic sum of the perpendiculars from the sources diminished by the sum of the perpendiculars from the sinks is zero. It is obvious without analysis that this condition is necessary, that the velocity perpendicular to the asymptote, at its point of contact with the curve, may be absolute zero. If sinks weigh upward, all lines passing through the centre of gravity of the system are asymptotes, and $2m-n$ of these lines, equally inclined to each other, belong to one stream line. The system must have a centre of gravity, for by pairing sources and sinks we produce couples which will always give a single resultant when compounded with the weights of the extra sources.

A complete system has no centre of gravity, but (14) is satisfied for all lines perpendicular to the axis of the resultant couple. If the axis of the couple formed by pairing a source and sink at distance ρ_m makes an angle ψ_m with the axis of the resultant couple

$$\Sigma(\rho \sin \psi) = 0 \quad . \quad . \quad . \quad (15),$$

an equation with only one root to determine the direction of the asymptote. In this case the asymptote meets the curve in a double point, and has contact of the third order, or x has three roots $= \infty$.

The condition for this is obviously—

$$\Sigma(\pm kh) = 0 \quad . \quad . \quad . \quad (16),$$

which since $\Sigma(\pm k) = 0$, does not depend on the point of the asymptote from which h is reckoned.

If (15) is satisfied identically, the asymptote meets the curve in

a triple point. Two of the branches belong to the line at infinity, and the finite branch sinks to the $n-2$ degree.

In this case not only $\Sigma(\pm k) = 0$, but $\Sigma(\pm h) = 0$. Hence (16) no longer gives a fixed point on the asymptote, but only fixes its direction. A further analytical condition is easily found, but is unnecessary. For in this case the centre of gravity of the sources coincides with the centre of gravity of the sinks. The stream lines due to the sources alone would have the same sets of asymptotes as those due to sinks. One of these sets is necessarily asymptotic in the complete system, which has always one line with real asymptotes. The set will consist of $\frac{n}{2}$ rays, all passing through the common centre of gravity of the sources and sinks, and equally inclined to one another.

Rectilineal Branches are asymptotes coinciding with their curves. Hence, in an incomplete system, all straight lines pass through the centre of gravity of the system, and belong to one stream line, unless the centre of gravity be a source. In any case they are equally inclined to one another, for if not branches of one stream line, they would be so for the system got by removing the source at their intersection.

In a complete system there can be only one rectilineal stream line, unless sinks and sources have a common centre of gravity. In the latter case, there can be at most $\frac{n}{2}$ straight lines, forming equally inclined rays through that point.

The condition for a rectilineal branch is in general that the sources must be either on the line or be two by two, each other's images on the line. For if not, remove all the sources on the line and all pairs of sources which are each other's images in the line. Next, remove all sources on one side of the line by placing equal sources of opposite sign at the place of their images The straight line is still a stream line, and on one side of it there are no sources, and therefore constant potential, which is absurd. Similarly it can be shown that a circle is a possible stream line only when the sources are on the curve or image each other. From this it follows that no finite number of sources can give parallel rectilineal streams or non-intersecting circular streams.

A similar investigation applies to equipotential lines. The image of a point in a rectilineal equipotential line is the same in position as the image in a stream line, but of opposite sign. No source can lie on an equipotential line. Hence, to show that for right equipotential line the points must image two by two, we have only to remove all sources on one side of the line, placing equal sources of the same sign at their images. The line is still equipotential, therefore we may suppose it charged to constant potential, and all sources removed. Hence all stream lines become rectilineal, which is absurd. Similarly if a circle is equipotential, the sources must balance about it two by two, *i.e.*, must be in a straight line with its centre, at distances to which the radius is mean proportional—otherwise we can find a system reducible to a single point at the centre of the circle, and in which all stream lines are rectilineal. Hence, no incomplete system can have a rectilineal or circular potential line.

Points of Inflexion occur at all points on the locus—

$$\frac{d^2u}{dx^2} + 2\frac{d^2u}{dxdy}\frac{dy}{dx} + \frac{d^2u}{dy^2}\cdot\left.\overline{\frac{dy}{dx}}\right|^2 = 0 \quad . \quad (17).$$

Remembering that

$$\frac{d^2u}{dx^2} = -\frac{d^2u}{dy^2} = \Sigma\left(\pm\frac{\sin 2\theta}{r^2}\right),$$

$$\frac{d^2u}{dxdy} = \Sigma\left(\mp\frac{\cos 2\theta}{r^2}\right),$$

we can readily bring (17) into the form—

$$\Sigma\left(\frac{\sin 2\theta}{r^2}\right)\Sigma\left(\frac{\cos(\theta+\theta')}{rr'}\right) - \Sigma\left(\frac{\cos 2\theta}{r^2}\right)\Sigma\left(\frac{\sin(\theta+\theta')}{rr'}\right) = 0.$$

or,

$$\Sigma\,\frac{\sin(2\theta-\theta'-\theta'')}{r^2r'r''} = 0 \quad . \quad . \quad . \quad (18).$$

In this last expression θ' and θ'' may assume the value θ.

The radius of curvature may be similarly expressed, but such expressions can hardly have a practical application.

The cases of practical interest are mainly those where the number of sources is small. We have already examined the cases of two

sources of the same or opposite signs. We will now proceed to consider the cases that arise when there are three or four sources.

Three Sources.—In general the curves will be cubic passing through the three sources, and having asymptotes determined as above. The direction of flow at any point of the field may be found by observing that if ϕ be the angle between the tangent and a radius vector,

$$\Sigma \pm \frac{\sin \phi}{r} = 0 .$$

It will sometimes be possible to find the direction of flow geometrically by the following obvious theorem.

If a circle be described touching a stream line at any point, and cutting off from the radii vectores of that point, fractions of their lengths, μ μ', &c., where μ is negative if the point of intersection is in the radius vector produced, and also negative if the radius vector is drawn from a sink, then—

$$\Sigma (\mu) = 0 .$$

When the number of sources is large this theorem is not in general convenient, but it is often applicable where there are only three points.

The lines of flow can, however, be readily described with any degree of accuracy when there is one sink, by describing segments of circles with constant difference of angle through the sink and one source, and drawing through the other source straight lines with the same difference of angle. The stream lines will be diagonals of the quadrilaterals into which the field is thus divided. The process may be extended to the case of two sources and two sinks by taking the intersections of two sets of circles.

When there are two sources and one sink, the singular points may be found by an easy geometrical method. Let A, B, be sources, C the sink, and P a point of zero velocity. The resultant velocity due to A and C is in the tangent to the circle PAC, and also—since P is a singular point—in the line PB. Therefore—

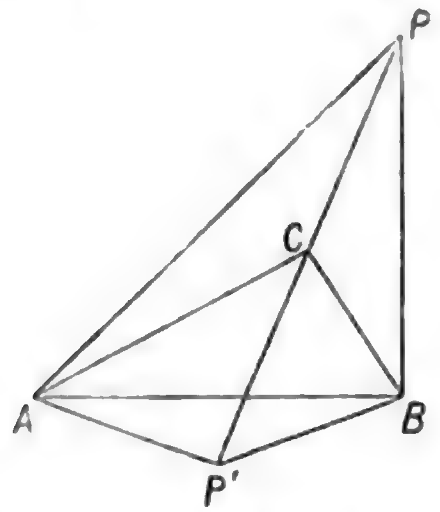
$$BPC = PAC .$$

Similarly

$$APC = PBC .$$

Hence PCA, BCP are similar triangles, and there are two points

of zero flow, P and P′, lying in the line bisecting the angle C, and such that PC is a mean proportional to BC and AC. The directions



of the orthogonal branches at P bisect the angle APB and its supplement.

For the initial line is a tangent at the singular points if

$$\frac{d^2u}{dx^2} = \Sigma\left(\pm\frac{\sin 2\theta}{r^2}\right) = 0 \quad . \quad . \quad (19).$$

Let now APC $= \alpha$, BPC $= \frac{C}{2} - \alpha = \beta$, and assume the bisector of APB as initial line. Then

$$\sin\frac{C}{2}\left(\frac{1}{PA^2} - \frac{1}{PB^2}\right) + \frac{\sin\overline{\alpha-\beta}}{PC^2} = \Sigma\frac{\sin 2\theta}{r^2},$$

which since

$$\left.\begin{aligned}\frac{1}{PA} &= \frac{1}{PC^2}\cdot\frac{\sin^2\beta}{\sin^2\frac{C}{2}}\\ \frac{1}{PB^2} &= \frac{1}{PC^2}\cdot\frac{\sin^2\alpha}{\sin^2\frac{C}{2}}\end{aligned}\right\}$$

becomes,

$$\sin^2\beta - \sin^2\alpha - \sin\overline{\alpha-\beta}\,.\,\sin\overline{\alpha+\beta} = 0,$$

which satisfies (20).

The chief interest lies in the cases where the cubic breaks up into a straight line and a conic. This takes place for one stream line of the system when all the sources lie on a straight line, or when they form an isosceles triangle with points of the same sign at the base. The cases are—

1. *Two Sources and a Sink.*—The conic is always a *circle* with the sink as centre. If the sink lies in the line of the sources produced, the radius of the circle is a mean proportional to the distances of the sink from the sources. If the sink lie between the sources, the circle is impossible. If the sink is the vertex of an isosceles triangle, the circle passes through both sources, and all asymptotes meet in the point of zero flow furthest from the sources. If the sink is half way between the sources, there are two straight lines and a real and impossible circle.

2. *Three Sources of the same Sign.*—Every stream line has three asymptotes, meeting in the centre of gravity, and inclined at angles of $\frac{\pi}{3}$. If one of these asymptotes becomes a branch, the other branch is a hyperbola, with centre of gravity as centre, and axes in ratio of $\sqrt{3}$ to 1. If the points form an isosceles triangle, the hyperbola passes through the extremities of the base. If the triangle is equilateral, the hyperbola coincides with its asymptotes. If the vertical angle is less than $\frac{\pi}{3}$, the rectilineal branch is the transverse axis; if greater than $\frac{\pi}{3}$, it is the conjugate. If the points are all in a line, the vertices of the hyperbola lie on that line, and are the points of zero flow, which are easily found. If one point is half way between the other two, we have two rectilineal branches and two hyperbolas, the conjugate axis of the one being equal to the transverse axis of the other. The hyperbolas are, therefore, confocal.

Four Points.—Complete System.

Singular Points.—If A and B are sources, C and D sinks, there is a singular point at P, if the circles APC, BPD, and also APD, BPC touch at P. Hence, there are no real singular points if the sides of the quadrilateral ACBD intersect, unless all the points be on a circle, which in this case contains all the singular points.

Straight Lines.—The one stream line which has an asymptote is of the third degree. If a straight line is one factor, the other factor is a conic, which is always a circle. For if A, C are the images of B, D respectively in the straight line, a circle can be

drawn through them, which is obviously the branch sought. But if A, B lie without the line, and C, D on it, a circle through A, B having its centre O in CD produced, so that OA is a mean proportional between OC and OD is the circle required. If ABCD are all on a straight line, the other branch is manifestly a circle with centre on the line.

Conics.—The parabola is an impossible conic for any finite number of points. For the parabola has two asymptotes meeting at infinity. Hence the centre of gravity of an incomplete system, or of the sinks and sources separately in a complete system, must be at an infinite distance, which is absurd. The conics are therefore central.

The *hyperbola*, which has two asymptotes, is only possible when the cubic reduces to a conic. This demands that the centre of gravity of sinks and sources shall coincide, *i.e.*, that AB, CD are diagonals of a parallelogram. The asymptotes must meet at right angles, and the hyperbola is equilateral. It is obvious, indeed, that in this case the sources and sinks give separately sets of concentric rectangular hyperbolas, of which the one passing through the four points belongs to both sets, and is the only asymptotic curve of the complete system.

In this case the equipotential lines are lemniscates. Let the origin be the centre of the system, $2a$ and $2b$ the diagonals of the parallelogram, α and β their angles with the initial line. At any point P

$$AP^2 \,.\, BP^2 + \lambda CP^2 \,.\, DP^2 = 0 \,.$$

That is,

$$r^4 + a^4 - 2a^2r^2 \cos 2\,\overline{\theta - \alpha} + \lambda(r^4 + b^4 - 2b^2r^2 \cos 2\,\overline{\theta - \beta}) = 0$$

$$(1 + \lambda)(r^4 + a^4) - 2r^2 \cos 2\theta\,(a^2 \cos 2\alpha + \lambda b^2 \cos 2\beta)$$
$$+\, 2r^2 \sin 2\theta\,(a^2 \sin 2\alpha + \lambda b^2 \sin 2\beta) = 0 \,.$$

When $$\lambda = -\frac{a^2 \sin 2\alpha}{b^2 \sin 2\beta}\,,$$ the curve becomes

$$(b^2 \sin 2\beta - a^2 \sin 2\alpha)(r^4 + a^4) - 2a^2b^2r^2 \sin 2(\beta - \alpha) \cos 2\theta = 0 \,,$$

a lemniscate, with foci on the initial line, and centre at the origin.

If the parallelogram is a rectangle $a = b$, and the curve is

$$r^4 - 2a^2r^2 \frac{\cos \overline{\beta - \alpha}}{\cos \overline{\beta + \alpha}} \cos 2\theta + a^4 = 0 \,.$$

It is easily shown that the stream lines orthogonal to these are lemniscates with the same centre, passing through the four points, one of which becomes a circle when the parallelogram is rectangular.

The ellipse appears to be an impossible conic for four points, for conics occur in pairs orthogonal to each other. The orthogonal of the ellipse must be a confocal hyperbola, which is impossible, the only hyperbola being that discussed above. Orthogonal circles, however, are possible, and fall under two classes, according as all the points are on one circle, or two on each.

If ABCD lie on a circle, that circle is obviously a stream line. Let BA.DC produced meet in O. Then $OA.OB = OC.OD$, and the circle, with centre O and radius $\sqrt{OA.OB}$ is the other branch of the stream line. If O lies within the circle ABCD, the second circle becomes impossible. If CA.BD produced meet R and CB.AD in S, R and S are centres of equipotential circles, only one of which is real, unless the second stream circle is imaginary. We may take as an example the case of a rectangle, points of the same sign lying on the same diagonal. Let the circle through the four points be ($2a$ and $2b$ being the sides of the rectangle)

$$x^2 + y^2 - a^2 - b^2 = 0\,.$$

The other branch is the imaginary circle

$$x^2 + y^2 + a^2 + b^2 = 0\,;$$

and we know that another stream line is the hyperbola

$$y^2 - x^2 - a^2 + b^2 = 0\,.$$

Hence the stream lines are

$$(x^2 + y^2)^2 - (a^2 + b^2)^2 + \lambda(y^2 - x^2 - a^2 + b^2) = 0\,,$$

lemniscates as above.

The equipotential circles degenerate into the straight lines

$$x = 0 \quad \text{and} \quad y = 0\,.$$

If O be the point in CD produced which is equidistant from A and B, and $OC.OD = OA^2 = OB^2$, the circle with O as centre passing through A,B is a line of flow.

The circle having its centre P in AB produced, and passing through CD, is obviously orthogonal; and since $PA.PB = PC^2$

$= PD^2$ is also a line of flow. In this case both circles are necessarily real.

It is clearly impossible that the same system should have two pairs of circular stream lines of either of the classes we have analysed. Nor can two complete pairs of different classes occur, since otherwise two stream lines would intersect. But three real and an imaginary circle are possible, if ABCD lie on a circle, and at the same time obey the condition for a pair of circles of the second class, that is, if AB produced pass through the pole of CD with respect to the circle ABCD. The three circles are manifestly orthogonal, and their radical centre is centre of the fourth (imaginary) circle.

If the circle through ABCD is

$$S = x^2 + y^2 - a^2 = 0,$$

the lines AB, CD respectively

$$u = hx + ky - a^2 = 0$$
$$v = h'x + k'y - a^2 = 0,$$

we have

$$hh' + kk' - a^2 = 0,$$

and the second and third circles become

$$S - 2u = 0$$
$$S - 2v = 0.$$

The fourth or imaginary circle is

$$S - 2w = 0,$$

where

$$w = \frac{a^2(k'-k)x}{hk'-kh'} + \frac{a^2(h-h')y}{hk'-kh'} - a^2,$$

$w = 0$ representing the polar of the intersection of AB, CD.

Thus the equation to the stream lines may be written

$$(S - 2u)(S - 2v) + \lambda S(S - 2w) = 0,$$

or,

$$(1 + \lambda)S^2 - 2(u + v + \lambda w)S + 4uv = 0,$$

which degenerates into a cubic when $\lambda = -1$.

The equations may, in general, be simplified by a proper choice of co-ordinates.

Take, for example, the case when $S-2u$, $S-2v$ are equal circles.

Then $$h^2 + k^2 = h'^2 + k'^2 ,$$

and by proper choice of axes,

$$\begin{aligned} h &= -h' \\ k &= k' \\ k^2 - h^2 &= a^2 . \end{aligned}$$

Hence,

$$w = \frac{a^2 y}{k} - a^2 .$$

The lines become

$$(1+\lambda)S^2 - 2\left(2ky - 2a^2 + \frac{\lambda a^2 y}{k} - \lambda a^2\right)S + 4(ky - a^2)^2 - 4h^2x^2 = 0.$$

If the three circles are equal, we have further,

$$\begin{aligned} h^2 + k^2 &= 2a^2 \\ h &= \frac{a}{\sqrt{2}} \\ k &= \frac{\sqrt{3}a}{\sqrt{2}} \end{aligned}$$

Accurate drawings of this case, and of the lemniscates in the case of a rectangular parallelogram, have been prepared, to accompany this paper, by Messrs Meik and Brebner, in the Physical Laboratory of the University. The dotted lines in these diagrams show the lines of flow when the signs of a source and sink are transposed.*

Verifications have been sought by determining equipotential lines experimentally, and superposing them upon drawings of the stream lines. The experiments were executed by students in the Physical Laboratory. The process employed was essentially that of Kirchhoff, but the use of Thomson's galvanometers has made it much more rapid, as well as more delicate.

Spherical Surfaces.—To extend the method above used to spheri-

* That a greater variety of curves might be given, without overcrowding the figure, the two sides of one of the diagrams have been made unsymmetrical, some of the curves being given (in half) on the one side, others on the other.

cal surfaces, we must take as starting point, not a single source, but a source and sink at the extremities of a diameter. For brevity, we shall speak only of the source, assuming the existence of a corresponding sink.

When there is one source, the stream lines are manifestly great circles through it, and the equipotential lines small circles, of which it is the pole.

If the radius of the sphere is a, the circumference of the small circle, whose angular radius is θ, is $2\pi a \sin\theta$. Hence if u be the potential,

$$\frac{du}{d\theta} \propto \frac{1}{\sin\theta}$$

$$u \propto \frac{1}{2}\log\frac{1-\cos\theta}{1+\cos\theta}$$

For any number of sources the potential will be

$$\frac{1}{2}\left(\Sigma \pm \log\frac{1-\cos\theta}{1+\cos\theta}\right),$$

and the equation of the equipotential lines,

$$\frac{1-\cos\theta_1}{1+\cos\theta_1}\cdot\frac{1-\cos\theta_2}{1+\cos\theta_2}\cdots = C\,\frac{1-\cos\theta'_1}{1+\cos\theta'_1}\cdot\frac{1-\cos\theta'_2}{1+\cos\theta'_2}\cdots,$$

the accented angles belonging to sinks.

For the lines of flow we have, precisely as in a plane, $\Sigma(\pm\,\phi)=c$, where ϕ is the angle between the great circle through a source and a point on the line, and a fixed great circle through the source.

Let us take, as an example, the case of one source and one sink. Let the co-ordinates of these points be $h, k, 0$; $h, -k, 0$, and those of any point on an equipotential line, x, y, z.

We have for the equation of this line,

$$\frac{1-\cos\theta}{1+\cos\theta} + \lambda\,\frac{1-\cos\theta'}{1+\cos\theta'} = 0,$$

where

$$\cos\theta = \frac{hx+ky}{a^2},\ \cos\theta' = \frac{hx-ky}{a^2}.$$

Hence the projections of the equipotential lines on the plane of xy have as equation,

$$(a^2-hx-ky)(a^2+hx-ky)+\lambda(a^2-hx+ky)(a^2+hx+ky)=0,$$

or—

$$a^4 + k^2y^2 - h^2x^2 - 2\,\frac{1-\lambda}{1+\lambda}\,a^2ky = 0$$

—a series of similar hyperbolas, whose centres lie on the axis of y, whose axes are parallel to the co-ordinate axes, and inversely proportional to the co-ordinates of the source, and which all cut the axis of x at points distant $\pm\frac{a^2}{h}$ from the origin. Obviously one of the lines is the great circle perpendicular to the line joining the sources.

For the stream lines we have in this case,

$$\phi - \phi' = c,$$

observing that

$$\tan\phi = \frac{az}{xk - hy}$$

$$\tan\phi' = \frac{-az}{xk + hy}.$$

This equation becomes

$$k^2x^2 - h^2y^2 - a^2z^2 + \lambda xz = 0,$$

a cone which intersects the tangent plane to the sphere at the extremity of the axis of x, in a series of similar ellipses, having their centres on the intersection of the plane with the plane of xz, and passing through the points $a, \pm\frac{ak}{h}, 0$. Two of the stream lines are manifestly great circles, whose equations are $x = 0$ and $z = 0$.

If we divide the sphere along the former of these circles, we cut off the subsidiary source and sink, and get the case of a hemisphere, in which the source and sink are equidistant from the pole. A curious hemispherical case is got by dividing the sphere along the equipotential hemisphere. In this case we have two sources of the same sign within the hemisphere, one being the subsidiary source of the removed sink. But in order that the distribution may remain unchanged, we must have the potential maintained constant at the edge of the hemisphere. This may be effected by making the base a conductor with a sink at its centre, or, indeed,

by placing the sink at the vertex of any conducting surface of revolution which joins the hemisphere. From these hemisphere cases, obvious cases of half and quarter hemispheres follow.

4. On the Kombi Arrow-Poison (*Strophanthus hispidus*, DC.) of the Manganja district of Africa. By Dr Thomas R. Fraser.

(*Abstract.*)

In nearly every narrative of exploration in uncivilised tropical regions, accounts are given, often no doubt somewhat fanciful, of poisonous substances which are said to possess the most remarkable properties. Usually these poisons are of vegetable origin; and the great majority may be included in the two divisions of *ordeal* and of *arrow* poisons, according as they are applied to one or other of these purposes. Among the most remarkable of the *ordeal-poisons* are the *Tanghinia venifera* of Madagascar, the *Physostigma venenosum* of Old Calabar, and the Akazga poison of the Gaboon; and of the *arrow-poisons*, the famous Curara or Wourali of South America, and the *Antiaris toxicaria* of Java.

The examination of these substances has not only proved of great value to physiology, but practical medicine has likewise been benefited—one of them, at least, being now an important medicinal agent.

In bringing before the Society a few of the results of a recent examination of a new arrow-poison, the author has to express his gratitude to the President, who very kindly gave him the specimens of poison with which the experiments have been made. These specimens, consisting of a number of ripe follicles, were sent to Dr Christison by Mr Walker, and were collected in the expedition of the late Bishop M'Kenzie.

Several specimens of the poison have likewise been sent to Professor Sharpey by Dr Kirk, H.M. consul at Zanzibar. Dr Kirk says "that the plant is a woody climber, growing in the forest, both of the valley and hills, and found at various places between the coast and the centre of the continent, above the Victoria Falls of the Zambesi. The stem is several inches in diameter, and rough outside. The plant climbs up the highest trees, and hangs from

one to the other like a bush vine. The flowers are of a pale yellow, and last for but a short time during the months preceding the first rains of the season (October and November). The fruit is ripe in June, and collected by the natives, who separate the rough outer coat before drying it, preserving the more leathery inner covering and the seeds."*

Dr Livingstone gives some interesting information regarding the poison in his "Narrative of an Expedition to the Zambesi and its Tributaries." He mentions that arrows poisoned with it are used for killing wild animals only; arrows destined for the more noble object of killing men being poisoned with the entrails of a small caterpillar. Dr Livingstone says that in hunting, the natives follow the game with great perseverance and cunning:—"The arrow, making no noise, the herd is followed until the poison takes effect, and the wounded animal falls out; it is then patiently watched till it drops; a portion of meat round the wound is cut away, and all the rest eaten" (p. 465).

Dr Livingstone also says that the poisoned arrows are made in two pieces. "An iron barb is firmly fastened to one end of a small wand of wood, ten inches or a foot long, the other end of which, fined down to a long point, is nicely fitted, though not otherwise secured, in the hollow of the reed which forms the arrow-shaft. The wood immediately below the iron head is smeared with the poison. When the arrow is shot into an animal, the reed either falls to the ground at once, or is very soon brushed off by the bushes; but the iron barb and poisoned upper part of the wood remain in the wound. If made in one piece, the arrow would often be torn out, head and all, by the long shaft catching in the underwood, and striking against trees" (p. 466).

The follicles examined by the author vary in *length* from about nine and three-fourths, to about twelve and one-fourth inches, and in greatest *thickness* from about one inch to three-fourths of an inch, and they vary in *weight* from about 130 to 330 grains. They contain from 100 to 200 seeds, each of which weighs about half a grain, and has attached to it a beautiful comose appendix, placed on an extremely brittle stalk. For the identification of the plant the author is indebted to Professor Oliver of Kew, who writes, in a letter

* Extract from letter to Professor Sharpey, dated January 1, 1864.

dated 10th Dec. 1869,—"I reopen your note to say that I have just dissected a flower, and conclude to name the Kombi plant *Strophanthus hispidus*, DC." This plant belongs to the natural order *Apocynaceæ*.

When the seeds contained in these follicles are bruised and treated in a percolator with rectified spirit, a greenish yellow tincture is obtained. By distilling off the greater part of the spirit, and drying the residue on a water bath, and in the exhausted receiver of an air-pump, an extract is procured which weighs about 25 per cent. of the seeds employed, has an intensely bitter taste, and contains about one half of its weight of an inert fixed oil. From this extract the author has succeeded in separating a very powerful active principle.

As, however, the greater number of the experiments have been made with the extract, the results of these experiments only will be described in the following brief account of the physiological action of the Kombi arrow-poison, it being understood that the action of the active principle is of the same character.

When a small dose ($\frac{1}{20}$th of a grain) of this extract is mixed with a few minims of water, and injected under the skin of a frog, no distinct symptom is seen until about half an hour, when the animal's movements become somewhat sluggish. Soon afterwards the respirations cease, some stiffness occurs in the thoracic extremities, reflex sensibility diminishes, some stiffness appears in the pelvic extremities, and in about two hours after the administration, voluntary movements entirely cease, and strong galvanic irritation produces no effect, even when applied to exposed muscles and nerves. An examination of the heart shows that it is completely paralysed, the ventricles being pale and contracted, while the auricles are dark and distended.

It was obviously suggested by these phenomena that this substance acts as a cardiac poison; and, accordingly, some experiments were made in which the heart was exposed before the administration, of which the following is an example:—

One-tenth of a grain of extract was injected under the skin of a frog. Five minutes thereafter, it was observed that the ventricular systole was somewhat prolonged; in six minutes, the ventricular diastole was imperfect, so that only portions of the ventricle dilated

to admit blood from the auricles; in six minutes and thirty seconds, the greater portion of the ventricle was continuously pale and contracted, each auricular systole propelling merely a small drop of blood into the ventricle, where it produced a dark, pouch-like projection, which at times disappeared, and at other times only changed its position during the imperfect systole of the ventricle; in seven minutes, the ventricle altogether ceased to contract, while the movements of the auricles continued at nearly the normal rate; and in eighteen minutes, the auricles in their turn became motionless, but, in place of being contracted and empty like the ventricle, they were distended and full of dark blood. Notwithstanding this absolute paralysis of the heart, respiratory movements occurred for thirty-five minutes after the ventricle had ceased to contract, and the frog jumped about actively for some time after this.

The experiments that have been performed with birds and mammals have likewise shown that this poison acts primarily on the heart.

An endeavour was made to ascertain by what mode of action these very peculiar cardiac effects are produced. With this object experiments were made, in which the cerebro-spinal axis was completely destroyed, in which the vagi nerves were divided, and in which the peripheral terminations of the vagi were paralysed by atropia, previously to the exhibition of the Kombi poison; but no important modifications were thereby caused, and it is therefore obvious that the action on the heart is not exerted through the cerebro-spinal nerves. In other experiments, after complete cardiac paralysis, the surface of the heart was irritated by galvanic and other stimulants, but no effect was thereby caused.

Another very prominent action of this poison is that exerted on the voluntary muscles, by which their activity is gradually impaired, and finally completely destroyed, so that the muscles are quickly in a condition of true *rigor mortis*.

Regarding the other physiological effects, it is sufficient briefly to mention that the sensory and motor spinal nerves, the abdominal and cervical sympathetics, and the muscular walls of the stomach, intestines, bladder, and uterus, are paralysed at an early stage, although not until the blood-heart has ceased to contract; while

the lymph-hearts of the frog maintain a normal rate long after paralysis of the blood-heart.*

From these results it is apparent, that the primary action of the Kombi arrow-poison is isolated in the heart, and that it may therefore be included in the class of the *cardiac poisons,*—a class of poisons whose action has been most accurately defined by the researches of Kolliker, Vulpian, Pélikan, Hammond and Weir Mitchell, Hilton Fagge and Stevenson, Holme, Dibkowsky, and others.

5. On Thebo-lactic Acid. By J. Y. Buchanan, M.A.

Thebo-lactic acid was discovered in Turkey opium by Messrs T. & H. Smith, the eminent morphia manufacturers of this city. It was examined by Stenhouse, and found to have the same composition as lactic acid, from which, however, it was supposed by the Messrs Smith to differ in the crystalline form of its copper and morphia salts. At present we are acquainted with three isomeric lactic acids, two of them differing from each other chemically, whilst the third is distinguished by its power of rotating the plane of polarisation of light. The last named acid, having been but recently† discovered, it is impossible to say whether it possesses any decidedly distinctive chemical properties or not. The other two, namely, the ordinary or ethyliden—and the ethylen-lactic acids, possess perfectly distinct chemical properties, determined by the different relative position in each of the alcoholic hydroxyl. The following rational formulæ express the different constitution of the two acids:—

CH_3	CH_2OH
$CHOH$	CH_2
$COOH$	$COOH$
Ordinary lactic acid.	Ethylen-lactic acid.

They may be distinguished at once by replacing in each the alcoholic hydroxyl by chlorine. We thus obtain from ordinary lactic acid the so-called α-, from ethylen-lactic acid, the β- chloro-

* The author is indebted to Professor Sharpey of London for an account of some experiments made with this poison in 1862. The results mentioned in the above abstract harmonise in the most satisfactory manner with those obtained by Professor Sharpey.

† Berichte der Deutschen Chem. Ges. 1869, 620.

propionic acid. These two bodies possess such different properties, that they may be at once and with certainty recognised.

The task, therefore, which I set myself, was, by the assistance of the chlorinated acid, to determine the position in the molecule of the alcoholic hydroxyl. Thebo-lactate of lime, dried at 150°, was treated with perchloride of phosphorus in the proportion of two molecules of the latter to one of the former. This mixture was heated in a retort, attached to the lower end of a Liebig's condenser, until the disengagement of hydrochloric acid ceased, when the condenser was reversed and the volatile products distilled off. By this means the decomposition is so complete that the residue, consisting of chloride of calcium, may be heated until the glass of the retort softens without carbonising to any very sensible extent. The distillate was separated by rectification up to 111° into a residue, which did not distil without partial decomposition, and a distillate. The latter was treated with the necessary precautions* with water, to obtain the chlorinated acid, and the former with absolute alcohol, to obtain its ether.

The acid thus obtained possessed all the properties of that formed from ordinary lactic acid. A chlorine determination gave 32·95 per cent. chlorine. The theoretical amount calculated from the formula $C_3H_5ClO_2$ is 32·72. Its specific gravity is 1·27, against 1·28 found for the acid derived from ordinary lactic acid. It passed entirely between 185° and 186°; the boiling point of α- chloropropionic acid is 186°. The two acids have also the same outward appearance, being colourless, uncrystallisable liquids, possessing the same smell, and exercising the same corrosive action on the skin, unaccompanied by pain or blisters.

The ether also possesses exactly the same properties as that prepared from ordinary lactic acid. A chlorine determination gave 26·34 instead of 26·01 per cent. demanded by the formula $C_5H_9ClO_2$. They both boil at 144°, and have the same smell; they are also both formed with great ease by heating their acids with alcohol and sulphuric acid.

It is thus evident that the chlorinated acids obtained by the same means from the two acids under comparison are identical. The chlorine, therefore, in both cases, is united to the same carbon atom,

* Compt. Rend. lxvi. 1157.

and consequently the acids, in which this chlorine is replaced by hydroxyl, have this last named group attached to the same carbon atom, and are therefore identical.

It is proper to mention that all the above experiments on thebo-lactic acid were repeated with ordinary lactic acid, and with uniformly identical results.

The copper salts of thebo-lactic and of ordinary lactic acids were prepared side by side, as nearly as possible under the same conditions, and in similar vessels, and on comparing the two salts, it was impossible to detect the slightest difference in their crystalline form. The free acid in concentrated solution produced no effect on the plane of polarisation of light.

I am engaged at present on the further comparison of the acids, and hope to have the honour of communicating my results to the Society on a future occasion.

In concluding, I take this opportunity of expressing my best thanks to the Messrs Smith, who in the most liberal manner placed at my disposal a large quantity of perfectly pure thebo-lactate of lime.

6. On the Bones of a Seal found in Red Clay near Grangemouth, with Remarks on the Species. By Professor Turner.

Towards the end of last autumn, one of my pupils, Mr William Stirling, B.Sc., requested me to determine some bones which had been found whilst sinking a new shaft for a pit in the Grangemouth coal-field. On examination, I found these bones to be the two halves of the lower jaw, a fragment of the upper jaw with some loose teeth, the right temporal bone, the atlas with fragments of other vertebræ, the glenoid part of the left scapula, the right astragalus and femur, and small fragments of other bones of the skeleton of a seal. The animal had not reached the adult state, for the epiphyses of the femur were not united to the shaft. The bones were imbedded in a stiff red clay.

Early in the present year, I was informed by Mr Stirling, the manager of the Grangemouth collieries, that Mr Burns, of Glasgow, had obtained some seal's bones from the same locality, and had exhibited them at a recent meeting of the Geological Society of Glas-

gow. Through the courtesy of Mr Geikie and Mr Croll of the Geological Survey, I have had the opportunity of examining the bones obtained by Mr Burns, which undoubtedly formed a part of the skeleton of the animal, some of the bones of which Mr Stirling had previously given to me, for I found amongst them the missing condyloid epiphysis of the right femur. These consist of one of the cervical, and of fragments of other vertebræ, of portions of the ribs, of the left occipital condyle, of a portion of the innominate bone and acetabulum, and of digital bones, more especially the terminal phalanges.

On a visit to the locality a few weeks ago, Mr Stirling gave me the following particulars :—

In the summer of last year a new shaft was sunk on Towncroft Farm, Grangemouth, to reach the coal in that district.* In the course of the operations the following strata were bored through :—

	ft.	in.
Surface soil,	4	0
Gravel sand,	0	9
Blue mud and sand,	16	0
Channel bed,	4	0
Sand and water,	8	0
Red clay mixed with sand,	11	0
Pure red clay,	36	0
Soft blue till,	38	0
Red sand,	1	0
Blue till,	5	0
Sand,	1	0
Hard blue till,	31	0
	155	9

The hard blue till lies on the rock in which the coal occurs.

* In a paper read before the Geological Society of Edinburgh, May 1869, and published in their Transactions, Mr Jas. Croll has given an account of the geology of this district; and in a paper read before the Geological Society of Glasgow, April 2, 1868 (Transactions, iii. p. 133), Mr Jas. Bennie has recorded the results obtained in the course of "boring" operations in the valley of the Clyde near Bowling, the haugh of Balmore, the valley of the Kelvin, and round by the south-eastern end of the Campsie Hills into the valley of the Forth, near Grangemouth, which reveal that "a great deep hollow stretched from sea to sea, fairly splitting Scotland in twain."

Whilst removing the blue mud and sand, superficial to the channel bed, the lower end of the left humerus of a large red deer was met with.

The seal's bones were found near the bottom of the pure red clay, at a depth of nearly 80 feet from the present surface of the soil, and nearly 68 feet below the present sea-level. The shaft of the pit is 530 yards distant from the Carron river to the south, and 1680 yards from the estuary of the Forth on the east.

That bones of a species of seal have occasionally been found imbedded in clay, in the middle district of Scotland, is a fact well known to naturalists. But the relations which these bones had to the surface, and to the present sea-level, differ in some important particulars from those of the Grangemouth seal.

In 1825, Dr Knox* directed attention to the bones of a seal found near Camelon, in a bed of clay 90 feet above the present level of the Forth. Dr David Page described† and presented to the Museum of Natural History in this city the almost perfect "skeleton of a seal, found in the Pleistocene clays of Stratheden," 150 feet above the present sea-level, about 16 feet from the surface of the soil, and about 5 miles inland from the influence of the tides.‡ Dr Allman on two occasions § exhibited to this Society bones of a seal—in the one instance, obtained from the Tyrie clay-field, Kirkcaldy, 30 feet above the present sea-level, 18 or 19 feet from the surface of the soil, and a quarter of a mile from the shore of the Forth; in the other instance, from the clay-field at Portobello, about 20 feet above the present high-water level, and about 15 feet below the surface of

* Memoirs of Wernerian Society, v. 572.

† Proc. British Association, Sept. 1858.

‡ Since my paper was read to the Royal Society, Dr Page has informed me that he obtained a second young seal's skeleton from the Stratheden clay, which is now in the Museum of Natural History, St Andrews. Nearly perfect skeletons of the surf and eider ducks, *Oidema* and *Somateria*, were found in the same clay. Dr Page also tells me that he has obtained seal's bones from the brick clays at Garbridge and Seafield, near St Andrews; from a brick-field at Dunbar; and from brick clay at Invernetty, Aberdeenshire. These clays are in the same horizon as the Stratheden clay. I find also that the skeleton of the young seal, in the St Andrew's Museum, has been carefully described by Mr R. Walker (*Annals and Magazine of Natural History*, Nov. 1863). He shows clearly that it is not *Callocephalus vitulinus*, and he considers it to be a young individual of *P. groenlandicus*. I have not yet examined this specimen.

§ Proc. Roy. Soc. Edinburgh, April 19, 1858, and March 21, 1859.

the soil. The Rev. Thomas Brown showed to this Society* portions of the skeleton of a seal, obtained from a brick-field at Errol, 45 feet above the present sea-level, and about 1½ mile from the estuary of the Tay. The bones were well imbedded in the brick clay, which also contained shells such as are now found in the polar seas, and which testify to the arctic rigour of the climate at the time when the clay was deposited.

As to the character of the clay in which the bones of the Grangemouth seal were found, Mr Peach, who has surveyed the district, and Mr Geikie, and Mr Croll, pronounce it to have been deposited under decidedly arctic conditions. Mr Peach also tells me that the Grangemouth clay is continuous with that at Camelon, near Falkirk, where the seal's bones which Dr Knox examined were found, and that it possesses the same characters as the Stratheden clay, in which lay the skeleton of the seal described by Dr Page.

Mr David Robertson of Glasgow has also examined the Grangemouth red clay with reference to the occurrence in it of minute organisms. He reports that he has found two species of Foraminifera, *Polymorphina compressa* (D'Orb) and *Nonionina asterizans* (F. & M.), and one species of Ostracoda, *Cytheropteron montrosiensi*. This Ostracod Mr Robertson states to be common in the brick clays of Annochie, Dryleys, Errol, Elie, and Bannie on the east of Scotland, which deposits contain arctic shells not now living on the British coasts.

Mr Bennie also informs me that Mr Robertson has obtained from the muddy sand and fine sandy clay which overlie the Grangemouth pure red clay, fragments of shells, the *Tellina balthica*, a shell which, Mr Jeffreys states, agrees exactly with similar fragments found by Professor Lilljeborg at Upsala. No fragments of shells have as yet been found in the red clay itself. The geological evidence is in favour of the view that the Grangemouth clay is glacial, and belongs to the same class as other undoubtedly glacial clays on the east coast of Scotland. The difference in the relation to the present sea-level between the Grangemouth clay and the other clays presents no difficulty in placing them in the same category; for we have but to suppose that, during the period of submergence, when these clays were formed, the water in the Grangemouth

* Trans. Roy. Soc. Edinburgh, xxiv. p. 629.

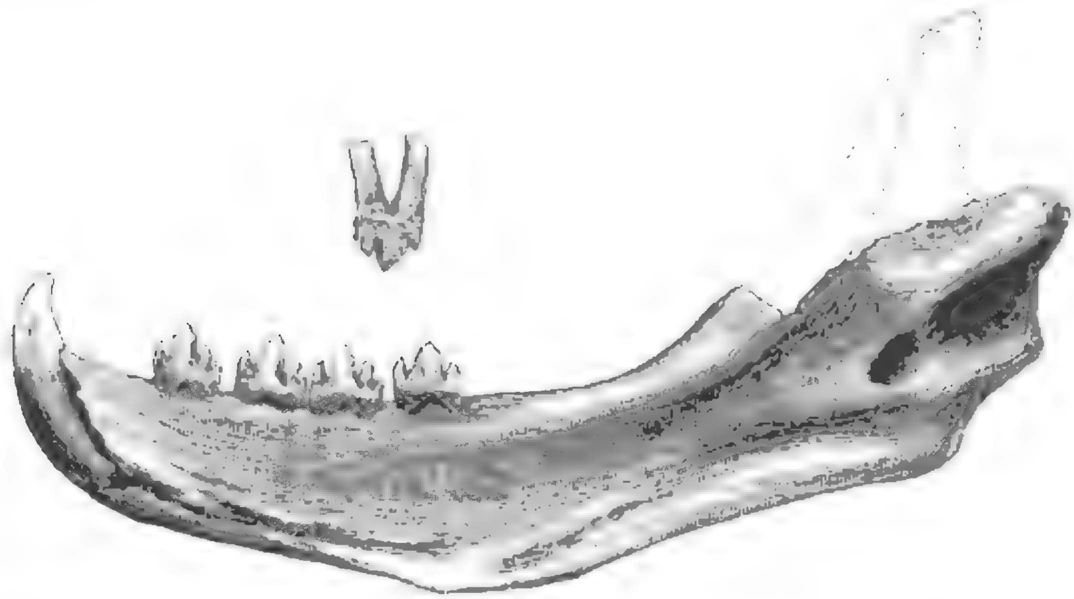
locality was some 200 feet deeper than in the districts of Stratheden or of Errol, so that the change in the relative position of land and water which has taken place since that time has caused the Stratheden clays to be elevated 150 feet above the present sea-level, whilst the Grangemouth clay is some 60 or 70 feet below it.

I shall now proceed to inquire into the characters of the bones of the Grangemouth seal, with the view of determining—1*st*, Whether the animal was of the same species as the seals whose bones have been found in beds of clay in Scotland by other naturalists; and, 2*d*, Whether the species is or is not the common seal, *Callocephalus vitulinus*, which now frequents our coasts.

With regard to the first part of the inquiry, I have compared this Grangemouth seal with the Errol seal found by the Rev. Thomas Brown, with the skeleton from Stratheden, and with the bones of the Portobello seal, which form a part of the natural history collection in the Museum of Science and Art. I may mention, that the bones from Portobello have received some important additions since Dr Allman drew attention to them at the meeting of this Society; for Dr Andrew Balfour, by whom they were discovered, has added to the collection one-half of the lower jaw and several teeth.

As regards the Errol seal, the bones recovered were vertebræ and ribs, of which two only, viz., the atlas and one of the lower cervical vertebræ, have representatives in the Grangemouth skeleton. The Errol seal is an older animal, and the bones are larger and more completely ossified than those of the Grangemouth seal; but when due allowance is made for the difference in age, their form and general characters are so much alike that I believe them to be animals of the same species. The materials for comparison with the Portobello and Stratheden seals are, fortunately, more complete; for in them, as in the Grangemouth seal, the lower jaw and teeth are almost perfect, and the femur, scapula, and other bones are represented in each skeleton. All three animals were immature, for the epiphyses of the thigh bones are not yet anchylosed to the shafts. The atlas of the Portobello seal is somewhat less in its antero-posterior diameter than in the one from Grangemouth, and the distance of the inferior dental foramen from the hinder end of the lower jaw is greater in the Portobello and the Stratheden than in the

Grangemouth specimens. These differences are, I believe, merely individual and not specific. On the other hand, there is so close a correspondence in the general form of the lower jaws, in the number and cuspidation of the teeth, and in the mode in which they are implanted in their sockets, that I am of opinion these seals were animals of the same species. This identity in the specific characters of the seals found in the clay formations on the east coast of Scotland furnishes an additional argument in favour of the view, that they have been deposited at the same epoch and under the same conditions. We may now inquire if this clay seal corresponds with the present British species, the *Callocephalus vitulinus*.



Inner surface of the right half of the lower jaw of the Grangemouth seal, the size of nature. The outline of the coronoid process is filled in from the Portobello seal. The single tooth is one of the upper molar series.

Dr Knox stated that the Camelon seal was identical with the species now inhabiting the Forth, and many other naturalists who have written on this matter are inclined to the same view. At the time when Dr Knox wrote, the specific differences between the various northern seals had not been precisely made out, and the determination is even yet one of much difficulty, unless the skulls and teeth can be compared with each other. Dr Knox does not say what the bones were which came under his observation, so that we have now no means of knowing how far he had in his possession the materials for making an exact comparison.

Dr Page expresses himself with more reserve regarding the Stratheden seal. He looks upon it "as a pretty widely divergent variety of the common seal, if not a distinct species—a point, however, which yet awaits the precise determination of the comparative anatomist."

I have now carefully compared the jaws (more especially the lower, which are best preserved), and the teeth of the Grangemouth, Stratheden, and Portobello seals, not only with the adult skulls and teeth of the common seal, but with a young skull of that species, apparently about the same age as the fossil specimens, and I have no hesitation in saying that they are not of the same species. The number of teeth is indeed the same, but the character and mode of implantation of the molars exhibit important differences. In the clay seals, the number of cusps in the premolar and molar series does not exceed four, and this number is distinctly marked in all but the first and last. The second cusp in each tooth is the largest, but it does not preponderate very greatly over the first and third cusps, and the bases of the crowns are not much swollen. The teeth are set in the jaw in longitudinal series, one directly behind the other.

In the young of the common seal the cuspidation of the lower molars is not so uniform as in the clay seals; the last molar has four cusps, the penultimate has five, and the third and second only three. One cusp preponderates largely over the others, and the base of the crown is swollen. The molar teeth, also, are set obliquely in the jaw, so that one tooth not only lies in front, but somewhat to the outer side of the one behind it. This oblique setting of the grinders is also seen in well grown specimens.

The upper molars in the clay seals are smaller and more delicately formed than in the common seal. They are, as a rule, tricuspidate, and with, as a rule, the central cusp the largest. They are not set obliquely, and the more anterior do not overlap those which lie behind. In the common seal, again, the anterior cusp is usually the biggest, and the upper, like the lower molars, are set obliquely.

I have also compared the jaws and teeth of these clay seals with the skulls of *Phoca barbata*, *Halichoerus gryphus*, and *Pagophilus groenlandicus*, northern seals, which possess the same general dental formula. With *barbata* and *gryphus* there are so many points of

difference that I cannot regard them as identical. With the Greenland seal, again, the points of resemblance are, in some respects, very striking. They agree in the number, mode of arrangement, and relative size of the cusps, and in the mode in which the teeth are implanted in the jaws, though the teeth are set closer together in the fossil than in the Greenland species. Unfortunately, I have not had access to a young skull of the *Pagophilus groenlandicus*, or to an adult clay seal, so that the materials for comparison have not, in this respect, been as perfect as to enable me to identify the species with accuracy. The examination, however, which I have made, leads me to think that these young clay seals may be either immature specimens of the *Pagophilus groenlandicus*, or of a closely-allied species. But it will be difficult to express a positive opinion until adult skulls are compared with each other, and the skulls of the clay seals be compared with the crania of *Pagomys fœtidus*, crania of which are not yet in my possession.

Addendum, March 12*th.*

Since this paper was read to the Society, I have received some additional material of considerable importance in connection with the determination of the species of seal found in the glacial clay-beds of Scotland. Dr Howden has kindly sent me the bones of an adult seal, found in glacial marine clay at Puggiston, three miles from Montrose.* Through Mr William Livesay and Dr M'Bain, I have had the opportunity of examining three crania of the small arctic seal, *Pagomys fœtidus*, Gray (*Phoca hispida*, Cuvier). These skulls were from two adult and one young specimen.†

The bones from Montrose included several vertebræ and ribs, pelvis, scapulæ, and the long bones of the extremities, together with the two halves of the lower jaw and the left upper jaw. They were found thirty feet below the surface, about three quarters of a mile from the tidal estuary of the South Esk, and about five feet

* The geology of this district has been carefully described by Dr Howden in the Trans. Ed. Geolog. Soc. 1867–68.

† These skulls were procured in the Spitzbergen seas during the arctic expedition conducted last summer by Mr Lamont.

above the present sea-level. I have compared these bones with the corresponding bones in the skeleton of the common seal, and have satisfied myself that they belong to animals of different species. I have also compared them with the bones of the other clay seals already referred to, and am of opinion that the Montrose seal is an adult of the same species as the Stratheden, Portobello, and Grangemouth specimens. Comparing the lower jaw of the Montrose seal with that from Grangemouth, depicted on page 110, we find that they have the same general form, differing from each other only slightly in size; that the teeth have the same characters, and are implanted in the jaw after the same manner.

If we compare the lower jaw of the adult Montrose clay seal with that of an adult *Pagophilus groenlandicus*, we find important differences in size, which are expressed in the following table, the dimensions being taken in straight lines—

	Clay seal.	*P. groen.*
Length from posterior border of condyle to socket of canine tooth, . . .	4·2	5·1
Vertical diameter of horizontal ramus opposite last molar, ...	0·8	1·0
Antero-posterior diameter of ascending ramus just above the tubercle, at the angle,	1·1	1·6
Vertical diameter of ascending ramus, .	1·6	2·4

On the posterior border of the ascending ramus of the lower jaw of *P. groenlandicus*, a large triangular tubercle projects obliquely backwards and inward; in the clay seals, both adult and immature, the corresponding tubercle is not triangular, and has the form of an elongated almost vertical ridge. The teeth in the adult clay seal are set more closely together than in *P. groenlandicus*, and though the cusps in the fossil are considerably worn, yet there is not that preponderance of the central cusp over the anterior and posterior cusps in the fossil, as in the Greenland seal. The comparison of the temporal bones, and of the upper jaw with its teeth, of the adult fossil with the Greenland seal also showed important differences, so that I am constrained to give up the idea, at one time thought probable, that these seals were of the same species.

I have now instituted a comparison between the lower jaws of the adult clay seal and of the *Pagomys fœtidus*, and find they correspond

much more closely, not only in form, but in dimensions. The corresponding dimensions of the latter to those of the clay seal already given in the table, being respectively 4 inches, 0·8 inches, 1·2 inches, and 1·4 inches; the differences, therefore, being so trifling as to be merely individual. They both possess the elongated ridge-like tubercle on the posterior border of the ascending ramus, and a deep masseteric fossa on its outer surface, which is bounded posteriorly by a ridge ascending to the outer end of the condyle, which ridge becomes continuous with that on the posterior border already referred to; in both the lower border of the horizontal ramus is incurved opposite the last molar tooth, behind which incurved portion it sweeps backwards and outwards in a graceful curve; in both the arrangement and cuspidation of the teeth are closely similar, although the intervals between the anterior molars are somewhat greater in *P. fœtidus*, than in the fossil.

The upper jaws and temporal bones in the two seals closely correspond in form.

The affinity, therefore, of the fossil seal to *Pagomys fœtidus* is very close,—so close, indeed, that I should not consider myself justified in pronouncing them to be distinct species.

So far, then, as I have had access to materials for comparison, I am inclined to think that the seal, the remains of which are found in the brick-clays of Scotland, corresponded with the now existing small arctic seal, *P. fœtidus*.

I am not aware that there is any satisfactory evidence to show that this northern seal ever visits our shores at the present day, so that we may consider the determination of its bones in the brick-clays to be an additional piece of evidence to those advanced from other data, that at the time when these clays were deposited an arctic climate prevailed over Scotland.

The following Gentleman was elected a Fellow of the Society:—

Dr J. Warburton Begbie, F.R.C.P.E.

Monday, 7th March 1870.

WILLIAM FORBES SKENE, Esq., Vice-President, in the Chair.

The following Communications were read :—

1. On the Rate of Mortality of Assured Lives as experienced by Ten Assurance Companies in Scotland from 1815 to 1863. By James Meikle, Esq. Communicated by Professor Tait.

The mortality of assured lives is introduced by a short statement of the process followed in the obtainment of the rate of mortality among the male population of England and Wales during seventeen years, and in which the results are compared with the rate obtained by following the same process with the male population of Scotland during ten years. A statement is given of the method employed for collecting the facts referring to assured lives, and of tabulating the results with the view of extracting, not only the total numbers entering upon and dying in each year of life, but of exhibiting the experience of several highly interesting and important sections of risks, and more especially with the view of showing the nature and benefits accruing from the assurance of selected healthy lives.

The subject generally is divided into the consideration of the mortality on *healthy* lives—males—females—and *diseased* lives.

Assured Male Lives.

In treating of healthy lives—males—a comparison is made of the actual number of deaths during each quinquennial period of life, with the number which might have died according to the Carlisle table and the Actuaries' table of 1837. The rates of mortality at each age, summed in periods of five years, are also compared. These comparisons point out that the Carlisle table exhibits a greater rate of mortality up to age fifty, and a lesser rate at higher ages than the experience of the offices; and that the Actuaries' table, at nearly all ages, is *slightly* greater than that of the Offices. A short com-

parison is made of the rates of mortality of male lives according to the three English life tables and that derived from the population of Scotland, already referred to, with the mortality of the selected healthy assured males of the ten Scottish offices. A very general view of the benefits of selection is thus obtained. The assurances on healthy male lives are divided into two classes—assurances *with* profits, and assurances *without* profits; the mortality of the "*with profit*" class exhibiting results in a highly favourable direction, and of the "*without profit*" class in an *un*favourable direction—the one being 10 per cent. and 7 per cent. less than the Carlisle and Actuaries' tables respectively, and the other about 12 and 13 per cent. greater.

The foregoing comparisons of the actual and computed number of deaths at each year of life are reclassified in another form, so as to exhibit the actual and computed deaths *out of the entrants at each age*, and thus show how far *one aggregate* table of mortality expresses or represents the experience of its several parts or sections. These comparisons are made with the Carlisle and Actuaries' tables, from which it will be seen that neither table accurately measures the experience of sections of entrants. Young entrants exhibit a greater mortality than estimated by either table. There is, at same time, exhibited a similar comparison of the experience of the ten offices, derived from the aggregate male lives, reapplied to the several sections of entrants, which points out in a still more marked manner the inappropriateness of *one* aggregate table of mortality to measure the experience of its sections. There is also exhibited the extent of the deviations, favourable as well as unfavourable, in *each year of the assurances*, from which it will be seen that the deviations are highly favourable during the first four years, and that after the fourth year they are almost always *un*favourable.

Assured Female Lives.

In considering the mortality of females, there is, in the first place, given a comparison of the difference between the mortality of males and of females of the population, and of the Actuaries' table of 1837, pointing out that a nearly similar relation exists between the results of these tables with that experienced between male and female assured lives in the Scottish offices, viz., a greater mortality of female life up to age forty-five. On the other hand, the male and

female Government annuitants of 1829 and of 1860 exhibit a greater mortality of male life *at all* ages. An explanation of these differences is offered. A comparison is then made between the actual number of deaths and rate of mortality, of healthy assured females, with the computations according to the Carlisle and Actuaries' tables. There is, at same time, given a table, showing the favourable and unfavourable deviations of the *one* aggregate table of mortality, as a measure of the experience of sections of entrants. From this table it will be seen that neither the Carlisle nor the Actuaries' table correctly measures the mortality of female assurants under age thirty-five; and it will be inferred from the results given, that the table, based upon the aggregate experience of assured female lives, cannot measure the *aggregate* experience and at same time accurately represent the mortality of its sectional parts.

Total Lives—Males and Females.

After the usual comparisons of the actual and computed number of deaths and of the rates of mortality, according to the Carlisle and Actuaries' tables, there is given a view of the rates of mortality experienced on assurances effected *with* participation in profits and *without* participation, and an explanation is given of the reason of the greater mortality of assurances without profits, by pointing out that a very much greater mortality has been experienced on assurances (without profits) effected for temporary periods, averaging about 40 per cent. on lives under age 50. The relation of the aggregate to the sectional experience, as in the case of male and female lives separately, is shown with similar results. A very full comparison is thereafter effected between the mortality of assured lives with the mortality of the population. After comparing these aggregate experiences, a comparison is made between the rate of mortality experienced on assured lives, *excluding* the light mortality of the *first* year, first *two* years, &c., of the assurances, and the general aggregate rate of mortality of the population, with the view of pointing out, *in this form*, the relation of the mortality of assured lives, after the effects of selection have subsided, to the mortality of the population. The effect of selection is thereafter considered in its proper manner, and comparisons made between the mortality of persons in the same quinquennial period of life, but arranged according to the

duration of the risks, showing that the light mortality during the years while selection is in operation is balanced by a heavier mortality thereafter, and showing further that that heavier mortality is considerably greater than the general average mortality of a single aggregate mortality table. These are exemplified in various forms. The comparisons, however, are all based upon lives *once* assured. There is, finally, given in one view the rate of mortality experienced on all entrants of each age, during each year of assurance, as the true exponent of the rate of mortality on assured lives, along with five abridgments of the same, in the case of persons assuring at each quinquennial age.

Causes of Death.

There is also given the intensity of the causes of death at each age, and the relation of the deaths of assured lives, from various causes, to the deaths of the male population of Scotland, pointing out the several orders of disease in which the mortality of assured lives is greater or less than the population. There is also given, in a general form, the effects of selection upon the various causes of death, pointing out those in which selection appears to have been of greatest benefit.

Diseased Lives.

The usual comparisons are made of the actual with the computed number of deaths, and also with the rates of mortality, pointing out that the mortality on diseased lives is greater than on healthy lives by about 20 per cent. The diseased lives were thereafter broken up into sections, according to the nature of the imperfections for which the extra charge was made, and showing the rate of mortality experienced on four such classes. For two of these classes—unfavourable personal history and gout—and also for the general class of diseased lives, the law of mortality is given, as well as the annual premium for assurance of L.100 at death, showing the extra charge for such classes of lives.

Years of Life.

All the foregoing methods of comparing actual with computed results have dealt with *numbers of deaths*. A method is pointed out

for making comparisons of the actual years lived, with the computed number according to any table. Examples are given in the case of entrants at age 25, 30, 35, 40, 45, and 50.

Interpolation.

A description is given of the methods of deducing, and of practically applying, two processes of interpolation. One of them is based upon the principle that the quantum of mortality in each decennial period of life, in the adjusted and unadjusted results, shall agree. The second principle is based upon a formula, which expresses the number living in the law of mortality, at any age, in terms of constants, and the complement of life at that age. The formulæ for several differences are given in both cases, and the results applied to the total assured lives in the general mortality experience of the English and Scottish Assurance Offices.

2. Notes on Indian Society and Life in the Age when the Hymns of the Rigveda were composed. By John Muir, D.C.L., LL.D., Ph.D.

(*Abstract.*)

The paper began by stating, that although the hymns of the Rigveda exhibit a simpler and less developed stage of religious belief and conception than we find in the works of the earliest Greek poets, and a system of ideas wildly diverse, both from the mythological forms and the theosophic opinions, of the later Indian pantheon, and of subsequent speculation; and although many of the customs and practices of that early age are different from those of later times, we are not to suppose that in the former period the condition of society was of a very primitive description. On the contrary, there are many signs of a considerable progress in civilisation and culture then existing. The opinion of the late Professor H. H. Wilson on this head was then quoted; and as one proof in support of the position, the variety and occasional elaborateness of the metres in which the hymns are composed was referred to.

1. Some account was then given of the country occupied by the Indians of the Vedic era—of which a considerable portion is con-

sidered to have been cultivated, though much was also covered by forests—and of their villages and cities, or fortified places, and their houses.

2. A sketch was then given of the manner in which a priest of the Vedic age may be supposed to have spent the greater part of the night watching for, and hailing, with hymns and offerings, the appearance of the several deities, the Asvins—Ushas (the Dawn), Agni (Fire), Surya (the Sun), &c., at the times when they were supposed respectively to manifest themselves.

3. The discrepant opinions of two Sanscrit scholars, Professor Max Müller and Dr Bollensen, on the question whether or not the Indians made images of their gods during the Vedic age, are adduced, but it was considered that the question is not ripe for decision.

4. It was next stated that this tract of country was divided into numerous principalities, governed by their respective kings, who appear to have lived in considerable state, and to have been possessed of a good deal of wealth, both in cattle and goods of different descriptions.

5. Reference was made to the existence of both rich and poor in the communities, and some verses, in praise of liberality to the latter, translated from the original, were read.

6. Some particulars relating to domestic relations, and life and manners, were then given. Polygamy appears to have existed, but not of course as the rule. It was considered a misfortune for a woman to grow old unmarried. Women appear, sometimes at least, to have been allowed to choose their own husbands. According to a hymn of the Atharva-veda, the remarriage of widows seems to have been permitted; and from a verse of the Rigveda, it appears probable that a widow could marry the brother of her deceased husband, when the latter had died childless. Allusions to conjugal infidelity and sexual immorality occur.

7. It was stated that considerable attention seems to have been paid to personal decoration, as reference is made, in various places, to elegance of dress, and to the use of jewels. No mention is made of cotton as a material for clothing; though, as the plant is considered to be indigenous in India, and the use of light cotton cloth seems essential to comfort in so warm a climate, it is probable that it was well known. Wool is mentioned in various places.

The hair appears to have been occasionally worn wound or braided upwards in a spiral form.

8. Barley, at least, if not wheat also, and no doubt other grains, were used as food. The flesh of kine also seems to have been eaten. Wine (from what material distilled does not appear) was drunk by people of the upper classes, contrary to the usage of the later Hindus.

9. A hymn, descriptive of the variety of men's tastes and pursuits, was given in a metrical translation, in which various professions are mentioned, viz., those of poet, priest, physician, carpenter: the construction of chariots is often alluded to; and working in iron or other metals, and in hides, must have been common, as the mention of weapons of war and other metal implements, and of leather, is constantly occurring. Weaving, too, was of course practised, and boat building understood, as boats are frequently referred to. The caste system does not seem to have been developed during the earlier part of the Vedic era; but in a few of the later hymns Brāhmans are mentioned; and in one text the names of the four castes Brāhman, Rājanya, Vaisya, and Sudra, occur in conjunction. A free translation was given of a hymn in which the Brāhmans and their observances appear to be satirised. From what precedes under head 8, it will be seen that agriculture was practised, and specific references to it, and apparently to irrigation as auxiliary to it, occur.

10. Playing at dice was a favourite amusement of the Vedic Indians, as appears from numerous texts. A hymn, in which the miseries of a gambler's life are strikingly described, was given in an English metrical dress. Gaily dressed dancers or actors are referred to as exhibiting their performances.

11. Theft and robbery are alluded to as common offences.

12. As animals, wild or tame, mentioned in the Rigveda, kine, horses, sheep, goats, dogs, deer, boars, buffaloes, apes, wolves, and lions, are adduced. Elephants, too, are alluded to in the Rigveda, certainly as wild, but whether or not as tame also is not so clear. Among birds, pigeons, falcons, vultures, ducks, swans, and quails are referred to.

13. It need scarcely be said that wars were frequent in the Vedic age. Parts of two hymns translated in prose were read—one of them in celebration of Indra's prowess, and supplicating victory, and the

second in praise of armour and the bow, &c.; and a portion of one of them was also given in verse. War chariots were in use, and banners, defensive armour, and various kinds of offensive weapons, bows and arrows, spears, &c., are referred to.

14. Finally, allusion was again made to the number and elaborateness of the metres in the Rigveda; and as regards the occasional beauty and variety of the illustrative imagery, the moral depth of many of the sentiments, and the power of observation exhibited in its contents, reference is made to the hymns to the Dawn, and to several of those adduced in the course of the paper. In a few hymns we find the beginning of speculation on the origin of all things. One of these was communicated, rendered into English verse.

The following Gentleman was elected a Fellow of the Society:—

John Winzer, Esq., Assistant Surveyor, Civil Service, Ceylon.

Monday, 21st March 1870.

The Hon. Lord NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. On the Lake Basins of Eastern Africa. By Keith Johnston, Jun., Esq., F.R.G.S.

1. *Livingstone's Recent Discoveries.*

In 1866 the indefatigable Dr Livingstone is again in Africa, with the determination of filling up the great gaps in our knowledge of the lake region from Nyassa to Tanganyika, beginning the great journey from which he has not yet returned.

News arrived in England, in September 1866, that the traveller had, for a third time, entered the Rovuma river, and had succeeded in penetrating for 130 miles from its mouth, where he had found a friendly chief, whose residence he intended to make the starting-point of his expedition to the northern end of Nyassa, and the south of Tanganyika. A long period of silence then intervened, during which we were ignorant of the whereabouts of the traveller, till a report was brought to the east coast by some lying Johanna

men who had deserted him, that Livingstone had been murdered near the south end of the lake. This report, however, was discredited by the head of the Royal Geographical Society, and a boat expedition sent out by the Society, under the leadership of Mr Young, confirmed the opinion of its untruth.

From his more recent letters, we learn that Livingstone passed round the southern end of Lake Nyassa, where he seems to have struck into nearly the old route of Lacerda and Monteiro, along the water parting between the tributaries of the Zambezi and the Nyassa.

Passing at a distance of about twenty miles to westward of Chinyanga, the furthest point which he had reached in his excursion of 1863 from Nyassa, he got into the valley of the Loangwa or Arangoa. The greater part of Livingstone's subsequent route is contained in his letter of date July 1868. In this he says—"Leaving the valley of the Loangwa, which enters the Zambezi at Zumbo, we climbed up what seemed to be a great mountain mass, but it turned out to be only the southern edge of an elevated region, which is from 3000 to 6000 feet above the sea. This upland may be roughly stated to cover a space south of Tanganyika of some 350 miles square. It slopes to north and west, but I have found no part of it under 3000 feet of altitude. The country of Usango, situated east of the space indicated, is also an upland. . . . Usango forms the eastern side of a great but still elevated valley. The other, or western side, is formed by what are called the Kone Mountains, beyond the copper mountains of Katanga."

Livingstone continues—"The southern end of the great valley, enclosed between Usango and the Kone Mountains, is between 11° and 12° south. In 11° 6′ south, ascending from the valley of the Arangoa, we were fairly on the upland." This was perhaps in January 1867, or about the middle of the rainy season here. He writes—"As we advanced, brooks, evidently perennial, became numerous. Some of these brooks went eastward, to fall into the Loangwa; others went north-west, to join the Chambeze." The Chambeze, with all its branches, flows from the eastern side into the centre of the great upland valley, "which," says Livingstone, "is probably the valley of the Nile. It is an interesting river, helping to form three lakes, and changing its name three times in

the 500 or 600 miles of its course. I crossed the Chambeze in 10° 34′ south, and several of its confluents, north and south, quite as large as the Isis at Oxford, but running faster, and having hippopotami in them."

Livingstone reached a place called Bemba, on the plateau, in February 1867, and fixed its position in 10° 10′ south, 31° 50′ east. Proceeding northwards, in April 1867, he discovered Lake Liemba. It lies in a hollow, with precipitous sides 2000 feet down, on the northern slope of the upland. "It is extremely beautiful, sides, top, and bottom being covered with trees and other vegetation. Elephants, buffaloes, and antelopes feed on its steep slopes; whilst hippopotami, crocodiles, and fish swarm in the waters. It is as perfect a natural paradise as Xenophon could have desired. On two rocky islands men till the land, rear goats, and catch fish. The villages ashore are embowered in the oil palms of the west of Africa."

"Four considerable streams flow into Liemba, and a number of brooks, from 12 to 15 feet broad, leap down the steep bright red clay, such are the rocks, and form splendid cascades, that made the dullest of my attendants pause and remark with wonder."

Livingstone does not give any note of the direction of these four rivers, which flow into the lake; but it appears a necessary conclusion, from its position, that these should have their rise on the higher side of the plateau, and flow to the lake from the east.

"The lake is from 18 to 20 miles broad, and from 35 to 40 miles long. It goes off to north-north-west, in a river-like prolongation, two miles wide—it is said to Tanganyika." Livingstone continues —"I would have set it down as an arm of Tanganyika, but that its surface is 2800 feet above the level of the sea, while Speke makes the lake Tanganyika 1844 feet only." The observation of Livingstone here confirms the opinion of Mr Findlay, given in an able paper read before the Geographical Society in 1867, in which, by a recomputation of the thermometer heights measured by Captain Speke, he came to the conclusion that Tanganyika Lake was at an elevation of 2800 feet above the sea; and that, since its fresh waters must have an outlet, this would most probably be found to be to northward.

Livingstone continues—"I tried to follow this river-like portion of Liemba, but was prevented by a war which had broken out between the chief of Itawa and a party of ivory traders from Zanzibar. I then set off to go 150 miles south, then west till past the disturbed district, and to explore the west of Tanganyika, but, on going 80 miles, I found an Arab party, showed them a letter from the Sultan of Zanzibar, which I owe to the kind offices of his Excellency Sir Bartle Frere, late governor of Bombay, and was at once supplied with provisions, cloth, and beads. . . . After peace was made, I visited Nisama, the chief of Itawa, and having left the Arabs, went on to Lake Moero, which I reached on the 8th September 1867. In the northern part Moero is from 20 to 33 miles broad. Further south it is at least 60 miles wide, and it is 50 miles long. Ranges of tree-covered mountains flank it on both sides, but at the broad part the western mountains dwindle out of sight."

Lake Moero is the central one of the three on the Chambeze river. The river runs into Lake Bangweolo, at the head of the valley, and on coming out of it assumes the name of Luapula. The Luapula flows down north, past the town of the Cazembe, and 12 miles below it enters Lake Moero. Passing up the eastern side of Moero, Livingstone came to the Cazembe's town. It stands on the north-east bank of the lakelet Mofwe. This is from 1 to 3 miles broad, and nearly 4 long. It has several low reedy islands, and yields plenty of fish, a species of perch. It is not connected with the Luapula or Moero.

"I was forty days at Cazembe's," says Livingstone, "and might then have gone on to Lake Bangweolo, which is larger than either of the other lakes, but the rains had set in, and this lake was reported to be very unhealthy. I then went north for Ujiji, where I have goods, and I hope for letters; for I have heard nothing from the world for more than two years; but when I got within thirteen days of Tanganyika, I was brought to a standstill by the abundance of water in front. A native party came through and described the country as inundated, so as to be waist deep, with sleeping places difficult to find. This flood lasts till May or June. At last I became so tired of inactivity that I doubled back on my course to Cazembe."

In this attempt to reach Ujiji, Livingstone appears to have intended to reach the west side of the Tanganyika by the road which Captain Speke reported from Warruwa (evidently the Rua of Livingstone) to the ferry by which he had crossed from Ujiji; and it was apparently during this attempt that Livingstone obtained, by actual observation, the report which he gives of the lower course of the Luapula. He says—" On leaving Moero at its northern end, by a rent in the mountains of Rua, the river takes the name of Lualaba, and passing on north-north-west forms Ulenge in the country west of Tanganyika. I have only seen it where it leaves Moero, and where it comes out of the crack in the mountains of Rua."

The flat inundated country beyond this point seems to have been his turning-point. He says—" To give an idea of the inundation which, in a small way, enacts the part of the Nile lower down, I had to cross two rivulets, which flow into the north end of Moero—one, the Luo, had covered a plain abreast of Moero, so that the water on a great part reached from the knees to the upper part of the chest. The plain was of black mud, with grass higher than our heads. We had to follow the path which the feet of passengers had worn into deep ruts. Into these places we every now and then plunged, and fell over the ankles into soft mud, while hundreds of bubbles rushed up, and bursting emitted a frightful odour."

Having returned to Cazembe's in about February or March of 1868, Livingstone seems to have gone south at the beginning of the dry season, to Lake Bangweolo, from which his letter is dated in July 1868.

The next news we have of the great traveller is in a letter from Ujiji, on Lake Tanganyika, dated May 1869. He appears to have reached this point by the *eastern* side of Tanganyika, not by the western as before attempted; since he writes in the above letter, " As to the work to be done by me, it is only to connect the sources which I discovered from 500 to 700 miles south of Speke and Baker's, with their Nile. The volume of water which flows north from latitude 12° S., is so large, that I suspect I have been working at the sources of the Congo as well as those of the Nile. I have to go down the eastern line of drainage to Baker's turning-point. Tanganyika and Nyige Chowambe (Baker's?) are one water, and the head of it is 300 miles south of this. The western and central

lines of drainage converge into an unvisited lake west or south-west of this. The outflow of this lake, whether to Nile or Congo, I have to ascertain." From the above it would appear that Livingstone had made an excursion northward from Ujiji, either by land or on the lake, to ascertain the union of the Tanganyika with the Albert Nyanza.

News has since been received, which shows that Livingstone was still at Ujiji in July 1869. In January of this year, a report arrived from the west coast of the continent, describing the fearful end which the traveller had come to, of his being quartered and burnt; but this report turns out to be an old story of date June 1868, with its plot laid on the Zambezi, and at this time we know that Livingstone was safe on the Chambeze lakes.

2. *The Sources of the Nile.*

The main point of interest in the latest travels of Livingstone, and that which gives to them a distinctive importance over the great accomplishments of his former journeys, is that in these Livingstone has undoubtedly visited and beheld the long sought-for sources of the Nile. It is true that there is considerable doubt as to which of the basins that he has explored will ultimately be acknowledged as the cradle of the Nile; but this at least is certain, that the real head streams have been visited by Livingstone, and the long-vexed question has, by these last explorations, resolved itself into a choice between two or perhaps three main streams.

Livingstone himself has apparently no bias in favour of one or other, so that the discussion is a perfectly open one. The three rival head streams are—first, the feeders of Lake Liemba; and, second, the Chambeze and its lake chain, both of which rise near the eastern edge of the great longitudinal plateau of the side of Africa next the Indian Ocean; the third is the source recently claimed for the Nile by Dr Beke, in his "Solution of the Nile Problem,"* the Great Casai or Kassabi river, which rises nearer the Atlantic side, in 12° S. Of the first of these, the feeders of Lake Liemba, we may say, with almost absolute certainty, that they are tributaries to the Nile, and it is most probable that they are the *sources* of that river. Livingstone has found these rivers

* Athenæum, February 1870.

flowing into Lake Liemba; a river-like prolongation unites Liemba and Tanganyika, these two appearing to be at the same level; then Tanganyika and Nyige Chowambe, which is evidently the Albert Nyanza, are one water, and that the last is a reservoir of the White Nile is undoubted.

The union of the second presumptive head stream, the Chambeze, with the Nile, is less apparent; indeed, the balance of evidence seems to show that it must be the head of the other great river of Africa, the Congo. If the Chambeze prove to join the Nile, then the streams to the Lake Liemba become mere tributaries, since the course of the Chambeze is by far the longer of the two. The feeders of Liemba and the Chambeze rise, however, side by side, on the eastern plateau. The Chambeze flows down into the central valley through Lake Bangweolo, and then northward through Lake Moero. Livingstone describes Lake Moero as beginning 12 miles below the position of the town of Lunda, the capital of the Cazembe (lat. 8° 40′ S., long. 28° 20′ E.), whose position may be laid down with tolerable accuracy from the former journeys of the Portuguese travellers. Since Livingstone proceeded north from Cazembe's town, along the eastern shore of Moero, in his attempt to reach Ujiji in 1867, the great bulk of this lake must lie to the westward of the meridian of Lunda, or about 120 miles to westward of Tanganyika. Dr Livingstone has seen the river at its outflow from the lake, and also at the point where it emerged from the crack in the mountains of Rua, when, according to his own observation, the river turned to north-north-west to form Ulenge, a third lake or marsh in the country west of Tanganyika. This north-westerly turn would carry the river quite out of the direction of the Nile basin, and the higher side of the continent being to the east, the probability is, that the river continues to curve to the west.

Again, the valley of the Chambeze, in the plateau where Livingstone crossed it, is, no doubt, one of the greatest hollows in the high land, so that the height of the river bed here may be taken at 3000 feet, the lowest level of the limits which Livingstone gives to the undulation of the plateau, or only 200 feet above the level of Tanganyika. Descending into the great valley to Lake Bangweolo from the plateau, the Chambeze must

have a considerable fall; from Bangweolo to Moero there must be a second descent. The Cazembe's country, which extends round to the south of Tanganyika, is described as flat, and its rivers are currentless and stagnant. If Moero were at a higher level than Tanganyika, would not the river which leaves it take a course over the level country instead of facing towards, and making its way through a crack in the mountains northward? Seeing that the river does force its way through these mountains, the presumption is, that Moero is at a lower level than Tanganyika; and if this be the case, the river which descends from it through the mountains can never again ascend to the level of the Nile lakes to join them, but must find some other course.

With regard to the third advocated source, the Kassabi river, of which Dr Beke affirms it to be his belief that it is the head stream and upper course of the Nile of Egypt, the difficulties of its joining the Nile appear to be even greater than the last. The upper course of this river only has been explored. It springs in the Mossamba Mountains, which are on the inner borders of Angola and Benguela, its sources being close to those of the Quango river, a tributary of the Congo. The Kassabi is known to flow northward as far as 8° S. to westward of the capital of the Muata Yanvo.

Dr Livingstone crossed its head on his journey from the Zambezi to Loanda; and the reports which he collected from the subjects of the Muata Yanvo's kingdom, all tend to prove, that whatever direction its middle course may take, in its lower course the Kassabi flows round to westward, and is joined by the Quango. The trader, Graca, who penetrated to the Muata Yanvo capital in 1846, says, that "the territory of this chief is shut in by the great rivers Kassabi and Lurua (a tributary of the Kassabi)." "These rivers," he continues, "flow into the river of Sena" (the Zambezi). The latter part of this statement we now know to be incorrect; but, taken as a whole, it indicates an easterly bend in the lower course of the river to enclose the kingdom of the Yanvo on the west and north, and to flow as if to the Zambezi. The Hungarian traveller, Ladislaus Magyar, has penetrated furthest of the three who have visited this region, and his information seems to agree well with this last. He reports that the Kassabi, after forming the waterfall of Muewe (in about 11° S. latitude), bends gently to northward; but

further on takes an easterly direction in its lower course, and reaches a breadth of several miles at the place where it *touches upon* the extensive lake Mouva or Uhanja.

Now, if we turn the Kassabi river eastward in latitude 8° S., in agreement with the above description, we find that it meets the position which Livingstone's letters give to Ulenge, the lake or marsh to which the Chambeze river flows, and whose waters Livingstone tells us by report, in his recent letters, are *taken up* by the Lufira, a large river which, by many confluents, drains the western side of the great valley.

Is not the *Lufira*, then, the lower course of the Kassabi, and the Lake Ulenge of Livingstone, whose waters are *taken up* by the Lufira—the Uhanja lake of Magyar, which the Lower Kassabi *touches* upon?

The same difficulties which appear in the way of the Chambeze river and lake chain joining the Nile, hold also against the Kassabi, which, from the above reports, would seem to join this river at Lake Ulenge.

Next, the question arises, if these rivers do not form a part of the Nile system, where then shall we find an outlet for them? The answer to this is plainly, in the Congo river.

The Congo was described by the Jesuit missionaries, who first visited its mouth, as so "violent and so powerful from the quantity of its waters, and the rapidity of its current, that it enters the sea on the west side of Africa, forcing a broad and free passage (in spite of the ocean) with so much violence, that for the space of 20 leagues it preserves its fresh waters unbroken by the briny billows which encompass it on each side." In the introduction to his narrative of his expedition to the Congo, Tuckey says, "If the calculation be true that the Congo, at its lowest state, discharges into the sea two millions of cubic feet of water in a second, the Nile, and the Indus, and the Ganges, are but rivulets compared with it, as the Ganges, which is the largest of the three, discharges only about one-fifth of that quantity at its highest flood." This estimate is greatly exaggerated, but Tuckey actually found that this vast river has a width of two, three, or even four miles, whilst flowing with a current of two or three miles an hour (p. 342), and this not at its mouth, but inland beyond the mountainous coast regions. Such a

vast river cannot be formed in a short course, but must have its rise far in the interior of the continent.

If we take the Kassabi river and its drainage to the Nile, where shall we find a sufficiently lengthened course for the Congo? Tuckey's unelaborated notes give the opinion that the "extraordinarily quiet rise of the river shows it to issue from some lake, which had received almost the whole of its waters from the north of the line;" and again, he says, "I cannot help thinking that the Congo will be found to issue from some large lake or chain of lakes, considerably to northward of the equator." The reason of Tuckey's supposition that the lakes, which evidently maintain the volume of water in the Congo, would be found north of the equator, is this, that he found the rising of the river beginning on the first days of September. At the time of his journey little or nothing was known of the times of the rainy seasons in Central Africa from actual experience. Since then the traveller Burton has told us (in his account of the expedition to Tanganyika, R. G. S. Journal, vol. xxix.), that in the latitude of Tanganyika the rain sets in at the end of August, lasting till May; and Livingstone says, in his latest letter, that he did not proceed to Lake Bangweolo from the Cazembe's capital, where he arrived about the middle of September, *because the rains had set in.* Lake Ulenge lies between these latitudes, so that the rise of the waters of the Congo on the first of September is perfectly explainable without the necessity of taking its reservoir lakes to the north of the equator; if the lakes were there, the rise of the Congo would occur at a much earlier period of the year, as we shall afterwards notice, and, indeed, the space in which Lake Ulenge lies, seems to be the only one on the continent whose rainy season would agree with the observed rise of the Congo.

3. *The Physical Features of the Lake Region and the Lakes.*

The great highlands of the world encircle and turn their steepest verge towards the Pacific and Indian Oceans; the slope is gentle towards the great plains which surround the Atlantic and Arctic Seas. Africa is no exception to this rule, since it presents to the Indian Ocean the abrupt descent of the plateau which extends along its eastern side from the Cape Colony to Abyssinia, north-

ward. It is true that the whole of South Africa is a plateau, with a general elevation of about 3500 feet, and that the outer edges of it rise steeply from both coasts; but the eastern side is the higher of the two, and the law of a general slope towards the Atlantic is maintained on its surface. The Lake Region occupies the central part of the eastern, or higher side, of the South African plateau, and here the line of descent to the coast-land of the Indian Ocean is marked continuously from north to south; first by the southward continuation of the outer slope of the Abyssinian table-land, then near the equator by the edge on which the great mountain peaks of Kenia and Kilima Ndjaro rise; farther south by the Rubeho Mountains, up which Burton and Speke ascended to the plateau; and then by the N'jesa Mountains, which wall in the Lake Nyassa. Farther south the cataracts of the Shiré river, 35 miles in extent, show where this river tumbles over the edge of the plateau, and the Zambezi breaks through it at the narrows of Lupata. Below this steep edge the coast-land slopes in gently to the sea, and is diversified by wide plains or scattered hill ridges.

The high surface of the South African plateau inland is hollowed out in the wide high valleys which contain its lakes and great rivers. The most northerly of these depressions in the Lake Region is that of the great lake reported by the ivory trader Piaggia, who approached within 60 miles of its northern shore. This lake appears to lie in a high valley on the northern edge of the plateau of South Africa, or rather in a recess of the northern lower land, partly shut in by the slopes of the plateau southward, and the mountain range which the traveller saw rising to south-westward beyond the lake, is perhaps only the steep northern edge of the southern plateau here.

The wide depression in which the Victoria Lake lies is shut in eastward by the continuation of the Abyssinian highland into the South African plateau. This valley appears to include the basin of the Bahari N'go, which is believed to be a vast salt marsh, or perhaps a sort of backwater of the Victoria Lake, and its slope is to north-westward, towards the angle of the northern lower land which is formed by the inner side of the Abyssinian highland running north and south, and the northern edge of the plateau of South Africa, which has a direction from east to west.

Between these two northern depressions lies the deeper and narrower valley of the Nile, which contains the Tanganyika and Albert Lakes. The beginning of this depression may be said to be at Lake Liemba, which lies sunk 2000 feet down in the edge of the plateau north of Lake Nyassa; then it opens out into a wider valley to the east of Southern Tanganyika, but again closes in the northern part of that lake, and is only a little wider where the Albert Lake is sunk between the edge called the Blue Mountains and the part of the plateau which separates this depression from the higher one of the Victoria Nyanza. To south-west of Tanganyika the narrow valley of the Upper Nile appears to have an opening into that one which contains the Chambeze and its lakes, made known by Livingstone, in the low-lying country of the Cazembé.

This valley of Bangweolo and Moero Lakes seems to be most completely surrounded on all other sides; by the high plateau of Usango eastward, by a narrower portion of it called the Muchinga Mountains southward, and again by the Kone Mountains, and a broader part of the plateau, the copper country of Katanga, on the west. The only other opening or outlet into this valley is apparently the rent in the mountains of Rua, through which the river makes its escape to join Ulenge. Westward is another and wider depression —the wide high plain which forms the kingdom of the great Muata Yanvo, watered by the Kassabi River, and stretching out between the Mossamba Mountains where the river rises, and the plateau of Katanga, which separates the Yanvo's from the Cazembe's valley.

The Zambezi valley closes the Lake Region southward. The Zambezi is the exceptional river of Africa, since it breaks through the higher side of the plateau to reach the Indian Ocean. Its sources, however, seem to be on the inner side of the plateau, springing on the western slopes of the Kone Mountains, and flowing first to the south-westward. The vast basin of this river (about 568,000 square miles) is comparable to that of the Volga, and would make more than one hundred river basins such as that of the Thames.

On the west, the waterparting of the Zambezi valley at Lake Dilolo is apparently but little elevated above the plain of the Muata

Yanvo's kingdom; but as the valley descends eastward its northern side appears to rise to the range of the Kone and Muchinga Mountains, and the valley becomes deeper and narrower where it cuts through the high edge of the plateau eastward, to reach the coast.

Lake Nyassa, a tributary lake of the Zambezi, lies in a deep longitudinal hollow near the edge of the plateau, only retained by the high barrier of the N'jesa Mountains. The narrow valley of the Shiré river, which flows from it, continues this hollow to the Zambezi. Lake Shirwa is similarly situated, but has no outlet, and in consequence its waters, in distinction to the fresh sweet water of the other lakes, are brackish. The approximate area of each of the eleven great lakes of this region, so far as their extent is known, is as follows:—

	Square Miles.
Victoria Nyanza,	29,900
Albert Nyanza,	25,400
Piaggia's Lake,	11,000?
Tanganyika,	10,400
Nyassa,	8,600
Bahari N'go,	6,000?
Bangweolo,	3,700
Moero,	2,000
Ulenge,	1,000?
Shirwa,	800
Liemba,	700
	99,500

The whole extent of water surface in this Lake Region is then nearly 100,000 square miles, an area not far short of that of the British Isles. A more definite notion of the great extent of these inland seas of fresh water may perhaps be obtained, if we observe that a direct passage across the Victoria Lake, from shore to shore (in its presently believed extent), corresponds in length to a voyage across our North Sea from Hull to Rotterdam, or from the eastmost land of Scotland at Peterhead, to the Norway coast.

The more important of the rivers of the Lake Region have been noticed in speaking of the routes taken by the travellers who have discovered them. One of the main hindrances to the exploration

of South Africa, is the difficulty of making use of these rivers as highways into the continent. The coast rivers of the Lake Region, or, indeed, of the whole of Eastern Africa, are barred at their mouths by the aggregated debris which they carry down, raised in banks on the coast between the downward current of the river and the opposing monsoon, or trade wind, blowing towards the coast. If this bar is passed at the mouth, still the navigation even of the largest rivers cannot cross the edge of the plateau where cataracts and rapids form a new obstruction. The vast lakes of the interior, and their great connecting rivers, however, present great lines of navigable water, which in a higher civilisation would be utilised for busy traffic, the line of the Nile basin in the Tanganyika and Albert lakes alone affording an unbroken voyage of about 900 English miles.

Piaggia, the traveller who has been nearest to the great lake which lies to the north-west of the Albert Nyanza, reports a great river called the *Buri*, flowing to westward, at some days' journey from Kifa (his furthest point), and which issues out of his great lake. The same river has been reached, at some distance from its supposed outlet, by the brothers Poncet (French ivory traders), who have long trafficked in this region, and they express the opinion that this river unites the equatorial lakes with Lake Tchad, by means of the Shari river. This they proposed to prove by an expedition on it in boats. The question, What becomes of this great river? which, at its outlet from the lake, is so large as to be only passable in boats, is an interesting one. It is certainly no tributary of the Nile, and the two most probable lower courses which it may have are those of the Shari to Lake Tchad, or of the Benue river to the Niger. If it ultimately proves to flow to Lake Tchad, it will give a striking evidence of the vast amount of evaporation which must exist in the region of that lake, since it has no outlet; but the Benue river seems to be its most probable course, for at its confluence with the Niger, the Chadda, or Benue, is the larger river. The Ogowai river is also a possible lower course for the Buri, but if the lake reported by Piaggia be, as we suppose, on or beneath the northern edge of the plateau of South Africa, it seems only natural that the river from it should seek the lower land to northward, than turn westward along the northern slope of the plateau.

4. *The Nature of the Surface of the Lake Region—Its Great Fertility.*

Africa, the only one of the continents which has a large extent of land on each side of the equator, presents a series of zones, each of which has a different nature of surface, and these belts correspond very closely with one another on the opposite sides of the equator. The central area of Africa, below the equator, in the zone of long rainy seasons, or of almost constant rain, is a region characterised by dense forests, and a most luxuriant overgrowth of vegetation, comparable to that of the selvas of the Amazon River in South America, which occupy the same equatorial position on the globe. To north and south of this forest zone is a belt of less wooded country, merging gradually into open cultivated or pasture lands. Next, these grass lands pass into the two great almost rainless deserts of the Sahara in the north, and of the Kalahari southward. Beyond the deserts, at the extremities of the continent, the outer slopes of the Cape Colony in the south, and of the plateau of Barbary, the "Tell" country, in the north, present a second zone of fertile and cultivated country.

The Lake Region extends from this central forest zone, in which the equatorial lakes are formed, through the more open belt of less wooded country southward, as far as the Zambezi valley, and this area is almost everywhere adorned with the choicest natural varieties of shady forest, with luxuriant underwood, or clumps of trees with rich grassy plains between.

5. *Climate of the Lake Region.*

Nowhere more than in this central region of Africa are the subjects of temperature, rain, and winds, more closely interwoven, or mutually dependent, the one upon the other. In the passage of this area beneath the sun, a low atmospheric pressure is produced by an ascending heat column, and by the condensation of vapour in this; the winds flow into the ascending column, and bring with them the moist air of the ocean, which, condensing in copious floods of rain, reduces the temperature, whilst causing a further opening, into which the winds blow with increased power. The area of low pressure, with its attendant circumstance of winds and rains, always tends towards that part of the continent which is

vertically beneath the sun's rays, and thus moving up and down the face of the land within the tropics, gives the wet and dry, the cold and hot, seasons of the year in this region. On the coast the seasons are sharply defined: the continental and the oceanic monsoons divide the year between either a single or a double wet and dry season; but in the high interior plateau in which the lakes are situated, the winds are drawn into the pendulating area of low pressure from the ocean, nearly throughout the year, and it is only when extreme limits of the tropical zone come directly under the sun, that a higher barometric pressure, an outflow of the winds, and a consequent dry period, is experienced here.

In the coastland under the equator, the country explored by the German traveller Brenner, the mean temperature of the year is 85°·1 (mean of three daily observations), the highest observed temperature (of 92°·8) having occurred in January, and the lowest (73°·4) in May. The rainy season here sets in with the south-east monsoon in April, and lasts till the end of June. The second rainy season, which we shall notice, taking place farther south in September and October, is almost lost at the equator. The north-east monsoon brings a cloudless sky of clear blue, and begins to blow here in November, lasting till March, and in this season rain is never thought of.

At Zanzibar Island, six degrees south of the equator, the mean temperature of the year is nearly 80° Fahr., rising in January to an average of 83°, falling in July to 77°; and it has a double rainy season, a stronger in March, April, and May, when the column of low pressure has passed this latitude in moving northward; and again in a weaker in September and October, when the low pressure passes in its southward course, at which times the monsoon winds change from the north-east, blowing out of Asia towards South Africa, to the south-west, blowing from Africa towards the Asiatic continent. In the low countries, beneath the edge of the plateau, about Zungomero, Burton tells us that the rain is constant, except for a single fortnight in the month of January; at most times the sun shines through a vale of mist with a sickly blaze and a blistering heat, and the overcharge of electricity is evinced by frequent and violent thunderstorms, so that the climate of Zanzibar is equally ruled by these two great land masses. On the Mozambique

coast the winds are again ruled by the African continent only, and the year is divided into a dry and wet season. From April till November the undeflected south-east trade wind blows upon this coast, and either from the lowness of the land or the shelter it obtains from the high island of Madagascar, this wind brings the dry season. From November to March the north-east monsoon, here at its furthest south limit, having passed over the warm Indian Ocean, brings the rainy season.

On the plateau inland, the climate and seasons are different. The mean annual temperature of the table-land in the neighbourhood of the Victoria Nyanza was found by Speke and Grant to be only about 68° Fahr., a temperature not greater than that of the south coasts of the Mediterranean, a climate not unsuitable to Europeans, since a hot summer in England is far more oppressive.

The rainfall in this high region is also an exceptionally small one for a tropical country, having been found to be only about 49 inches, or not so much as that of many parts of England, and this may partly be accounted for by the fact that this part of Africa is deprived of all rain from northerly winds, which come overland, and the prevailing east winds lose much of their moisture on the high eastern slopes of the plateau before reaching this region.

The traveller Burton gives an account of the very different climate of the deeper valley of the Tanganyika Lake. Here the rains divide the year into two unequal portions of eight and four months,—namely, the wet monsoon, which commences with violence in the end of August, and lasts till May, and the dry hot weather which completes the year. During the wet monsoon (1858) the prevalent winds were constantly changing. The most violent storms came up from the south-east or south-west of the plateau of Umyamwesi, to westward of the lake. Here he says that there are but two seasons, a summer and winter, and the rains begin in the middle of November. "The moisture bearing wind in this part of Africa is the fixed south-east trade, deflected into a periodical south-west monsoon." Further south in the Cazembe's country, the rainy season appears from Dr Livingstone's letter to begin in September, and he says that the floods in the country west of Tanganyika last till May or June. In the northern part of the Zambezi valley the traveller Silva Porto found the rains set in on the

Arangoa river in February, and they ended with him on the eastern side of the Nyassa in June.

On the Zambezi river in the Makololo district, Livingstone observes that the rain follows the course of the sun, since it falls first in October and November when the sun goes over this zone southward. When the tropic of Capricorn is under the sun in December, it is dry, and December and January are the months in which the droughts are most severe in the countries between the Zambezi and the Kalahari. When the sun turns again to northward in February, March, and April, the great rains of this part of the Zambezi valley are experienced.

6. *Population.*

The Lake Regions of Africa are well peopled. Behm, in his "Geographical Year-book," has estimated the population of that part of Eastern Africa, which lies between the equator, the line of Lake Tanganyika, the Cazembe's country, and the Portuguese colonies on the coast, at 3,500,000. This gives a density of population of about six to a square mile, but is apparently rather under than above the mark. It is true that the slave trade must reduce and disturb the population of this part of Africa to a great extent, since many thousands of slaves are annually brought down to and exported from the harbours on the coast; but, on the other hand, travellers in this region report a continuous population. Captain Grant describes the part of the Lake Region which he traversed as too thickly peopled to harbour many wild animals; the shores of Lake Tanganyika are, according to Speke, "thickly inhabited by numerous tribes;" and in his voyage on Lake Nyassa, Livingstone says, "Never before in Africa have we seen anything like the dense population of the shores of Lake Nyassa, especially in the south. In some parts there seemed to be an unbroken chain of villages. On the beach of well-nigh every little sandy bay, black crowds were standing gazing at the novel spectacle of a boat under sail."

The inhabitants of the Lake Region appear to belong entirely to the negro or negroid race, but are closed in to north and south by peoples of a different stamp.

The *Niam Niams* who inhabit the country north of the lake reported by Piaggia, and west of the Albert Lake, who had formerly

the reputation of being "half men and half dogs, with a fan-like tail," and of having a disposition to eat their fellow-creatures. prove, on nearer inspection by the traveller Piaggia, to be men of powerful, regular, and fine figure, of stately carriage, with bronze-coloured skin, long hair, and thick beard, barbarous indeed in their customs, but not cannibals. They are considered to be identical with the interesting race of the *Fellatah*, the dominating people of the western Soudan, or are perhaps a step between these and the Gallas of the east.

Burton describes the peoples he met with between the east coast and the Lake Region:—"The Sawahili of the Zanzibar coast are sprung from the intercourse of foreign traders and emigrants, Phœnicians, Jews, Arabs, and Persians, with the African aborigines. The Balonda people of the kingdom of the Muata Yanvo, to the west of Lake Tanganyika, are almost pure negroes; and between these and the mixed east coast there is a tolerably regular gradation of negroid races from east to west, brought about partly by long intercourse with foreign settlers, and in part by intermixture with the non-negro races of North Africa. The high road from the coast to Ujiji runs through comparatively quiet and peaceful races." "Cannibalism," says Burton, "is rare in Eastern Africa, and results either from policy or necessity."

The aspect of the great mass of this negroid race is not unprepossessing. They are tall and well-made mulattos, rather above the European standard. A giant or a dwarf is never seen. The people of the maritime regions have rough dirty skins of a dull pale black, like that of diluted Indian ink; from the central elevation of the eastern plateau the complexion improves, and further inland the yellow skin, so much prized in Eastern Africa, appears. From the Unyamwesi plateau to Tanganyika Lake, in those lower levels where heat and humidity are in excess, the people become lamp black, without a shade of brown. The negroid races appear to extend down the outer slope of the continent to near the Zambezi valley southward.

Livingstone speaks of the *negro* peoples of the shores of Lake Nyassa; and Silva Porto describes the natives he met with in the northern watershed of the Zambezi valley as "hospitable negroes."

The River Zambezi is nearly the boundary between the negroes

or negroids of the Lake Region, and the Kaffir races of South Africa.

South of the Zambezi the kingdom of Mosilikatze has been made up of the remains of a number of formerly independent tribes conquered by the Matebele Kaffirs pushing northwards; and Sekeletu's Makololo kingdom, in the Upper Zambezi valley, was founded by a former ruler who led this conquering Kaffir tribe from the head of the Orange river northward, and incorporated the vanquished tribes with this one to form his kingdom.

The most important kingdom of South Africa is the empire of the Muata Yanvo, whose subjects are purely negroes. The dominion of this potentate seems to reach from the Mossamba Mountains, at the head of the Kassabi river westward, to the town of Shinte, on the Leeba river, and the Muchinga Mountains southward, and thence round to the southern part of the Tanganyika Lake.

The northern extent of this kingdom is as yet unknown. The Muata Yanvo's empire includes that of the Cazembe, who is his vassal, and who rules for his sovereign over that part of the kingdom which is separated from the main portion by the desert or mountainous country of Katanga. The fertile and thickly peopled area, known to be under the sway of this great Central African ruler, is far greater than any of the kingdoms of Western Europe, and might be compared in extent to the united bulk of France and Italy.

In conclusion, we may glance at the enormous labours of the great traveller Livingstone, to whom the world is indebted for so vast a portion of its knowledge of the African continent, and whose recent travels have given a fresh interest to this part of the globe. The area of South Africa, which Livingstone has already explored, and not only explored, but in great part surveyed with accuracy, has an extent of about one million of square miles. It is difficult to form a correct notion of the space covered by such an area; and it may help to give an idea of the work which has been accomplished, if we remember that the united areas of all the western kingdoms of Europe—France, Austria, Germany, Italy, Spain—would scarcely make up the extent of land which Livingstone has virtually added to the known world.

2. On the Steady Motion of an Incompressible Perfect Fluid in Two Dimensions. By Professor Tait.

While discussing some of Mr Smith's applications of Maxwell's ingenious idea of representing galvanic currents by the motions of an imaginary fluid (*ante*, p. 79), I was led to the present investigation. I have since found that, as was only to be expected, I had been anticipated in a great many of the results I obtained—especially by Stokes, in the *Trans. of the Cambridge Phil. Soc.* 1843. Still it appears to me that I have a few novel results to communicate.

If ψ = const. be the equation of a current-line, Stokes has shown that—

$$\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} = f(\psi),$$

where f is an arbitrary function.

By the integration of this equation various singular results are obtained, especially as to the nature of the families of curves which can be lines of flow.

The equation of lines of equal pressure is then formed, and from it corresponding results are derived. A curious result is obtained when the motion is irrotational; in which case there is a velocity-potential ϕ, and we have—

$$P = \frac{2p}{\rho} + C = \left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2,$$

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0.$$

Here the elimination of ϕ gives us—

$$\frac{d^2 \log P}{dx^2} + \frac{d^2 \log P}{dy^2} = 0.$$

The method is also applied to certain cases of motion which, though not steady, can be treated as if they were steady—viz., cases in which a given state of motion is propagated in the fluid by translation or rotation; so that to a spectator moving in a given manner in a plane parallel to the fluid, the motion *appears* to be steady. Thus, for instance, we can treat as steady motion the case of two

equal parallel vortex-filaments rotating either in the same or in contrary directions.

3. On the most general Motion of an Incompressible Perfect Fluid. By Professor Tait.

This is a quaternion investigation into the circumstances of fluid motion, especially with reference to the case of vortices. The method employed is very similar to that which I gave to the Society in 1862 (*Proc. R.S.E.* April 28).

It is shown that if σ be the vector-velocity of a particle of fluid, so that

$$\sigma = iu + jv + kw,$$

and if we introduce the operators D_σ and δ_σ such that

$$D_\sigma = \frac{d}{dt} + u\frac{d}{dx} + v\frac{d}{dy} + w\frac{d}{dz} = \frac{d}{dt} + \delta_\sigma,$$

together with Hamilton's operator—

$$\triangleleft = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

the equations of fluid motion and of continuity are—

$$\left.\begin{aligned} \triangleleft P - \frac{1}{r}\triangleleft p &= D_\sigma \sigma \\ S\triangleleft\sigma &= 0, \end{aligned}\right\}$$

where r is the density, and P the potential of the applied forces.

The principal transformation is effected by means of the curious theorem in kinematics

$$\triangleleft D_\sigma \sigma - D_\sigma \triangleleft \sigma = \delta_{\triangleleft\sigma}\sigma.$$

Thus, for instance, we have from the equation of motion

$$V\triangleleft D_\sigma \sigma = 0,$$

because $\triangleleft^2\left(P - \frac{p}{r}\right)$ is obviously a scalar. The above theorem then gives

$$D_\sigma \triangleleft \sigma = \delta_{\triangleleft\sigma}\sigma,$$

which proves that if $\triangleleft\sigma$ is ever zero for any particle of the fluid it must remain so for that particle.

As an additional instance of the simplicity of the method employed, the following may be given in this abstract:—

If τ be the instantaneous axis of the element of fluid, whose velocity is σ, we have—

$$\triangleleft \sigma = -2\tau .$$

But

$$S \triangleleft^2 \sigma = 0 ,$$

whence,

$$-\frac{1}{2} \triangleleft^2 \sigma = V \triangleleft \tau ,$$

and

$$-\frac{1}{2}\sigma = \triangleleft^{-2} 0 + \triangleleft^{-2} V \triangleleft \tau .$$

This contains the solution of the problem, treated by Helmholtz, to determine the linear velocity of each fluid particle, when the angular velocity is given.

4. Mathematical Notes. By Professor Tait.

The following self-evident propositions were employed for the deduction of several curious consequences—

(*a.*) $$4x = (x+1)^2 - (x-1)^2 ,$$

or, $$x^3 = \left(\frac{x(x+1)}{2}\right)^2 - \left(\frac{x(x-1)}{2}\right)^2 ,$$

or, "Every cube is the difference of two squares, one at least of which is divisible by 9."

(*b.*) If

$$x^3 + y^3 = z^3 ,$$

then

$$(x^3 + z^3)^3 y^3 + (x^3 - y^3)^3 z^3 = (z^3 + y^3)^3 x^3 .$$

This furnishes an easy proof of the impossibility of finding two integers the sum of whose cubes is a cube.

Monday, 4th April 1870.

The Hon. Lord NEAVES, Vice-President, in the Chair.

At the request of the Council Professor Wyville Thomson, Belfast, delivered an address on "The Condition of the Depths of the Sea."

Monday, 18*th April* 1870.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. Facts as to Brain-Work; in Illustration of the New and Old Methods of Philosophical Inquiry in Scotland. By Thomas Laycock, M.D.

A few words in explanation are needed. In my summer course of lectures on Medical Psychology and Mental Diseases delivered in the University, I have to investigate the human mind in its practical relations to the body, and especially I have to teach how each influences the other, so that the physician, or any intelligent person, may be able to modify these relations beneficially. The starting-point in these inquiries is the fundamental fact of experience, that no changes in the mind or the consciousness of whatever kind can or do arise, or continue, without a corresponding series of changes somewhere in the brain-tissue. This fact being held as certain as the fact of gravitation, the solutions of the problems to be solved depend upon a knowledge of the relations which the two series of phenomena bear to each other; for which knowledge it is necessary to analyse and classify the varying states of consciousness on the one hand, and the changes in the brain-tissue which correlate them on the other. As to the last mentioned, it is certain that they are vital; they come, therefore, under the sciences of Life collectively termed biology.

But all molecular changes in living tissues, of whatever kind they may be, and consequently those of the brain, can be brought also within the circle of molecular physics, for they can all be resolved into motion of something, whether we designate that something an atom, a molecule, a vortex, a ring, or a centre of force. They are due, therefore, to energy; or, as distinct from *mind*, to motor energy. The Rev. Professor Haughton, M.D., of Dublin University, was led by experimental research to the conclusion, that as much motor energy is expended in brain-work in

five hours as in muscle-work—say by a street-paviour—in ten hours. Although all the changes going on in living tissues may be finally resolved into chemical changes,—a fact well illustrated by Dr W. B. Richardson, and by Professor Crum Brown's and Dr Thomas R. Fraser's valuable researches into the connection between physiological action and chemical composition, lately communicated to the Society,—they are distinct from those induced in inorganic matter by chemical affinity, and hence the need of connoting the energy by the term vital. Now the distinguishing character of that energy, whether manifested in plants or in animals, is adaptation of all motion to ends. Evolved in the brain, this vital energy is manifested as mind, and life is thus spiritualised. I would even venture to say that matter is thus immaterialised, for since all states of consciousness correlate motion of something, it is not the connection of mind with mere ponderable or brute matter we have to discuss, but of mind with adapted motions in infinite variety. All external impressions received through the senses and exciting states of consciousness can be resolved into motions that can be exactly measured, in regard to impressions on the eye and ear, and all internal impressions passing from one part of the brain or of the body to another part, can be resolved also into an energy correlative with motion, termed *vis nervosa*. So that psychology by this method is, in one sense, a department of physics; in a wider sense it is a science or philosophy of nature, and therefore differs essentially from modern physiology, which is only a restricted department of physiology in the true and ancient sense of the word. In fact, the method I adopt is an adaptation of the ancient Aristotelian method to modern philosophy, and in adopting it with me, the Faculty of Arts of the University would only return to a former arrangement of work. Sir William Hamilton observes on this point to the effect, that "Aristotle's treatise *On the Soul* being (along with his lesser treatises on *Memory and Reminiscence*, on *Sense and its Objects*, &c.) included in the *Parva Naturalia*, and he having declared that the consideration of the soul was part of the philosophy of nature, the science of mind was always treated along with physics."*

* Lectures on Metaphysics, vol. i. p. 127.

The cause of this change in Faculty-work was, in fact, the rise of different methods of philosophical inquiry named the reflective, which discarded all observation and experimental research whatever. Sir William Hamilton explicitly taught that the only external condition needed for philosophical inquiry is a language "capable of embodying the abstractions of philosophy without figurative ambiguity,"—a condition not yet attained, however, nor likely to be. "With this one condition," Sir William declares, "all is given; the philosopher requires for his discoveries no preliminary preparations, no apparatus of instruments and materials . . . . it is only necessary that the observer enter into his inner self [and here is truly a figurative ambiguity of language] to find there all he stands in need of."* Hence the reading and writing of books, and discussions of opinions, are the proper results of reflective inquiry. It was to his extreme devotion to the literature of philosophy that was due that lamentable palsy of the sign-making organs, the right hand and speech-muscles, termed *aphasia*, with which he was afflicted, for these were overworked in the acquisition of that immense erudition which distinguished him. The locality of the brain-disorder in these cases is in the anterior lobes, more especially the posterior third of inferior frontal convolution.

Although the principles of the reflective method there laid down by its greatest modern master exclude observation and experimental research, Sir William Hamilton did not neglect physiological inquiry. My own researches into the reflex and unconscious functions of the brain, made twenty-five years ago, were rewarded by his highly valued approval and friendship, because he saw in them the physiological side of his doctrine of "latent" consciousness; but the kind of inquiry he followed was physiological in the restricted sense of a physiology of the human brain, and not in the wider sense of a science of nature. But I do not advocate this restricted method as the best or even a true method of philosophical inquiry, nor do I wish to defend the errors to which it leads. I speak only for my own method as just explained.

Matters being thus, it interested me to read the manifesto of principles and methods which my reverend and respected colleague,

* Lectures on Metaphysics, vol. i. p. 383.

the Professor of Moral Philosophy, gave forth when he took possession of his chair in November 1868, and which he published under the title of "Moral Philosophy as a Science and a Discipline." In this essay he specially criticised the physiological method, and in such a way that the Professor of Physiology thought it expedient to publicly controvert his views. The facts I have to place before the Society having a reference to this criticism, I quote it. Professor Calderwood said, "There are evidences of great activity on the part of upholders of a sensational philosophy, differing only in its modifications from that which Scotland formerly rejected under the leadership of Reid and Stewart. In conjunction with this revival of sensationalism, there is eagerness not only to combine physiological and mental science, but even to question the sufficiency of our investigations regarding the facts of consciousness —to make nerves and muscles the only safe approach to a science of mind,—and to proclaim the necessity of making physiology the basis of psychology. The consequence of this is, not only that mental philosophy is being encumbered with irrelevant investigations concerning such physical processes as mastication and respiration, and such physical experiences as toothache and cramp in the stomach, but we are involved in all the hazard connected with the use of a false method." I gather from this sentence that my reverend colleague, however opposed or misinformed he may be as to the physiological method, certainly means not only to defend and resolutely maintain the sufficiency of the reflective method as laid down by his great master, but to assert its superiority over the Aristotelian method of observation and research. Now, it is upon these points that I join issue with him. I shall select two problems for illustration, taken from my respected colleague's own department, viz., the nature of belief and of personal identity, being guided to the selection by his own declaration, viz., "The supposition that physiology can lead us to philosophy of mind, is doomed to rejection by all to whom it is clear that our personality is not essentially connected with our body, which is only a temporary dwelling," &c. In this condemnation of physiology is included the assertion of the psychological proposition that mind, considered as an energy or principle, is separable from life, and that it only occupies the living body as a temporary tenant. Now, the holders

of this opinion have, in common with the physiologists, a belief in a future life, and follow two methods of inquiry as to that truth of religion, viz., the *confirmatio veri* and the *inquisitio veri*. The spiritualists (so-called) have adopted the latter or scientific method, the orthodox philosophers the former. To this end they state certain propositions as unquestionable. Firstly, that every man assuredly believes he is a mental unity, one, or Ego; secondly, that "our thinking Ego . . . is essentially the same thing at every period of its existence,"—I quote Sir William Hamilton, vol. i. p. 374; and, thirdly, that the evidence upon which these assumed beliefs are founded is sufficient, being that of consciousness itself. In other words, I feel assured that I am one and the same person that I ever was, and therefore I am one and the same. Is this evidence sufficient? Can we rely absolutely and without need of verification upon the veracity of consciousness manifested as belief? To answer this question clearly, it is necessary to understand how beliefs arise and are modified. Now, since according to the fundamental fact that every state of consciousness coincides with corresponding molecular change in brain-tissue, we conclude that all beliefs, being states of consciousness, must be coincident with such changes. Is this conclusion true in fact? First, as to the Ego. A man, like other mammals, is one in body—a corporeal unity—in accordance with the fundamental biological law of organisation *ad hoc*. The belief that he is one, or Ego, bodily, is founded upon his knowledge of this fact. The belief that he is a mental unity, or a thinking Ego, correlates, as I shall shortly show, the unity of cerebral function manifested in the various states of consciousness of the man at any given moment. But the belief that this Ego, whether corporeal or mental, is essentially the same thing at every successive period of a man's existence, includes wholly different phenomena, since it refers to past time, and consequently implies a reminiscence of what it was at some moment of past time, or in past time generally. Now, reminiscence is proveably dependent upon a recording vital process, whereby we are enabled to know in time present by virtue of the so-called association of ideas—what we were, and thought and did in past time. If there be no record or memory, or if there be a record, but no association of ideas so as to induce reminiscence, then there is no knowledge of past mental

states. What is essential, therefore, to belief in continuous personal identity as a mental state, is that consecutive continuity of vital processes which is necessary to reminiscence, and not a continuous consciousness, as is the doctrine of reflective philosophy. Memory in this sense may, and does extend in fact beyond the consciousness, so that changes may and do take place in the consciousness which are due to preceding records made without consciousness, but which not being for that reason recognised as belonging to past mental life, are believed to be intuitive. Memory in the individual from this point of view, and considered as a vital process, has its exact counterpart in what may be termed memory of the species of both plants and animals, in virtue of which consecutive continuity of vital process through the seed or germ is maintained, and ancestral qualities reproduced in offspring.

Such being the philosophy of belief, considered as the result of brain-work, it is not difficult to understand why the philosophy of morals, in so far as it is founded on identity of belief simply, or orthodoxy, and not upon knowledge, is chaotic; nor how it is that all the efforts made to secure identity of mere belief, independently of knowledge of the order of nature, whether by education or otherwise, must fail.

I shall now illustrate these views by morbid or insane beliefs. The reflective philosophy, as is well-known, discards all inquiry into aberrant mental states; with much the same propriety, however, as an astronomer would discard the observation of planetary observation: in the inductive method these are of the greatest value as experiments of nature. By examining every kind of result of the molecular change as manifested by others, and comparing these with our own, we are enabled in truth to study them as directly manifested to our own consciousness. Hence all facta, all writing, all art, and all conduct, however normal or abnormal, are the appropriate facts for inductive inquiry. To illustrate the method in this direction, and at the same time to show the true relations of belief, I place before the Society the portrait of a house-carpenter painted by himself, with a descriptive legend describing himself as three persons, viz.—1. George Elliot, his true personality. 2. "George the Fifth, son of George the Fourth;" and, 3. "The Emperor of the world—the true and lawful God." The reflective philosopher

would think it a sufficient explanation to say that the man is a lunatic. He should remember, however, that he owes this explanation to the physiological method. Formerly, the explanation, according to the reflective method was, and with many still is, that the lunatic is either inspired or else possessed by a spiritual being. The inductive philosophy, starting from the fundamental fact that all states of consciousness of a man, however manifested, cannot be manifested independently of vital processes, lays down the law that in the living man Life and Mind are inseparable, and consequently that the "thinking Ego" is the man himself. Now, although his person is double, whether as to limbs or brains, his corporeal condition of unity is no more affected thereby in a healthy state than the unity revealed in consciousness—the one being the reflex of the other. His two brains act together so as to attain the unity of consciousness, just as his two eyes act in unity of vision; but as he may see double when the two eyes act disjointly, so may he have a double consciousness when the two brains act disjointly. Whether he believes, or whether he doubts that he sees two objects, or that he is one or two persons, depends upon those molecular conditions upon which the belief and doubt of the moment depend. Or, again, just as an object of vision may, from disorder of the corresponding brain-tissue, appear to a man to be something wholly different, as when his friend appears to be the devil, constituting what is termed a hallucination, so his personality, from disorder of the corresponding brain-tissue, may appear to be something wholly different, and he may chance to have an hallucination that he is the devil. It appears probable, therefore, that although a man may have many and various delusions as to his state of mind and body, he will rarely exceed three distinct and fixed delusions as to his personality, viz., one resulting from disorder of each brain acting disjointly, and one from disorder of both acting conjointly. Under the restrictions stated, the result of numerous observations I have made is in accordance with this view. So much for the break-up of the unity of consciousness by brain disorder. It is obvious at a glance that these diversities of belief as to personal identity are associated with brain changes involving memory and reminiscence; otherwise, when Elliot came to a belief in his royal birth and parentage, he would also remember, to the confusion of the belief,

that he is and always has been George Elliot the house-carpenter; or, at least, a reminiscence, however vague, would induce doubt. But no such results followed, and the belief is fixed and unwavering.

These considerations apply to belief only; but to understand the questions at issue better, I shall inquire how a man comes to doubt, and what is essential to as accurate knowledge as he can attain under the circumstances. For this purpose I shall select the state of consciousness known as dreaming. No well-informed inquirer now holds the doctrine that in that state man is inspired, or that the soul or mind acts independently of the body; it is admitted that every such change of consciousness as constitutes dreaming is directly dependent upon molecular changes in the brain-tissue. In accordance with the physiological law already laid down, the dreamer believes in the reality of his dreams, however absurd they may be, and however far removed from the normal conditions the molecular changes. It is only when he awakes, and the normal condition is restored, that he doubts or disbelieves. Now, an analysis of these purely physiological phenomena shows that those states of consciousness which in the waking condition of the brain are either reminiscences or anticipations, have in dreams no true element of time, either past or to come; they are either wholly of the present, or have no true relation either to time or to space. Memory, therefore, as the knowing reminiscence of past states of existence, and judgment as the perception of the future, are abolished. Memory of the past is abolished, on the one hand, because the association of ideas upon which that faculty depends, and which began at some past time, is abolished; while, on the other hand, there is no knowledge of any existing personal relations to time and space, because the senses being shut, there is no perception possible of these relations. Hence the merest phantasms of the imagination, admittedly due to molecular changes induced under these conditions, are received as verities. Reid relates how, on a certain occasion, when he slept with a blister on his head, he believed he was being scalped by Indians. It is only on awaking, when memory, and external perception, and normal associations of ideas are restored, that a true knowledge of the fallacious character of the beliefs can be attained. Hence it is

clear that these conditions are necessary to a right belief in continuous personal identity. These conclusions are strictly applicable to all hallucinations and beliefs of morbid origin. Many persons have delusive beliefs during the waking state as transient as dreams. This is very common in the brain-failure of old age. Delusive beliefs, more strictly insane, may come and go in like manner in the earlier stages of an insanity. I had a patient under my care, in whom they came on only when he was in a heated room, and who could recover from them by the cold douche applied to the face. In cases like George Elliot, the morbid state is best described as a fixed dream. When those molecular changes, which coincide with the mnemonical records of his daily life, of things done, succeed each other, he truly believes he is George Elliot, a house-carpenter; but when the mnemonical records of his dream-life, and which are wholly dissociated from the former, are presented to the consciousness, then the associated personality is presented also, and, for the time being, he believes as firmly he is another person than George Elliot. These delusive states may have every degree of duration. In certain kinds of waking somnambulism, the individual lives an actual life, as two wholly dissociated personalities, for hours or days alternately, the mnemonical records of the two being quite as dissociated as dreaming and waking life; or they may occupy only a few moments, as in the artificial somnambulism induced mesmerically, where the brain has been so acted on that the patient is made to hold the most absurd beliefs,—to believe, in short, whatever he is told is real. In this way Sir J. Young Simpson changed the personal identity of two ladies in regard to the husband of one of them, so that the unmarried believed she was the married, and *vice versa*. From these facts, and they might be multiplied to any extent, it is clear that the notion or belief of personal identity is not due to mind in the abstract, considered as an immaterial substance acting in entire independence of life and organisation, but to mind in the concrete, as inseparably associated, not with brute inert matter, but with the motions and forces upon which life depends. This, I need hardly say, is no new doctrine of philosophy, whether profane or biblical. The earliest record of Scripture affirms that man only became a living soul after the breath of life was breathed into his

nostrils; and St Paul, the philosophic apostle, adopting this view to explain the resurrection, uses the biological analogy of the continuous life of the species of plants through the germ, to indicate how the individual or personal life of man may be continued independently of consciousness, and how it may be evolved into consciousness at some future time, plainly adopting thereby the Aristotelian doctrine of the soul.

Many attempts have been made to verify the separate existence of the soul, whether as a religious dogma or a philosophical doctrine, and, of necessity, all have failed. I have placed before the Society an illustration of these attempts, by the so-called spiritualists, to prove the fact of an independent personal identity. It is a drawing, by a member of an eminent literary family, of the spirit-emblem of a distinguished and much esteemed fellow of this Society. Here are published representations of like emblems, taken from Mrs Newton Crossland's "Light in the Valley." The seeress, we are told, who beholds these mystical appearances, describes them as appearing to her in colours of liquid light, with the utmost clearness, more rich and radiant than earthly jewels. These emblems are usually seen to be situate behind the persons to whom they belong, the centre of the emblem rising just above the head, and occupying a circumference of several feet. They are the badges by which persons are recognised in the spirit-world, even while they remain on earth. To the production of these emblems a belief in the separate existence of "spirits" is essential—doubt, like the waking from a dream, either prevents or dispels the phantasies. Physiologically they differ in no respect from the delusions of George Elliot, or of dreamers. The verification of any belief means the investigation of the order of nature, so as to determine whether the conclusions presented to the consciousness as brain-work coincide with the natural order of events. To those who are confident that they can assuredly believe in their own eyes, the sun undoubtedly moves, and the observer is motionless, but a verification of the conclusion shows that the motion is in the observer, and the sun is motionless. Now, when a spiritualist attempts to verify his belief in spirits, he ignores the fact that his belief is due to molecular changes out of, at least, direct relation to any spiritual influence, except that which constitutes his own

spiritual nature, and is thus led to esteem the mere phantasms of his own imagination as proof of external agencies which may exist, but which, by the terms of the hypothesis, cannot be verified. Resolved into their ultimate elements, all the so-called proofs of spirit-life, when stated *bona fide*, are simply presentations to the consciousness of the inquirer's own brain-work, as delusive as those of the lunatic or the dreamer. It has been commonly said that this class of inquirers are, for the most part, either of weak mind, or credulous, or ignorant. But this is not so. Here are delineations of the od-force, as investigated by Baron von Reichenbach, a skilled scientific inquirer. He never saw what is here represented as the manifestations of the od-force, he simply shows what was described to him as such by hysterical and morbidly nervous women; and if they be true as descriptions, they are only representations to the consciousness of phantasmal brain-work. Some of these so-called spirit operations are instructive illustrations of æsthetical automatic action of a cultivated brain. The emblem of a fellow of this Society, drawn by a person of high culture, is contrasted well with the uncouth mystical emblems of an uneducated female lunatic before me. I was assured by my late friend David Ramsay Hay, and no one was more competent to judge, that it is exactly true to the geometrical principles of form and colour.

In the delusions of George Elliot we have an illustration of another interesting result of brain-work, the ideational evolution of the intuition of the infinite, a subject so much and so earnestly discussed by reflective philosophers, and which is equally as capable of biological illustration as the preceding.

2. On Change of Apparent Colour by Obliquity of Vision. By Robert H. Bow, C.E., F.R.S.E.

I discovered the peculiarity of chromatic vision, which is the subject of this paper, in the month of January, when conducting some experiments upon the perfection of definition at different parts of the retina; and I may introduce the subject by first referring to these experiments.

In the case of ordinary sensation seated in the skin, there are

two offices performed by the nerves—first, that of informing the mind of the fact of the contact or impression being made; and, second, that of giving more or less minute information as to the locality of the sensation. Professor Weber experimented upon the latter power, by testing the least distance apart at which two objects touching the skin of any part of the body could be felt as two distinct sensations; and, as you are aware, this tactile power bears no constant proportion to the mere power of feeling a sensation of contact. For instance, the back of the hand is perhaps more sensitive to a simple contact than the tip of the finger, but Weber found that the points of contact are required to be fourteen times further apart at the back of the hand than at the tip of the finger, before they can be distinguished as separated.

Now, a very strong analogy exists between these two functions of ordinary sensation and corresponding offices of the retina. Objects seen obliquely are not strikingly different in brightness from the same seen in the direction of the optical axis, but the power of definition (apart altogether from mere optical causes) varies immensely. I attempted to investigate this defining power for different parts of the retina by a method exactly analogous to Weber's—namely, by inspecting two white spots on a blackened card, and determining, for different angles of obliquity and direction, the greatest distance from the eye at which these spots could be detected to be double. But I soon found that, when the vision is very oblique, there is a puzzling feeling of uncertainty as to the result; and it occurred to me to assist the judgment by substituting for the white spots objects of contrasting colours.

On attempting to put this idea into practice, I made the important discovery, that when coloured objects are inspected under oblique vision, the colours are at the same time reduced in intensity, and changed in character: thus, *scarlet* becomes successively orange, yellow, and whitish-yellow, according to the obliquity; *green*, of a medium character, tends to become white, and *violet* to become blue.

In experimenting upon the subject, it is best to place the coloured object obliquely on the nasal side of one eye, the other eye being closed; much smaller angles of obliquity bring about the phenomena when seen on this side of the eye, and we get rid of any complicity

with the insensitive spot where the optic nerve joins the retina. I may point out here, however, an experiment that shows the general peculiarity, and also the excess of change that takes place when the object is on the nasal side compared with the other. Against a dark-coloured wall hold up, at arm's-length, an orange-coloured object of three or four inches in diameter. We suppose it held by the right hand; then turning the face rather towards it, look at a point in the wall eighteen or twenty inches to the left of the object; and now closing the eyes alternately, it will be observed that, when the right eye is open, the object will appear of nearly its full orange colour, but when the right eye is closed and the left opened, the object will assume a pale, sickly, yellow tint; and if the point in the wall be taken further from the object, the colour seen by the left eye will approach nearer to white. To cause the same amount of change to the right eye, the obliquity must be very much greater. Another mode of conducting the experiment, as depending upon the contrast of effect upon the two sides of the eye, is this: Choose two objects of the same colour, place these two or three inches above or below a mark on the wall, close one eye, and with the hands withdraw the objects equally away on either side from the central position, the eye being rivetted to the mark on the wall; it will then be noticed that, relatively, the object on the nasal side of the observing eye undergoes a rapid change of tint or colour. But, it may be repeated, the most satisfactory mode of examining the changes is to use one eye and observe with the coloured object on the nasal side of it, the eye being held steadily upon a mark, which may or may not be of the same colour as the object. Observed in this way, the following changes will be presented:—

First. The colours lose more or less their chromatic intensity, and approach nearer to white or black, according as they are placed upon a dark or light ground. But extreme red is especially marked as losing illuminative power, as well as chromatic character. Ultramarine blue, on the contrary, appears to lose very little by oblique vision; it assumes a lighter blue hue.

Second. The colours undergo a change of chromatic character.

a. Brilliant scarlet, painted with biniodide of mercury and gum arabic.—This, when placed on a dark ground, and observed at an obliquity of about 30° on the nasal side,

appears orange; at 40° to 50° it looks of a somewhat meagre yellow, beyond this a pale yellow. As seen at the outside of the eye, the orange only appears when the obliquity reaches 80°, and the yellow at 90°.

b. Some *orange* colours show the change very markedly to yellow, and to nearly white.

c. *Emerald green.*—This, at 40°, becomes nearly white, generally yellowish.

d. *Ultramarine.*—This is very persistent, visible at 40° as a blue.

e. *Pink*, of a purplish cast.—This in day-light, when placed on a white ground, appears—even at a very moderate obliquity—a purplish *blue*; if placed on a black ground, it assumes a lavender *blue* colour.

This change of purples and pinks to blue is one of the most striking; perhaps the best way of witnessing it is to use two thicknesses of cobalt blue glass, fortified with a pink or purple one, so as to allow both extremities of the spectrum to pass freely. This screen, held before a gas light, appears by direct vision of a fine pink colour, but by a moderate obliquity it is reduced to a bright blue.

f. A bluish-*green* glass, held in front of a gas light, appears to become blue by oblique vision.

g. A yellowish-*green* glass becomes by oblique vision more decidedly yellow.

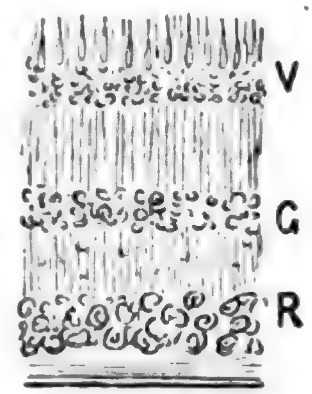
Remarks and Speculations on the Phenomena.

Under oblique vision the purples or pinks become blue, and the extreme red becomes dull. It would appear, therefore, that towards the margins of the retina the sensation of *blue* is less reduced in intensity than that of *red*, and a step in the explanation of the results is this: the red in the purple or pink becomes a dull orange or yellow under oblique vision; this gives rise to the sensation of white light when combined with a part of the blue, and reduces the remaining part of the blue to a paler cast. The same explanation applies to a blue-green becoming blue—the green becomes white or pale yellow under oblique vision, and so dilutes the blue ingredient to a paler shade.

The second observation that may be made upon the results is, that by oblique vision the various colours are seen under the same conditions as in the most common form of colour-blindness, wherein there are really only two colour-sensations, the upper half of the spectrum, from blue-green up to violet, and including pinks and purples, appearing *blue;* and the lower half, from yellow-green down through yellow, orange, and scarlet, to bright red, appearing *yellow;* and in such colour-blindness the extreme red is frequently very dull. We may, therefore, expect the discovery of some similarity in the conditions of the central part of the retina of an eye affected with this form of colour-blindness, and the marginal parts of the retina of a normal eye.

Before concluding, I would venture to connect the discovery with an existing theory of colour-sensation, as it may help to establish that theory, should a prediction the connection leads to be found to be correct.

The figure here given shows a section of part of the retina (Kölliker). Now, it has been suggested that each of the layers V, G, and R, is receptive of the sensation of light,—the layer V being affected by the more refrangible rays blue and violet, R being affected by the less refrangible yellow, orange, and red, while the central layer G is affected by the central parts of the spectrum, blue, green, yellow, and orange; and this would account for the approximate achromaticity of the eye, for when the eye is arranged for the most acute vision, the focus of blue rays will correspond with V, of green rays with G, and of scarlet rays with R.



But it is well known that the eye does not see any colour quite purely; there is always white light present, or, in other words, one of the layers, V, G, or R, cannot be agitated or excited without the others partaking to some extent in the excitation. Now, there is a probability that the degree of freedom with which one layer may transmit its special sensation without one or both of the others participating, to an important degree, in the excitement, depends in part upon the maintenance of a considerable interval between the layers. Let us then imagine the interval between G and R to

become more or less perfectly obliterated, and it is evident that no simple sensation of red or green could be felt, but only a colour-sensation, which corresponds with the excitement of both of these layers, which is *yellow*. It may, therefore, be worth the attention of anatomists, skilled in working with the microscope, to ascertain if any decided reduction of the interval G to R takes place towards the margins of the normal retina, or has place in the central part in eyes that have shown, during life, the commonest form of defective vision of colour; we should also expect a reduction of the interval V to G, but to a less decided degree. In the case of an eye completely colour-blind, we should look for the coalescence of the three layers into one, unless the defect were accounted for by the absence or paralysis of two of the layers.

The following motion by Mr Sang was considered:—

1. Every Communication intended for the Society shall be submitted to the Council, and passed by them as not containing anything objectionable, before being mentioned in the Billet.

2. The Society shall not take up any matter which has not been announced in the Programme, except in cases of extreme urgency.

The motion was not adopted, as the Society thought that Mr Sang's views were already embraced in the printed regulations for the order of business.

Monday, 2d May 1870.

DAVID MILNE HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Remarks on the Theories of Capillary Action. By Edward Sang, Esq., F.R.S.E.

That theory of capillary action, which seems to have satisfied the greater number of physicists, is founded on the assumption that the particles of a fluid are separated by distances immensely

great in comparison with their magnitudes, and that these particles attract each other,—the sphere, however, of their attraction extending to a distance infinitesimally small in comparison with the observed disturbances of the fluid-level.

The accommodation of this theory to the actual phenomena is accomplished by long operations, comprehensible only by those who are familiar with the higher calculus. The object of the present paper is to examine this theory in the light afforded by a general knowledge of the leading laws of mechanical science. For this purpose, the author proceeds to analyse the ordinary phenomena of the rise of water round a piece of clean glass which has been plunged into it. Assuming a fluid particle situated upon the inclined surface, he observes that, according to the hypothesis of an infinitesimally small sphere of attraction, this particle is beyond the direct influence of the glass; the only other influences to which it is subjected are gravitation and the attraction by the adjacent fluid particles.

Now, according to this same hypothesis, the particle is attracted by that part of the fluid which is within a small sphere described around it; but the curved surface, having its radius of curvature infinitely greater than the radius of this sphere, may be regarded as *flat* within the range of attraction, and therefore the solicitation, to which the particle is exposed, must be exerted in a direction normal to the surface. By a more minute examination, the author shows that, if the radius of the sphere of attraction be reckoned as a differential of the first order, any deviation from normality must belong to the third order of differentials—that is, must be of an order infinitesimally smaller than the infinitesimally small sphere of attraction.

Thus the only two solicitations to which the particle can be subjected are, the attraction of the fluid exerted in a direction normal to the surface, and gravitation. Now, it is impossible that the resultant of these two solicitations can be normal to the surface; but no fluid can be in repose if the attraction exerted upon a particle at its surface be not normal to that surface, wherefore, the author of the paper concludes, the infinitesimally-small-sphere-of-attraction-hypothesis is untenable.

On considering the hypothesis of attraction generally, the author

proceeded ro remark that, in order to prevent the condensation which would result, we must suppose some resistance to the farther approach of the particles, which we may typify by a repulsion; and that these tendencies—the attractive and the repulsive—must be in equilibrium. A theory, then, which takes into account only one of these equilibrated antagonists, must necessarily be defective. And since, in all cases, the attraction supposed to exist between two sets of particles must necessarily be resisted by actions between them, there can be no tension like that which has been supposed to be exhibited by the superficial films of fluids.

2. Theory of Construction of the Great Pyramid. By John Christie, Esq. Communicated by the Rev. W. Lindsay Alexander, D.D.

In his early investigations on the principles of construction of the Great Pyramid, the author was forcibly struck with the following fact—viz., that if a perpendicular be drawn through the apex of the Pyramid to its base, and the unit angle with the horizontal thrown up from the base on each side of this perpendicular, the angle comes out on the faces of the Pyramid at the openings of the north and south ventilating air-channels; at the same time he was led to the conclusion that one-tenth of the base line, and the same tenths also applied to the faces of the Pyramid, ruled the entire structure. Following this out, and having erected perpendiculars on each of these tenths, and horizontals from each of the facial divisions, the first step procured a grand central point—viz., in the centre of the grand gallery; the next step was to account for the position of the King's Chamber, by the intersections of the first and second circles—used in the construction of the Pyramid, as shown in Diagram No. 1. Having thus obtained a central perpendicular for the King's Chamber, he then made use of the direction of the celestial equator, and where it cut the last-named perpendicular, a third point was gained as a centre for the third circle, which completes the Pyramid in its external form. He next found, that by connecting the south outcrop of the air-channel with the north corner of the base, a

parallel was gained for the angle of the grand gallery. By drawing a horizontal line between the two air-channel mouths, and dropping perpendiculars from these to the base, two oblongs are formed, one on each side of the axis; the diagonals of each of these being the unit angle.

The astronomical bearing of the Pyramid seems manifestly to be indicated in the sections of the King's Chamber. In the section of it in its breadth, the chamber is filled up by—first, a section of the Pyramid itself, the base of which is the floor line of the chamber; the space above, as regards height, being filled by an equilateral triangle, its angles 60°, corresponding as they do with the direction of the celestial equator, 60° seem to point with threefold force to the fact that the Pyramid has a direct reference to the sun.

The same is twice repeated in the section of the King's Chamber in its length, the length of the chamber being exactly twice its breadth. Another very marked reference of the same kind occurs in the position that the Queen's Chamber bears to the King's Chamber. If an equilateral triangle, whose apex is in the centre of the floor of the King's Chamber, be constructed, having its base in the base line of the Pyramid, the centre of the floor of the Queen's Chamber will be found to be exactly in the middle of the north limb of this triangle, other instances are also shown to be regulated by the equilateral triangle.

The unit angle regulates the length and height of the King's Chamber, the space between it and the ante-chamber, the form of the ante-chamber, and the distance to the great step, also the interior length, breadth, and depth of the much-abused granite coffer.

Coffer Unit Block.

Breadth,	$\frac{1}{15}$ part of interior length of coffer.			
Height,	$\frac{1}{5}$	,,	,, depth	,,
Thickness,	$\frac{1}{5}$	,,	,, breadth	,,

90 of these cover one side of coffer.
90 ,, bottom ,,
450 exactly fill coffer.

The shape of this block is regulated by the unit angle in its top sides and face, and consequently conserves the Pyramid facial

angle, which it would not do had its length, breadth, or thickness been different, in which case the complement of these blocks would have been too large or too small for the coffer content.

Record of Physical Facts—Water-Levels.

The King's Chamber is a noted index of these. That this was intended by the Pyramid builder seems to be demonstrated by the fact of the rock on which the Pyramid stands having been scarped down to the level of the Pyramid's base, so as to procure a horizontal line midway between the external physical fact to be recorded, and the internal index of that fact contained in the King's Chamber, serving as it did, at the same time, astronomical purposes, neither of which would have held good had the rock not been so scarped down.

These water-levels have been previously indicated by other modes than those by which they are now illustrated. It will be observed that the circles used to indicate them have also peculiar references to other parts of the Pyramid besides those they bear to the King's Chamber. One marked instance may be noted here. The circle, which indicates the High Nile-level, touches the floor of the King's Chamber in the centre, and also indicates the angle of the floor of the grand gallery. Reference may also be made here to one of the circles used in the construction of the chambers and passages, it being of a very marked and significant character. This circle has its centre in the Pyramid's base, in the point where the "direction of the celestial equator" cuts the base, its radius is the prime central point in the centre of the grand gallery, and in its course it touches—1*st*, The mouth of the entrance passage; 2*d*, The step leading down to the Queen's Chamber; 3*d*, The "bottom of well" in the lower part of descending passage; 4*th*, Bounds the Low Nile-level; and 5*th*, Where it cuts the lower portion of the direction of the celestial equator, the High Nile-level. The difference between the mean Nile-level and the mean sea-level is indicated by an equilateral triangle, the apex of which is in the mean sea-level, and the base the mean Nile-level, the length of the latter being contained between two perpendiculars—the first from the north corner of the Pyramid's base, the second from the first remarkable perpendicular joint in the entrance passage.

Independent Methods of Constructing the Great Pyramid Externally.

1*st*. Give a horizontal line. Bisect it, erect perpendiculars at both ends and also from the centre, from one of the ends throw up the unit angle with the vertical, and through the point where the angle cuts the opposite perpendicular draw a horizontal line, an oblong will thus be formed, the diagonal of which is the unit angle, join the top of the central perpendicular with the lower corners of the oblong, and the Pyramid is complete.

2*d*. Given a vertical line, the radius of a circle, at right angles, through the centre of circle, draw a horizontal line, bisect the vertical line, and throw down the unit angle with the vertical from both sides of the vertical at its bisection, through the points where these cut the horizontal line, join the extreme end of the radius, and the Pyramid is complete.

The Diagrams submitted to the Society were as follow:—

Diagram No. 1.—Construction of the Great Pyramid in its external angles, its chambers and passages by the unit angle, and one-tenth of the base, on a given horizontal line.

Diagram No. 2, one-sixteenth of the full size.—Sections of the King's Chamber, in its length, and also in its breadth, showing how it is regulated by the unit angle, &c.

Diagram No. 3, one-half of the full size.—Sections of the granite coffer in its length, and also in its breadth, showing how it is regulated by the unit angle and conserves the Pyramid facial angle.

Coffer unit block, in further illustration of Diagram No. 3.

Diagram No. 4, one-sixteenth of the full size.—Section of the King's Chamber in its breadth, the ante-chamber, great step, and south end of grand gallery, showing that the space between the King's Chamber and ante-chamber, the form of the ante-chamber itself, and the distance to the great step, are all regulated by the unit angle; showing also the references between a portion of the

chambers of construction and the overlappings of the grand gallery.

Diagram No. 5.—Independent method of constructing the Great Pyramid in its external angles from a unit angle oblong.

Diagram No. 6.—John Taylor's theory of the reference the Great Pyramid bears to the circle, with Professor C. Piazzi Smyth's amplification of the same, and further amplification by the author.

3. On the Structure of Tubifex. By W. C. M'Intosh, M.D.

The paper consisted of a detailed account of the external form; the arrangement of the body-cavity and its walls; the perivisceral space and corpuscles; the digestive, circulatory, and generative systems.

It was specially mentioned, in regard to the perivisceral corpuscles, that the author was not at all inclined to think that they originated from the glandular fatty coating of the digestive tract and the dorsal blood-vessel. The corpuscles seem rather to be the product of the perivisceral cavity itself and its special (free) contents. This view requires no stretch of ordinary physiological principles, and is quite in keeping with what is found in other groups. In the Nemerteans, for instance, a complex corpusculated fluid is produced within a closed chamber with smooth walls.

The following Gentlemen were elected Fellows of the Society:—

James Sime, Esq.
Thomas Harvey, LL.D.
John Young Buchanan, M.A.
John Hunter, M.A., Belfast.
The Right Hon. The Lord Justice-Clerk.
The Hon. Lord Gifford.

Monday, 16th May 1870.

DR CHRISTISON, President, in the Chair.

On taking the chair the President alluded to the loss which the Society had sustained by the death of Sir James Y. Simpson, Bart.

The following Communications were read:—

1. Primitive Affinity between the Classical and the Low German Languages. By the Hon. Lord Neaves.

(*Abstract.*)

In this paper the author adverted to the limited attention that was paid in this country to comparative philology, and noticed the principles it had developed and the progress it had made elsewhere of late years.

In illustration of the results thus attained in the Aryan or Indo-Germanic languages, he took as familiar examples the affinities that could be traced between the Latin and the Old English tongues, viewing the Latin as a type of the earlier branches of the family, including the Greek and Indian; and the English as a type of a later branch, consisting chiefly of the Low German dialects. The affinities referred to were not those which connected Latin with English through the romance languages, but those which subsisted between Latin and vernacular English, and which must have arisen from a prehistoric identity or connection.

The chief law regulating these affinities was what is commonly called Grimm's law, but which is subject to various limitations and exceptions.

The affinities between words in cognate languages which have had no historic connection are to be found out—1st, by studying the general law of letter-change prevailing between the primary and secondary branches of the family; and 2d, by finding out the peculiarities or idiosyncrasies of the individual languages sought to be compared; for each language has a character of its own, and

both Latin and English have strong peculiarities distinguishing them from other languages, which help to conceal cognate words from each other, and which must be mastered before the double disguise can be seen through.

He exemplified these views by detailed instances, and concluded by urging that all nations of the Aryan race ought to be regarded as susceptible of the highest culture, and that the good hopes might be entertained of their being all raised to as elevated a state of Christian civilisation as the best of them had attained.

2. On the Genetic Succession of Zooids in the Hydroida. By Professor Allman.

In this communication an attempt was made to express by means of formulæ the various modifications presented by the life series of the *Hydroida*. It was also shown that there existed among the *Hydroida* both centripetal and centrifugal forms of development. These were compared with one another, and numerous analogies between the hydroid gonosome and the inflorescence of plants were demonstrated.

3. On Green's and other Allied Theorems. By Prof. Tait.

(*Abstract.*)

In this paper an attempt is made to supply, at least in part, what the author has long felt as a want in the beautiful system of quaternions, so far as it has yet been developed. To apply it to general inquiries connected with electricity, fluid motion, &c., we require to have means of comparing quaternion-integrals taken over a closed surface with others extended through the enclosed space—and of comparing integrals taken over a non-closed surface with others extended round its boundary. The author recently found that he had already, in the *Quarterly Math. Journal*, and in the *Proc. R. S. E.*, furnished the means of attacking the problem.

By very simple considerations it is established that

$$\iiint S\nabla\sigma\, d\varsigma = \iint S.\, \sigma U\nu\, ds,$$

where ∇ is Hamilton's operator,

$$i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

σ is any vector-function of the position of a point, $d\varsigma$ an element of volume, ds an element of surface, ν the normal at ds; and the integrals are extended respectively through the content, and over the bounding surface, of a closed space Σ.

From this equation Green's Theorem is deduced in the form

$$\iiint S.\nabla P\nabla P_1 d\varsigma = -\iiint P_1\nabla^2 P d\varsigma + \iint P_1 S.\nabla P U\nu\, ds,$$
$$= -\iiint P\nabla^2 P_1 d\varsigma + \iint PS.\nabla P_1 U\nu\, ds.$$

Some sections are devoted to the representation of

$$\iiint q d\varsigma$$

(where q is any quaternion whatever) by a surface-integral, and the arbitrary part of the solution in the equation

$$\iiint \tau d\varsigma = \iint ds\, S(U\nu\nabla^{-1})\tau,$$

where τ is any vector, is explained.

It is next shown that, if ρ be the vector of a point, σ and ν as before, we have the equation

$$\int S\sigma d\rho = \iint S.\nabla\sigma U\nu.ds,$$

expressing an integral taken over a limited and non-closed surface by another taken round its curvilinear boundary. That some such representation is possible is obvious from the fundamental theorem above, which shows that for a closed surface

$$\iint S.\nabla\,\sigma U\nu.ds = \iiint S\nabla^2\sigma\, d\varsigma = 0,$$

and therefore the surface-integral must have the same value (with a mere change of sign depending on the difference between *outside* and *inside*) for the two parts into which the surface is divided by any closed curve drawn upon it.

Other theorems of a similar character are given, such as

$$\int V\sigma d\rho = -\iint ds\, V.(V.U\nu\nabla)\sigma,$$

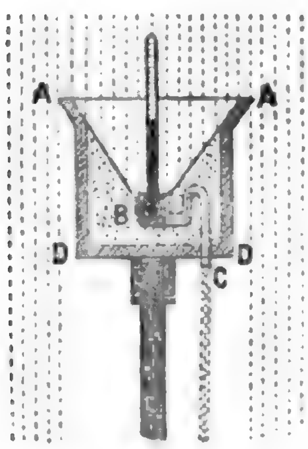
and

$$\int P d\rho = \iint ds\, V.U\nu\nabla P,$$

which, in fact, contains the two preceding.

4. Proposed Method of ascertaining the Temperature of Falling Rain. By Thomas Stevenson, F.R.S.E., Civil Engineer.

A friend informed me some time ago that the late Principal J. D. Forbes had often noticed that a long continuance of rain resulted in a track of cold weather. Principal Forbes attributed this fact to the rain having a lower temperature than the atmosphere through which it fell. It does not appear, however, that he made any observations to determine the truth of his hypothesis, and as the subject is of considerable importance in other meteorological questions, it occurred to me that a simple instrument could be made for ascertaining the temperature of falling rain. This instrument, a rough model of the funnel of which was lately shown at a meeting of the Scottish Meteorological Society, is represented in the accompanying diagram, in which A B C is a conical funnel of thin glass, terminating in a small tube deep enough to contain the bulb of a thermometer, and recurved so as to form an off-let or waster. A D D A represents a box of wood into which the glass funnel is inserted, the space between the glass and the wood being carefully filled with sawdust or any other bad conductor of heat. The rim of the funnel should be bent over the upper edges of the box, so as to prevent the possibility of rain lodging itself among the sawdust.* The rain-drops intercepted by the funnel will pass off through the bottom of the box by the tube C.



By this or some such simple arrangement the temperature of any heavy fall of rain may be ascertained with tolerable accuracy. It is, of course, necessary that a dry bulb thermometer, properly protected by a louvre boarded box, should be observed simultaneously with the *rain thermometer*.

The difference of temperature between the air and rain could

* It may be found better to carry the tube, at the second curve, horizontally through the side of the box instead of downwards.

also be ascertained by means of an instrument on the principle of Leslie's differential thermometer, one bulb of which would be placed at the bottom of the glass funnel, while the other would be protected from the rain. In this way the differences of temperature would be constantly shown by means of a single instrument.

The following Gentlemen were elected Fellows of the Society:—

James Watson, Esq.
The Hon. Lord Mackenzie.

Monday, 6th June 1870.

Dr CHRISTISON, President, in the Chair.

The Secretary read the following letter from Professor W. J. Macquorn Rankine:—

Diagrams of Forces in Framework.

To the Secretary of the Royal Society, Edinburgh.

Sir,—As Mr Clerk Maxwell, in a paper lately published in the Transactions of this Society, has done me the honour to refer to me as having been the first to show how to combine in one diagram a system of lines representing the directions and magnitudes of all the forces acting in a given frame, I wish to put on record, in the Proceedings of the Society, the time and manner of my first publication of the method in question. It was in the year 1856, in a lithographed synopsis of lectures which I delivered in the University of Glasgow, entitled "Mechanical Laws, Formulæ, and Tables." Copies of that synopsis were distributed to the students of my class, and to a few men of science.

I beg leave herewith to send for presentation to the Society a copy of the first part of that synopsis, and regret that at present I am unable to make up a complete copy. The construction of diagrams of forces for unbraced frames is shown at p. 7, and for braced frames at p. 8.

The next publication of the method took place in 1857, in the

article "Mechanics Applied," of the "Encyclopædia Britannica," eighth edition; and the next again in 1858, in a work of mine entitled "A Manual of Applied Mechanics."

Mr Clerk Maxwell made a material improvement in the mode of applying the method to *braced* frames, which he published in the "Philosophical Magazine" for 1866, and described to the Dundee meeting of the British Association.—I am, Sir, your most obedient servant, W. J. MACQUORN RANKINE.

GLASGOW, *2d June* 1870.

The following Communications were read:—

1. On Spectra formed by Doubly Refracting Crystals in Polarised Light. By Francis Deas, Esq., LL.B., F.R.S.E.

The instrument used in the experiments forming the subject of this paper was a spectrum microscope, to which a polarising apparatus is attached, consisting of two Nicol's prisms, each of which is capable of being turned through any required number of degrees.

The first part of the paper relates to the spectra obtained when one or more thin films of mica or selenite are interposed between the polariser and the dispersion prisms, the light being subsequently analysed.

The method employed was, having first determined the axes of the films, to place them on the stage of the instrument which is rotatory, and to adjust them at various angles to the plane of polarisation.

The general appearance presented, may be described as being a more or less continuous spectrum, interrupted by one or more well-defined black bands, not unlike the ordinary absorption bands produced by many chemical substances.

The bands have in many cases a curious movement along the length of the spectrum as the analyser is turned. Sometimes a band may be observed to split into two halves, which move in opposite directions, and unite with other bands which advance to meet them.

In all cases a set of complementary bands is obtained when the plane of analysation has been turned through 90° to that of polar-

isation. The positions and relative movements of the bands depend partly on the thickness of the films, partly on the inclination of their axes to one another, and to the planes of polarisation as detailed in the paper.

Curious varieties of the movements are obtained by circularly polarising the light before or after its passage through the film.

Very beautiful results were further obtained by substituting a double image prism as the analyser. When the spectra thus obtained are superposed, the bands are no longer black, but coloured, each band in the one spectrum being of the colour of that part of the other spectrum on which it is superposed, while the adjacent colours are those arising from the blending of the two spectra.

To obtain these effects in perfection, however, certain adjustments of the apparatus must be attended to, which will be found described in the paper.

The second part of the paper relates to the effects obtained when a section of a doubly refracting crystal, cut perpendicular to its axis, so as to give the well-known systems of coloured rings, is substituted for the mica or selenite in the former experiments.

The crystal must in this case be placed, not upon the stage, but immediately over the eye lens of the instrument, and between it and the analyser. The entire length of the spectrum is now seen intersected by a system of black arcs, accompanied by two or more brushes, which are black or coloured according to the position of the analyser.

Interesting effects are produced upon the rings by interposing films of mica of different thicknesses, so as to polarise the light either circularly or elliptically; the mode in which the black and coloured rings alternate and change places during the revolution of the analyser depending on the thickness of the film used.

The effect of the rings, when viewed through a double image prism, is strikingly beautiful. Exquisite patterns resembling tessalated pavement, chain armour, &c., may thus, with a little ingenuity in the mode of arrangement, be produced by the interlacing systems of rings.

2. On the Heat Disengaged in the Combination of Acids and Bases. Second Memoir. By Thomas Andrews, M.D., F.R.S., Hon. F.R.S.E.

(*Abstract.*)

In the beginning of this paper the author recapitulates the five fundamental laws of the heat of combination, which he had deduced from his previous researches, and which form the subject of several memoirs published in the Transactions of the Royal Irish Academy and of the Royal Society of London, from 1841 to 1848. They are as follows:—

Law 1.—The heat disengaged in the union of acids and bases is determined by the base, and not by the acid; the same base producing, when combined with an equivalent of different acids, nearly the same quantity of heat; but different bases, different quantities.

Law 2.—When a neutral is converted into an acid salt by combining with one or more atoms of acid, no change of temperature occurs.

Law 3.—When a neutral is converted into a basic salt by combining with an additional proportion of base, the combination is accompanied with the evolution of heat.

Law 4.—When one base displaces another from any of its neutral combinations, the heat evolved or abstracted is always the same, whatever the acid element may be, provided the bases are the same.

Law 5.—When an equivalent of one and the same metal replaces another in a solution of any of its salts of the same order, the heat disengaged is always the same, but a change in either of the metals produces a different disengagement of heat.

The concluding part of the elaborate memoir of MM. Favre and Silbermann, on the heat disengaged in chemical actions, which appeared a few years later, is chiefly devoted to a repetition of the experiments already published by the author. They state that they consider the fourth law, which asserts the equality of thermal effect in basic substitutions, to be fully established; but they dissent from what they consider to be the enunciation of the first law, and infer from their own experiments that the organic acids—

oxalic, acetic, &c.—disengage sensibly less heat in combining with the bases than the nitric, hydrochloric, and other mineral acids. In his first memoir (published in 1841) the author of this communication had, on the contrary, found that the oxalic acid disengages quite as much heat as the nitric and hydrochloric acids, when it combines with the bases, and this property of oxalic acid he always regarded as the key to his whole investigations on this subject. He therefore considered it important to institute a new set of experiments in order to settle the question. These experiments, which were performed with great care, and with accurate instruments, are fully described in the present communication. The results confirm the general accuracy of his original experiments of 1841. They show that oxalic acid, far from disengaging sensibly less heat than the hydrochloric and nitric acids in combining with the bases, actually disengages a little more heat than either of those acids, when it combines with potash, soda, or ammonia. The following extract from a table given in the present communication will illustrate this point:—

Acid.	Potash.	Soda.	Ammonia.
Oxalic, . .	3°·058	3°·040	2°·648
Hydrochloric, .	3°·021	2°·982	2°·623
Nitric, . .	2°·993	2°·929	2°·566

The original experiments of the author, according to which oxalic acid stands, as regards thermal action, in the same rank as the phosphoric, nitric, arsenic, hydrochloric, hydriodic, boracic, and other mineral acids (with the exception of the sulphuric acid), are thus completely confirmed. The new experiments also agree with the former ones in showing that sulphuric acid disengages about $\frac{1}{8}$th more heat, and a group of acids comprising the tartaric, citric, and succinic acids, about $\frac{1}{30}$th less heat than the mean of the other acids. The results are fully discussed in the present memoir, and the influence of extraneous circumstances considered, which in this, as in other similar physical inquiries, disturb in all cases to a certain extent, and in some cases considerably, the experimental indications, and render them only first approximations to the general laws they are designed to illustrate.

3. Note on Professor Bain's Theory of Euclid I. 4. By Wm. Robertson Smith, M.A., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

In a paper communicated to this Society last session, I pointed out that the proof of Euc. I. 5, given by Mr Mill, is unsound; endeavouring, at the same time, to show that this is no mere accident, but that it is impossible to give a mathematically correct analysis of the processes of Synthetic Geometry on any theory that holds figures to be merely illustrative, and does not admit that intuition in the Kantian sense—*i.e.*, actual looking at a single engraved or imaginary figure—may be a necessary and sufficient step in a demonstration perfectly general. I now venture to draw the attention of the Society to the confirmation which I conceive that this argument derives from the way in which Euc. I. 4 is treated by Professor Bain in his recent "Logic"—a book which, on the whole, is based on Mr Mill's principles, and which is mainly original in an attempt, which I cannot regard as felicitous, to bring these principles into closer contact with the special sciences, especially with Physics and Mathematics.

It will be remembered that Mr Mill, undertaking to demonstrate Euc. I. 5 from first principles, has to supply, in the course of his proof, a demonstration of Euc. I. 4, and it is in the attempt to give to this process the form of syllogistic inference from Euclid's axioms that he errs. Professor Bain does not attempt to defend the blunder of his predecessor. He admits that Euclid's proof cannot be reduced to a chain of syllogisms. But, instead of surrendering Mr Mill's theory of mathematical reasoning, he concludes that Euclid has not demonstrated his proposition—that the superposition which he enjoins is only an experiment, and that "if his readers had not made actual experiments of the kind indicated, they could not be convinced by the reasoning in the demonstration." *

Now I believe, and in my former paper expressly pointed out, that the position that Euc. I. 4 is really an inductive truth, and that the usual demonstration is not in itself convincing, is the only

* Logic, vol. ii. p. 217.

ground that remains to Mr Mill and his adherents. So far, then, I am confirmed by Professor Bain: it remains only to show that this new position is mathematically as untenable as that from which Mr Mill has been dislodged. If Professor Bain grants that the proof of Euc. I. 4 is not by syllogism from axioms—if, again, mathematically it is plain that there is none the less a real proof, not merely an induction—we shall have gone far to establish the validity of proof by intuition.

Professor Bain tells us that Euclid, while professedly going through a process of pure deduction, requires us to conceive an experimental proof. There is surely an ambiguity here. Does Mr Bain mean that Euclid merely calls to our mind former concrete experiments with triangles of card-board or paper, for these alone are actual and concrete to our author? Does Euclid's "experiment" agree with the descriptions of experiments in books of Physics, save only in this, that we have all made Euclid's experiment before? Clearly not. In picturing to myself an experimental proof in the usual sense, I imagine mentally, or with the help of a diagram, certain arrangements, and then I am told to imagine a certain result following—or rather, I am told to believe this result, for to picture it is quite superflous and often impossible. Euclid, on the other hand, tells me to superpose ideally the point A on C, the line AB on CD, and so forth, and then I do not require to be told that the coincidence of the whole triangles follows. I have no choice to imagine coincidence or non-coincidence. I see that it follows, and that quite apart from previous experiment.

Professor Bain allows the possibility of ideal experiments on mathematical forms.* I presume, therefore, that he will not deny that the intelligent reader of our proposition does, as he reads, make a valid experiment in favour of the proposition. But if this be so, where is the deception in Euclid's proof, and what is the necessity of supplementing that proof by further "ideal" or "actual experiments"? The course of Euclid's argument shows that the two triangles are not only equal, but equal in virtue of the way in which they have been constructed, viz., the equality of the two sides and the included angle. The fact that the proof is not syllogistic does not make it any the less a case of that parity of

* Logic, vol. i. p. 225.

reasoning which Professor Bain, in another connection, admits to be not induction but demonstration.*

Our author draws a broad line between the fourth proposition, with its "appeal to experiment or trial in the concrete," and the mass of geometrical proofs in which the figure is referred to for verification only, "the effect of every construction and every step of reasoning being judged of by actual inspection." But if the inspection follows the construction, what is the construction itself? A construction is not proved by syllogism from axioms. It is necessarily drawn, and in the drawing (mental or other) looked at. Every construction involves a figure and an intuition, which, while it looks at the individual figure, sees in it the general truth.† Mr Bain grants that of such consequences as that the diagonal of a parallelogram divides it into two triangles, Euclid offers no other proof than an appeal to the eye.‡ In fact, no other proof can be offered. Yet surely it will not be asserted that this too is an induction. In one word, if no proposition is fairly demonstrated where it is essential to look at the figure, there is no sound demonstration in synthetic geometry.

Finally, Professor Bain himself seems not quite satisfied as to the inductive nature of Euc. I. 4. "The proof," he says, "rests solely on definitions," and hence "the proposition cannot be real—the subject and predicate must be identical." Surely an identical proposition is not an induction! And surely, too, the proof rests not on definitions merely, but on definitions and the use of the figure! But I do not think that Professor Bain means to speak here in strict logical terms, for he straightway adds in explanation, "The proposition must, in fact, be a mere equivalent of the notions of line, angle, surface, equality—a fact apparent in the operation of understanding these notions. It is implicated in the experience requisite for mastering the indefinable elements of geometry, and should be rested purely on the basis of experience." We should have known better what this sentence means, if the author had adopted here the distinction between synthetic and analytic judgments. He cannot mean that a truth that is an in-

* Logic, vol. ii. p. 5.

† *Cf.* Kant, Krit. d. r. Vern. p. 478. Ed. Hartenstein, 1867.

‡ Logic, vol. ii. p. 218.

duction, and rests on experience, is an analytic judgment, that it can be reached by a purely formal dividing and compounding of the definitions of terms. Such a proposition could be shown to be true without any figure or any experiment. Yet the proposition is, we are told, involved in the notions; we cannot know what lines, angles, &c., are without knowing this too. If this means anything, it means that Euc. I. 4 is a synthetic judgment *a priori*; and that, after all, Kant and the mathematicians are right, and Mr Mill and the empirical logicians wrong.

4. A Simple Mode of Approximating to the Wave-Length of Light. By W. Leitch, Assistant to the Professor of Natural Philosophy in the University of Glasgow. Communicated by Professor Tait.

The fundamental phenomenon or fact of the science of optics is vision, that is, the perception we have of distant objects through the eye, or by the sense of sight. That vision is an effect transmitted to the mind by the object seen, is a necessary truth, involved in the definition of the term, and independent of all theoretical views beyond the consciousness of that perception.

Common observation informs us that vision cannot take place without that which we call light, and that light itself cannot exist without the presence of a self-luminous body. Every one has a distinct conception of the meaning of the terms light and luminous; their definition according to that conception would be a verbal exercise of no utility at present.

Next may be placed the fact, first revealed by astronomical observations, and afterwards verified by other experiments, that light is not transmitted instantaneously,—in other words, that some portion of time elapses between the occurrence of a visible phenomenon and our perception of it by the eye, such as, for simplicity, the passage of an electric spark, or the occultation of a star by the dark body of the moon or of a planet; and that the portion of time in question is in direct proportion to the distance of the object seen from the eye, the intervening medium being the same.

The progressive motion of light from the object seen to the eye being established, and the supposition that it is a substance emanat-

ing from the object with the velocity found, being seen to be inconsistent with the phenomena of interference, we can scarcely be said to make use of a hypothesis when we conclude that it is an action transmitted through a medium bodily at rest, it may be, but whose component molecules act upon one another in such a way as to propagate the effect in question. By the term light we mean this action considered as a physical fact, separate from our perception of it by the eye, and independent of its arrival or non-arrival at our organs of vision.

The propagation of light from a luminous point with the same velocity in all directions (in a homogeneous medium), implies that the action originating at any instant in the source is diffused over a spherical surface whose radius, measured from the luminous point as centre, constantly increases at the rate of the velocity of light; and the constancy with which this propagation is kept up, implies that there are an infinite number of such spherical surfaces, over each of which is diffused an action which originated in the source at a preceding instant. Next the question presents itself whether all these actions originating in the source at successive instants, and occupying successive spherical surfaces, are similar and equivalent. The phenomena of interference answer, that if we imagine a series of these spherical surfaces separated from each other by a very small constant distance λ, the action propagated upon each of these surfaces is the same, and that midway between each pair of the series is a surface propagating an action capable of destroying that of its neighbour of the first series, from which it is separated by the constant distance $\frac{\lambda}{2}$. Now, that is equivalent to saying that each thin spherical shell of the medium through which the action is transmitted, vibrates between opposite phases, and as it is impossible for us to conceive or believe that any finite change can take place in the material world that does not involve an infinite number of intermediate infinitesimal changes, we are authorised to say that light consists in periodic vibrations, propagated with very great velocity, and decomposable in an infinite number of ways into half vibrations exactly contrary to one another.

Thus far we have arrived without having recourse to any hypothesis, having assumed nothing regarding the nature of these

vibrations, the word vibration being understood in its most general sense as meaning oscillation between opposite phases or conditions, a fact revealed to us by the phenomena of interference. Even at this point, however, the hypothesis which forms the basis of the undulatory theory cannot fail to present itself to our minds, the hypothetical part being not so much the existence of a medium, or the propagation of vibrations, but the nature attributed to these vibrations, viz., that they consist in mere mechanical action, involving nothing but variations of pressure and displacement among the particles of which the medium is composed, and propagated according to the same laws as in ponderable media with which we are more familiar. The suspension of interfering vibrations is interpreted in the simplest manner as the result of the simultaneous application of equal and opposite forces, or according to a fiction easily understood, the superposition of equal and opposite motions, and their reappearance after separation as the natural consequence of the indestructibility of force. Moreover, our experience does not enable us to conceive any other kind of vibrations decomposable in the same manner, though the phenomenon of electrolysis seems to indicate the propagation of a periodic oscillation between opposite phases of decomposition and recomposition, involving something more than variations of pressure and displacement among the particles of water. Even the small degree of uncertainty that may remain at this stage of the inquiry, is diminished by the phenomenon of diffraction, and by the physiological analogy between the eye and the ear, both of them situated like feelers of the brain; we know the variety of perceptions that are communicated to the mind by the effect of mechanical vibrations upon one of these organs.

Adopting the hypothesis, we call these vibrations waves, from their analogy to the vibrations so designated in the case of water, and the distance λ above mentioned we call the length of a wave of light. In order to effect its measurement, we produce the phenomenon of interference; that is done most directly by deflecting two pencils of light proceeding from the same source in such a way that they may be superposed after traversing paths differing by $\frac{\lambda}{2}$,

or $\frac{3\lambda}{2}$, $\frac{5\lambda}{2}$, &c.; but the most instructive method is to produce the phenomenon of diffraction, which is usually accompanied by that of interference.

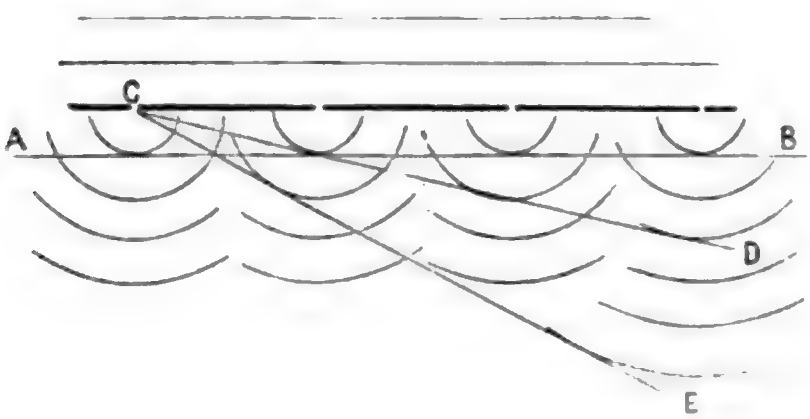
Diffraction is the name given to the lateral deviation of light in passing the edge of an obstacle, *i.e.*, of an opaque body. Having adopted the undulatory theory, we are ready to admit that such a deviation ought to take place, both from our experience of similar effects in air and water, and from our general ideas of the structure and equilibrium of fluids, from which we conclude that no single particle of a fluid can be disturbed without disturbing those surrounding it on all sides, that is, propagating a disturbance in all directions. When light, proceeding from a luminous source of very small apparent diameter, passes the edge of a dark body and is received upon a screen, instead of a sudden transition from light to darkness at the line where the geometrical shadow commences, we observe a gradually diminishing illumination for some distance inside of that line, and outside of it we observe maxima and minima of illumination arranged in bands parallel to it, if it is a straight line. In order to effect the measurement of the length λ, and understand the principle of the process, it is not necessary to follow the mathematical investigation of the position and intensity of these maxima and minima. That investigation is based upon the axiom that each point of a wave of light is a centre of force, the molecule there situated tending to propagate the energy with which it is animated in all directions around it, so that, if it were at any instant the only molecule agitated, it would immediately become the actual centre of a spherical wave. In the case of the uninterrupted propagation of a spherical wave, it is the envelope of all these elementary undulations to which is transmitted the vibratory movement of each molecule, and which, by reason of symmetry, is a spherical surface concentric with that which it succeeds. Diffraction takes place when part of the wave is intercepted by an obstacle, and the symmetry is destroyed which kept the surface of the wave concentric with its first position. The propagation of a spherical wave does not require that contiguous molecules be allowed free play. If we look at a luminous source through a fine grating, we see it in the same

position as if the grating were removed, which proves that a concentric spherical wave is formed by the union of the fragmentary parts of the incident wave which the grating has allowed to pass, or at least the fragmentary parts distributed over the spherical surface produce the same effect upon our sense of vision as if the surface were occupied by an unbroken wave. If the grating be sufficiently fine, and the luminous source not too near, we see not only the source in its proper position, but also images of it on both sides in the direction at right angles to the wires or dark lines of the grating. If the light of the source be homogeneous, that is, the same as we find at any point of a pure spectrum, these lateral images are counterparts of the true image, of various intensities. If the source emit white light, it is exhibited in each of these images separated into its component colours, the image being spread out so as to form a spectrum, with the violet extremity nearest to the central image.

In order to understand the origin of these lateral images, first suppose the transparent intervals to be of infinitely small width, and separated by dark spaces of finite and equal breadth. Suppose light coming from a distant source to be incident upon them in a direction perpendicular to their plane. The space occupied by the system of lines and spaces being very small, the surface of an incident wave may be considered as coinciding with their plane, so that a similar phase of vibration passes at all points of the transparent lines at the same instant. Each of these lines thus becomes the axis of a system of cylindrical waves behind the grating, and at any instant the same phase of vibration is found in each system at the same distance from the axis.

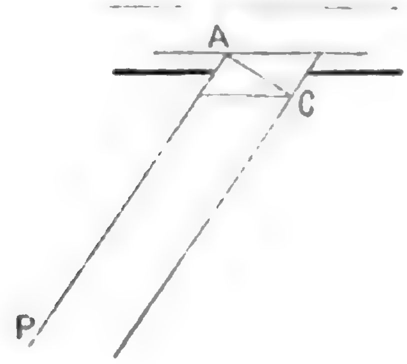
Suppose the dark lines in the figure to represent sections of these cylindrical surfaces in the same phase of vibration. Upon the surfaces which envelope a succession of these surfaces of similar phase, in a direction parallel to AB, are formed a system of waves by which we see the true image in its real position; similarly, by a system of waves which envelope surfaces of similar phase, in a direction parallel to CD, we see the first lateral image to the right; by a system of waves parallel to EC, we see the second image, and so on. If we denote by a the distance between the transparent lines, and by D, the angular deviation of the first lateral image,

we find, from the position of the surface CD, $a \sin D$ as the distance between successive surfaces of similar phase parallel to CD, that is to say, as the length of the wave of the light propagated in the direction normal to CD. Similarly, by drawing perpen-



diculars upon the successive envelope surfaces through C from the first opening to the right, we get for the same wave length $\frac{1}{2} a \sin D_2$ from the second image, $\frac{1}{3} a \sin D_3$ from the third, and so on. In the case of white light, the separation into its component colours exhibited in each lateral image enables us, by observing the deviation of each colour of the spectrum, to measure the wave length of light of that colour.

The lateral images are thus easily accounted for in the imaginary case, in which the transparent intervals are of infinitely small breadth. Gratings have been constructed by ruling sensibly dark lines upon glass so closely that the breadth of the transparent interval is only a small fraction of the length of wave. The explanation of the images seen through these is the same as that just given for the imaginary case. Suppose, however, the width of the spaces to be so much greater than the length of wave, that the small inclined surface AC which covers the opening, as seen in the direction AP normal to AC, stretches obliquely across the exact length of a wave of the incident light, the surface AC, which

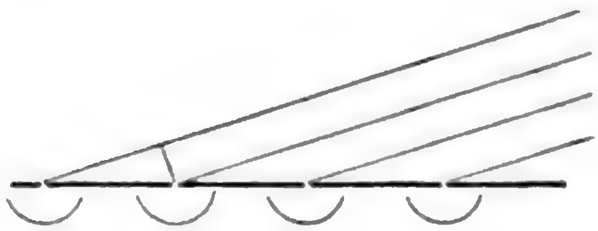


would be the locus of the same, or at least concordant phases of vibration if light were propagated in the direction AP,

will contain nothing but a series of equal and opposite phases, which will be discordant and mutually destructive, as far as concerns the propagation of light in the direction AP, and no image will be seen in that direction, whatever may be the distance between the transparent spaces. The same will be the case if the breadth of the spaces be such that the surface AC stretches across exactly 2, 3, or any whole number of wave lengths. But if the surface AC stretches across $n + \frac{p}{q}$ wave lengths, $\frac{p}{q}$ being a proper fraction, the vibratory movement transmitted along AP by the fractional part of the wave length will not be destroyed by the concurrence of its complete opposite, and light will be propagated along AP. The other transparent spaces will send concordant phases to the envelope wave, if AP be at the proper angle. In this case, however, the breadth ϵ of a transparent space must be added to a in the formula $a \sin D$, &c., $a + \epsilon$ being the distance between the successive effective remnants of the vibratory movements which pass to the envelope surfaces. The breadth $a + \epsilon$ occupied by a dark and a transparent space is called an element of the grating. If the fractional part $\frac{p}{q}$ of the wave length, which is effective in forming any one of these envelope waves, be either a very small or a very large fraction, its effect will be feeble, and the corresponding image of small intensity; but if it be exactly one-half of the wave length, its effect will be the greatest possible, and the envelope wave will receive from each opening the greatest possible amount of concordant action. In this manner is explained the difference of intensity of these lateral images, the one nearest to the central image not being always the brightest. Proximity to the central image is, however, also a cause of greater brightness, it being evident that the less the surface AC in the last figure is inclined to the incident waves, the greater is the absolute length of that part of it which stretches over any given fraction of the wave length, and the greater the amount of action of which it is the locus.

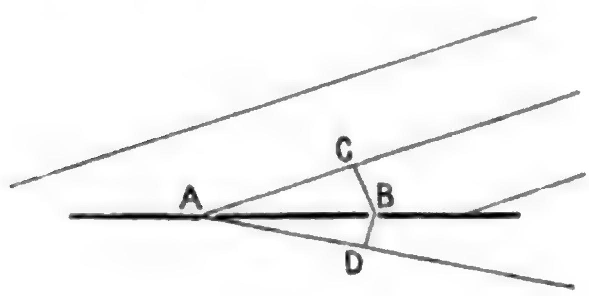
In the above the incident waves have been supposed to be exactly parallel to the plane of the grating, so that the same phase of vibration passes at the same instant through all the openings.

The figure annexed shows that if the incident waves be inclined to the grating at such an angle that the perpendicular from any opening upon the wave surface passing through the next opening is equal to the wave length, the same phase will in this case also pass all the openings at the same instant, though derived from different incident waves, and the first lateral image will be seen in a direction normal to the grating.



The same formula will give the wave length in this case, D being always the angular deviation from the true image or from the direction of the incident light. This is the condition approximately realised in the arrangements for measuring the wave length about to be described, but as no provision is made for an exact adjustment of the grating to the inclination just indicated, and as a very minute error in such an adjustment would cause the conditions of the experiment to be altogether different from those indicated by the figure above, it is necessary to account for the appearance of lateral images in the case of light incident at any angle, and find a formula for the wave length applicable to that case.

If, as in the figure below, the incident waves be so inclined to the grating that the perpendicular BC, together with the perpendicular BD, make up the wave length, the same phase of vibration will be situated at A and D; for the same reason, behind every two consecutive openings, like phases will be situated upon surfaces inclined at the same angle as AD, that is to say, AD produced will envelope like phases, and the first lateral image will be seen in the direction normal to AD.



If we denote by I the angle of incidence CAB, and as before by D, the angle of deviation CAD, we get $\lambda = (a+\epsilon)\{\sin \text{I} + \sin(\text{D}-\text{I})\}$. So long as I and D are small, the latter factor is approximately $= \text{I} + \text{D} - \text{I} = \text{D} = \sin \text{D}$, the same as before, so that in that case the error introduced by using the formula first obtained with only an approximate adjustment of

the grating is inconsiderable. The same is the case if either I alone or D − I be very small.

By differentiating the formula we get

$$\cos I + \cos (D - I)\left(\frac{dD}{dI} - 1\right) = 0 \quad . \qquad (2).$$

$$\therefore \frac{dD}{dI} = \frac{\cos (D - I) - \cos I}{\cos (D - I)} = 1 - \frac{\cos I}{\cos (D - I)},$$

$$(\text{and if } D = 2I) \qquad = 1 - \frac{\cos I}{\cos I} = 0,$$

that is, D is constant for small variations of the position of the grating, or angle of incidence, while the variation of the latter by condition (2) does not affect the value of λ calculated from the formula. There is, therefore, an advantage in observing with the grating adjusted to bisect the angle between the directions of incidence and diffraction, that being the position in which a small error in the adjustment has the least effect upon the result given by the formula, which becomes in this case,

$$\lambda = 2(a + \epsilon) \sin \frac{D}{2} .$$

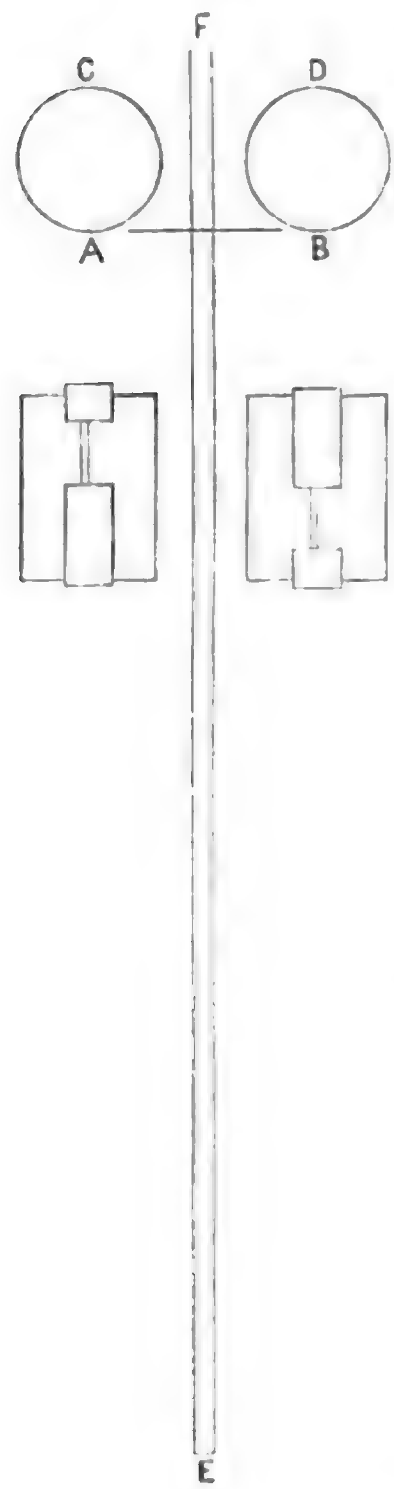
In the arrangements now to be described, in which we use two sources of light, one on each side of the normal to the grating, we make the angle $(D - I)$ approximately vanish, and use the mean of the two angles of incidence in the formula

$$\lambda = (a + \epsilon) \sin I.$$

By neglecting the part $(a + \epsilon) \sin (D - I)$, which is positive for the one light, and negative and of the same magnitude for the other, as is plain from the method of observing, we introduce no error into the result.

AC, BD, are sections of two rectangular pieces of tin bent into a cylindrical form round the glass funnels of two paraffin lamps. Their edges come short of meeting so as to leave a slit at A and B of about 1 millimetre in breadth. These slits are partially covered with tin as shown immediately below, where they are drawn as they appear to the eye of the observer. A thread is stretched round the two cylinders, partly shown between A and B. EF

is a straight stick passing horizontally immediately under the thread, and graduated in centimetres on its upper edge. A



grating constructed by drawing transparent lines at the rate of 2000 to the inch upon glass covered with a dark ground, is held by the hand against the end E of the stick, cut square with its edges. The stick is then pushed in or out in the direction of its length till the red colour of the first spectrum to the right of A is seen to be directly above the same colour of the first spectrum to the left of B. A pencil mark is then made upon the stick directly below the thread. The stick is then drawn further out until the yellow colours of the two spectra are seen in the same vertical line, and another mark is made; and so with the remaining colours. The distance from centre to centre of the two slits, in a horizontal line, being 10 centimetres, the distances marked off between E and the thread were read 99·5 centimetres for the red, 107 for the yellow, 116 for the green, and 135 for the blue. These numbers were taken for the distance to the light in each case, being only about $\frac{1}{50}$ per cent. less by calculation. The corresponding wave lengths by the formula $\lambda = (a + \epsilon) \sin I$, are

Red . $\frac{5}{99 \cdot 5} \times \frac{1}{2000} = \frac{1}{39800}$ of an inch = ·000638 millimetre.

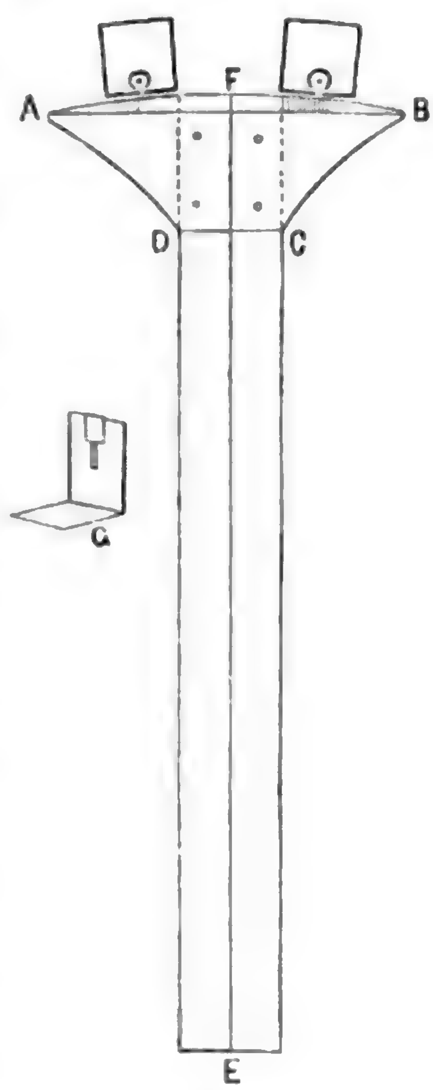
Yellow $\frac{5}{107} \times \frac{1}{2000} = \frac{1}{42800}$ „ = ·000593 „

Green $\frac{5}{116} \times \frac{1}{2000} = \frac{1}{46400}$ „ = ·000547 „

Blue . $\frac{5}{135} \times \frac{1}{2000} = \frac{1}{54000}$ „ = ·000470 „

Different measurements may be got by the same observer at different times from his uncertainty as to the points in the spectrum at which he should consider each colour to begin and end. This uncertainty is usually considered to be obviated by using solar light, and measuring the deviations of the dark lines in the spectrum; but as these lines are the parts of the spectrum from which no light comes, the process can scarcely be called the determination of the wave length of light.

Since the above measurements were made, an improvement was made in the apparatus by which the gratings were constructed, and finer gratings were made, which gave more brilliant spectra, by reason of the greater number of apertures from which similar phases of vibration came to the eye. With one of these, consisting of transparent spaces drawn at the rate of 3000 to the inch, a new set of measurements was taken in the following manner:—



EF represents a rectangular piece of wood upon which is pinned a piece of paste-board ABCD, whose edge AFB is an arc of radius 20 inches and centre at E. The chord AB is divided into tenths of an inch by perpendiculars to it meeting the arc. Touching the arc are placed, but not fixed, two pieces of tin bent as represented at G, each having a narrow slit so situated that the bottom of the one slit is on a level with the top of the other, and carrying a small piece of candle immediately behind the slit. The grating is held at E, and the pieces of tin are moved along the arc until the colour observed in each spectrum is in the same vertical line at F. The distance between the two slits is then read upon the graduated chord, and the half of that distance divided by 20 inches is the sine of the deviation. In this case the second spectrum from each light was observed, and the observed dis-

tances for the red, yellow, green, and blue, were 5·9, 5·5, 5·025, and 4·25 inches respectively. The wave lengths calculated from these data are in millimetres—

$$\frac{1}{2} \cdot \frac{5{\cdot}9}{2} \cdot \frac{1}{20} \cdot \frac{25{\cdot}4}{3000} = {\cdot}000624 \text{ millimetre for the red,}$$

$$\frac{1}{2} \cdot \frac{5{\cdot}5}{2} \cdot \frac{1}{20} \cdot \frac{25{\cdot}4}{3000} = {\cdot}000582 \quad ,, \quad ,, \quad \text{yellow,}$$

$$\frac{1}{2} \cdot \frac{5{\cdot}025}{2} \cdot \frac{1}{20} \cdot \frac{25{\cdot}4}{3000} = {\cdot}000531 \quad ,, \quad ,, \quad \text{green,}$$

$$\frac{1}{2} \cdot \frac{4{\cdot}25}{2} \cdot \frac{1}{20} \cdot \frac{25{\cdot}4}{3000} = {\cdot}000449 \quad ,, \quad ,, \quad \text{blue.}$$

The apparatus contrived and constructed by the author to produce these fine gratings has not been described, because its construction involves considerable trouble and expense, which the experimenter may avoid by applying to an instrument-maker who has a dividing machine. The difficulty of getting a sufficiently fine dark ground upon the glass will also be avoided if the dividing machine be fitted with a diamond point, which will scratch comparatively opaque lines on the transparent surface of the glass. The finest gratings constructed are produced in that way.

5. Note on Linear Partial Differential Equations. By Professor Tait.

The equation

$$P\frac{du}{dx} + Q\frac{du}{dy} + R\frac{du}{dz} = 0$$

may be put in the very simple form

$$S(\sigma\nabla)u = 0,$$

if we write

$$\sigma = iP + jQ + kR,$$

and

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz} \cdot$$

This gives, at once,

$$\nabla u = mV\theta\sigma ,$$

where m is a scalar and θ a vector (in whose tensor m might have

been included, but is kept separate for a special purpose.) Hence

$$du = -S(d\rho\nabla)u$$
$$= -mS.\theta\sigma\, d\rho$$
$$= -S.\theta d\tau,$$

if we put

$$d\tau = mV.\sigma\, d\rho$$

so that m is an integrating factor of $V.\sigma\, d\rho$. If a value of m can be found, it is obvious, from the form of the above equation, that θ must be a function of τ alone; and the integral is therefore

$$u = F(\tau) = \text{const.}$$

where F is an arbitrary scalar function.

Thus the differential equation of *Cylinders* is

$$S(\alpha\nabla)u = 0,$$

where α is a constant vector. Here $m = 1$, and

$$u = F(V\alpha\rho).$$

That of *Cones* referred to the vertex is

$$S(\rho\nabla)u = 0.$$

Here the expression to be made integrable is

$$V.\rho d\rho.$$

But Hamilton long ago showed that

$$\frac{dU\rho}{U\rho} = V\frac{d\rho}{\rho} = \frac{V.\rho d\rho}{(T\rho)^2},$$

which indicates the value of m, and gives

$$u = F(U\rho) = \text{const.}$$

It is obvious that the above is only one of a great number of different processes which may be applied to integrate the differential equation. It is quite easy, for instance, to pass from it to the assumption of a vector integrating factor instead of the scalar m, and to derive the usual criterion of integrability. There is no difficulty in modifying the process to suit the case when the right hand member is a multiple of u. In fact it seems to throw a very clear light upon the whole subject of the integration of partial differential equations. But I have not at present leisure to pursue the subject farther than to notice that if, instead of $S(\sigma\nabla)$,

we employ other operators as $S(\sigma\nabla)S(\tau\nabla)$, $S.\sigma\nabla\tau\nabla$, &c. (where ∇ may or may not operate on u alone), we can pass to linear partial differential equations of the second and higher orders. Similar theorems can be obtained from vector operations, as $V(\sigma\nabla)$.

6. On the Oxidation Products of Picoline. By James Dewar, F.R.S.E., Lecturer on Chemistry, Veterinary College, Edinburgh.

(*Abstract.*)

The author in this paper details the results of a series of experiments, commenced three years ago, on the oxidation of the pyridine series of bases. These bodies are readily attacked by permanganate of potash; and the oxidation products of picoline thus obtained are ammonia, carbonic, nitric, oxalic, acetic, and dicarbopyridenic acids, along with a very small quantity of some solid base, possibly a condensed base.

Dicarbopyridenic acid, $C_5H_3N\begin{matrix}CO_2H\\CO_2H\end{matrix}$, is bibasic, and bears the same relation to the nucleus pyridine that phthalic acid and its isomers bear to benzol. It crystallises from hot aqueous solutions in plates resembling naphthaline; the majority of its salts are soluble and crystallisable. The silver salt of the acid is very characteristic, being insoluble and gelatinous, not decomposed by boiling water, and not visibly affected by light. As this acid was got in only small quantity, the author had not the opportunity of producing its various derivatives.

The author observes that the two well-defined series of nitrile bases found in coal tar, viz., the pyridine and chinoline series, bear the same relation to each other that the benzol series of hydrocarbons does to the naphthaline. Thus, pyridine is supposed to be the nucleus in these bodies that benzol is in the aromatic series. The following are some of the analogies pointed out in the paper:—

Benzol.	Naphthaline.	Anthracine.	Pyridine.	Chinoline.
C_2H_2	C_6H_4	C_6H_4	C_2H_2	C_5H_3N
C_2H_2	C_2H_2	C_2H_2	NCH	C_2H_2
C_2H_2	C_2H_2	C_6H_4	C_2H_2	C_2H_2

Chinoline and pyridine, therefore, ought to be readily obtainable

from each other, and it is the intention of the author to work in this direction. It is observed also that indol, the nucleus of indigo, is benzol-pyrrol, being related thus,

Indol.	Pyrrol.
C_6H_4	C_2H_2
NH	NH
C_2H_2	C_2H_2

It is therefore likely that indol may be met with along with pyrrol among the products of the destructive distillation of nitrogenised organic substances. While this paper is passing through the press, Professor Baeyer of Berlin has pointed out, independently, a similar relation between pyrrol and indol, a note of which has just been published.

7. Notes of some Experiments on the Rate of Flow of Blood and some other Liquids through tubes of narrow diameter. By J. Matthews Duncan, M.D., F.R.S.E, and Arthur Gamgee, M.D., F.R.S.E.

The experiments, of which the results are recorded in the present communication, were undertaken in order to determine the rate at which blood flows through tubes of moderately small diameter, with a view to the study of the mechanical theory of dysmenorrhœa; they were afterwards extended to blood-clot, serum, milk, and urine, &c.

In a memoir inserted in the ninth volume of the "Mémoires des savants étrangers," M. Poiseuille stated the results of an investigation on the flow of water and other fluids through capillary tubes, showing how this is influenced by pressure, by the length and diameter of the tube, and by temperature. A committee of the French Academy, of which M. Regnault was the reporter, corroborated the results of M. Poiseuille's researches.* Subsequently this observer published a still more extended series of observations, including the determination of the rate of flow of serum and defibrinated blood.†

* Recherches expérimentales sur le mouvement des liquides dans les tubes de très-petits diamètres. Commissaires MM. Arago, Babinet, Piobert, Regnault rapporteur. Académie des Sciences, séance du 26th Décembre 1842.

† Recherches expérimentales sur le mouvement des liquides de nature différente dans les tubes de très petits diamètres par M. le Dr Poiseuille. Annales de Chimie et de Physique. Troisième série t. xxi. 1847.

The method employed by Poiseuille in his researches, and which is described at length in his Memoir, consisted essentially in causing air under a known pressure to force a known quantity of the fluid to be experimented upon through tubes of known diameter and length, and determining the time employed.

The following are the general results to which he arrived concerning the influence of the length and diameter of tubes of smaller diameter than a millimetre on the rate of flow of any liquid at a constant pressure and temperature:—

1*st*. The volumes of liquid flowing in equal times through capillary tubes of equal length, but of different diameters, are amongst themselves as the fourth powers of the diameters.

2*d*. The volumes of liquids which flow in equal times through capillary tubes of the same diameter, but of different lengths, vary inversely as the length of the tubes.

With regard to the influence of pressure, it was found that the rate of flow increased directly as the pressure; and with regard to the temperature, that, *as a general rule*, the rate of flow of solutions increases as the temperature rises.

With regard to the influence of various substances held in solution by a fluid, on the rate of flow, no general law was arrived at, connecting it either with chemical constitution, density, capillarity, or viscosity.*

The following are some of the results, extracted from M. Poiseuille's Memoir—

I. Tube employed (B) is 64 millimetres long; its diameter is $0^{mm}\cdot249$; capacity of receiver, 6 C. C.; pressure, 1 metre; temperature, 14°5 C.

	Time of Flow.
	s.
1. Distilled water, ...	535·2
2. Ether, ...	160·0
3. Alcohol, ...	1184·5
4. Serum of ox's blood, . . .	1029·0

* We may merely allude to the fact that M. Graham succeeded in showing a decided connection between the rate of flow of the different hydrates of sulphuric acid and their chemical constitution. His very interesting results are to be found in a paper "On liquid transpiration in relation to chemical composition." (*Philosophical Transactions*, 1861, p. 373).

M. Poiseuille made a single determination of the rate of flow of blood serum; of blood serum plus a small and unknown quantity of corpuscles, and of defibrinated blood, the same animal's blood (an ox's) having been used to furnish the three liquids. The following are the results—

Temperature and pressure stated to have been kept constant during all the experiments; length of tube, 110 millimetres; diameter, $0^{mm}\cdot256$; capacity of receiver, between 5 and 6 C. C.

	Time of Flow.
	m. s.
Serum, ...	20·33
Serum and a small and unknown quantity of blood corpuscles, ...	21·17
Defibrinated blood, ...	68·47

Poiseuille points out that the aggregation of blood-corpuscles, which always takes place in defibrinated blood, leads to a choking of the tubes employed, especially when these are of narrow diameter ($0^{mm}\cdot1$), or to an irregular flow, and that consequently defibrinated blood cannot readily be injected through the capillaries of the lungs of animals which have been bled to death. The recent experiments of Dr J. J. Müller,* carried on under the direction, and according to the method, of Professor Ludwig, in the Physiological Institute of Leipzig, are opposed to the statement of Poiseuille, for he succeeded in keeping up for long periods a flow of defibrinated blood through the lungs.

Method employed in the present research.

All experiments were conducted according to a method suggested by, and under the direction of, Professor Tait, in the Physical Laboratory of the University of Edinburgh. The liquids to be experimented upon were allowed to flow through tubes of known diameter and length, into a large air-pump receiver exhausted to a partial and known extent, the fluid being thus subjected to the pressure of the atmosphere, minus that of the air in the receiver.

Before enumerating our experiments, it may be well to point out certain fundamental differences which exist between them and

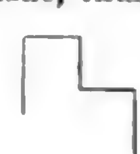
* "Ueber die Athmung in der Lunge von Dr J. J. Müller." Arbeiten aus der Physiolog. Anst. zu Leipzig Mitgetheilt durch C. Ludwig. Leipzig, 1870, p. 37–76.

those of M. Poiseuille. 1*st*, Our tubes had a much wider diameter—those used by the French observer varied in diameter from $0^{mm}\cdot 1949$–$0^{mm}\cdot 256$, whilst our tubes were from $0^{mm}\cdot 845$–$1^{mm}\cdot 259$. 2*dly*, By our tubes being much longer than those of Poiseuille; and, 3*dly*, By the liquids being allowed to flow, not into water, but into empty vessels placed in the partially exhausted receiver.

I.—Influence of the Shape of the Tubes employed on the Rate of Flow.

It was considered advisable to determine, in the first place, whether bends in the tubes through which the liquids were made to flow would exert any influence on the rate. Accordingly, a tube 1129 millemetres long was bent twice at right angles; one end was connected by means of a tightly fitting cork with the

TABLE I.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents. in Seconds.	
1–5	Water, . .	mm. 0·845	mm. 1129·8	13·0° C	mm. 708·59	126·4	Tube bent twice at right angles, thus, 
5–8	Common Sulphuric Acid,	,,	,,	13·5	,,	2978·0	
8–9	Water, . .	,,	,,	13·5	588·5	158·0	
10–11	Water, . .	0·845	1129·8	13·5	588·5	159·8	Tube bent four times at right angles in the same plane, thus, 
11–12	Water, . .	0·845	1129·8	11·5	588.5	157·4	Tube bent four times at right angles; at one point bent at an angle of about 135° to its former plane.
13–14	Water, . .	0·845	1129·8	11·4	588·5	161	Tube again bent as in experiments 10 and 11.
15–17	Water, . .	0·845	1129·8	33·0 C	588·5	108	

exhausted receiver, and the other was at a given instant immersed in water. The rate of flow having been determined, the tube was bent four times at right angles, and the experiment repeated; then it was not only bent four times at right angles *in one plane*, but bent at one point at an angle of about 135° to its former plane.

The results of these various experiments are exhibited in Table I., page 196.

It results from these experiments that the bends in the tubes had no perceptible influence in modifying the flow—the quantity of fluid flowing in the same time being directly as the pressure, and very much influenced by rises of temperature.

II.—*Rate of Flow of Defibrinated Blood of Sheep.*

Having determined that the shape of the tubes exerted no influence on the flow of fluids through them, we proceeded to examine the comparative rate of flow of the defibrinated blood of the sheep. The results are recorded in Table II.

The tube used in this experiment was 908·9 millimetres long, and was twice bent at right angles. The diameter was 1·214 millimetres.

TABLE II.

No. of Experi-ments.	Fluid used.	Diameter of Tube.	Length of Tube.	Tempera-ture.	Pressure.	Rate of Flow of 100 Cubic Cents in Seconds.
18–21	Water, . .	mm. 1·214	mm. 908·9	10°·5	mm. 583·5	67·6
22–25	Defibrinated sheep's blood, . .	,,	,,	16·7	583·5	227·6
26–28	,,	,,	,,	,,	,,	
29–31	,,	,,	,,	,,	,,	
32–35	,,	,,	,,	31·0	,,	143.4

TABLE III.

Comparative Rate of Flow of Water, Defibrinated Ox-Blood, Serum of Blood (obtained from same sample of Blood), and Defibrinated Sheep's Blood.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time occupied by Flow of 100 Cubic Cents in Seconds.
		mm.	mm.		mm.	
36	Water, . .	1·214	908·9	12°·0 C	598·7	68·16
37*	Serum of ox-blood,	,,	,,	13·1	,,	97·10
38	,,	,,	,,	,,	,,	98·14
38	,,	,,	,,	16°·0	,,	94·50
40	Defibrinated ox-blood, . .	,,	,,	,,	,,	365·7
41†	Defibrinated sheep's blood,	,,	,,	18°·0	,,	260·2

III.—*On the Rate of Flow of Pure* (i.e., *uncoagulated*) *Blood at the Temperature of Body through Narrow Tubes.*

Exp. 43.—In this experiment a calf, about a week old, was made use of. The jugular vein on the left side having been exposed, an opening was made into it as low in the neck as possible, and a flexible catheter was passed into the right side of the heart; the venous blood used was thus obtained.

Thereafter the carotid artery was exposed on the same side, and a ligature having been applied on the distal side of the exposed portion, a tube was introduced into the cardiac end. From this tube was obtained the arterial blood used in the experiment.

The temperature of the calf before the experiment was, 38°·8 C.
After the experiment, ... 38°·7 C.

The blood was received directly into graduated tubes heated to 38·°8 C.

* Solids in 1000 parts of serum, ... 90·41
Water, ... 909·59
† Solids in 1000 parts of the blood, ... 212·21
Water, ... 787·79

Two tubes were used in these experiments. The length was 56 inches. The first (Tube C) had a diameter of 1·259 of a millimetre. The second (Tube A) had a diameter of 0·9289 of a millimetre.

TABLE IV.

No. of Experiments.	Fluid used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents in Seconds.
		Tube C. mm.	mm.		mm.	
43	Water, . .	1·259	914·	15°· C	601·7	42·10
44	Water, . .	,,	,,	39°·5 C	,,	39·43
45, 46	Venous blood of calf,	,,	,,	38°·8	589·0	54·9
46, 47	Venous blood of calf, defibrinated and arterial, .	,,	,,	,,	,,	53·11
48, 49	Arterial blood of calf,	,,	,,	,,	,,	60·07
		Tube A.				
50	Water, . .	0.9289	914·	38°·5	601·7	69·4
15–53	Arterial blood of calf,	,,	,,	,,	,,	160·1

From this experiment it would appear that the rate of flow of blood just drawn from the vessels of a living animal is very much greater than the rate of flow of blood which, having been defibrinated, has been allowed to stand for some time, as was the case in experiment 40. In defibrinated blood the corpuscles tend undoubtedly to run together, and the masses thus formed by their coherence must necessarily account for the extreme slowness. The pure and perfectly warm blood flowed, indeed, more rapidly than did the serum obtained from ox-blood, which had been used in a previous experiment. In experiments 36, 37, 38, and 39, it was found that the time of flow of equal quantities of serum and water were represented by the ratio of 1·4 : 1. In experiments 43–49, it was found, on the other hand, that the rate of flow of equal quantities of pure blood and water were represented by the ratio of 1·3 : 1.

In a former part of this paper we stated that the diameters of the tubes used by us differed from those of Poiseuille in being much wider.

As was previously stated, the French author found that in capillary tubes of different diameter, the quantity of fluid flowing in equal times through equal lengths, varies not as the squares, but as the fourth power of the diameters. In the tubes used by us, in the experiment above described, the diameter was such that the quantities of water flowing through equal lengths were, *cæteris paribus*, as the squares of the diameters. It is interesting to observe in connection with experiments 43–53 inclusive, that whilst the amount of water flowing varied very much as the squares of the diameters, the quantity of blood flowing through the two tubes did not obey this law; the blood being retarded in its flow more than water though by no means to such an extent as to show that, for it, the tubes obeyed Poiseuille's law.

IV. *On the Pressure required to force Blood Clot through Tubes of Narrow Diameter.*

The clot used was obtained by allowing ox's blood to coagulate, and separating it from serum.

Exp. 54.—In this experiment a tube having a diameter of 1·162 millimetre was used. Although subjected to the whole atmospheric pressure (700 M.) none of the clot would pass through the tube.

Exp. 55 *and* 56.—In this experiment the same clot was used, but a different tube. The clot was found freely to flow through the tube, which had a diameter of 2·00 millimetres.

In experiment 55 the pressure of a column of mercury 24 inches high was employed. In experiment 56 that of a column 29 inches high was required.

V. *On the Rate of Flow of Milk and Urine through Narrow Tubes.*

The results of these experiments are shown in the annexed table. It will be observed that two tubes were employed in the determination of the rate of flow of milk, whilst the two sets of experiments with urine were performed with one tube. The rate of flow of urine is shown to be almost identical with that of water, whilst the rate of flow of milk is about the same as that of water when a large tube is used, but much slower when a tube of narrow diameter is employed.

TUBE A.

Fluid Used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Time of Flow of 100 Cubic Cents in Seconds.
Water, . .	mm. ·928	mm. 914	17° C	mm. 601·97	69·2
Urine, Sp. Gr. 1018	,,	,,	17·5	,,	71·3
Urine, Sp. Gr. 1007	,,	,,	,,	,,	70·3
Cow's Milk, .	,,	,,	24·6	594·3	90·3

TUBE C.

Fluid Used.	Diameter of Tube.	Length of Tube.	Temperature.	Pressure.	Rate of Flow of 100 Cubic Cents in Seconds.
Water, . .	mm. 1·259	mm. 914	15°	mm. 601·97	42·1
Cow's Milk, .	,,	,,	27	,,	38·1
Goat's Milk, .	,,	,,	22	,,	36·09

8. On Cystine ($C_3H_7NO_2S$). By James Dewar, F.R.S.E., Lecturer on Chemistry, Veterinary College, Edinburgh; and Arthur Gamgee, M.D., F.R.S.E., Lecturer on Physiology, at Surgeon's Hall, Edinburgh.

Preliminary Notice.

With the exception of the physical characters of this rare chemical substance, which is only known as an abnormal constituent of the human body, we know so very little, that even a few facts with regard to its behaviour with reagents may not be altogether uninteresting.

Cystine has the composition $C_3H_7NO_2S$; and crystallises in the form of six-sided plates. It forms with hydrochloric, nitric, and phosphoric acids, definite crystalline compounds.

Some of the most important facts with regard to the chemical reactions of cystine have been recorded by Dr Bence Jones, who for the first time showed that nitrous acid decomposes it with the evolution of nitrogen, and that in this operation the sulphur which it contained is oxidised to sulphuric acid, whilst a non-crystalline

substance is left which is precipitable by nitrate of silver, mercuric chloride, as well as by acetate of lead.

The cystine used in our experiments was obtained from two portions of calculi, one of which was furnished to us by Professor Maclagan, the other by the Royal College of Surgeons of Edinburgh. The cystine was obtained by treating the pounded calculi with strong liquor ammoniæ, which dissolved the greater part, and allowing the solution to evaporate at a very gentle heat. The cystine which separated was then again dissolved in ammonia and recrystallised.

Preparation of Hydrochlorate of Cystine.

One gramme of cystine was dissolved in boiling hydrochloric acid; on cooling beautiful needle-shaped crystals separated, which were very soluble in water. When thoroughly dried in vacuo over quicklime the crystals were found not to be readily soluble in water. 0·05 grm. of crystalline hydrochlorate of cystine yielded 0·0452 grm. of AgCl, corresponding to 22·2 per cent. of HCl (Calcd. 22·5).

Action of Nitrate of Silver on Cystine.

Cystine was dissolved in strong solution of ammonia, and to the solution was added a solution of silver nitrate in ammonia. No precipitate occurred, nor did the solution darken in the cold. When slightly acidified with nitric acid, a canary-yellow precipitate was thrown down, which was collected and dried *in vacuo*. The filtrate blackened when heated, and on filtering off the black precipitate a clear colourless solution was obtained, which was not further blackened when boiled with ammoniacal solution of oxide of silver.

On analysis the substance precipitated proved to be a compound of cystine with nitrate of silver.

In a subsequent experiment an ammoniacal solution of cystine was boiled with an ammoniacal solution of nitrate of silver. A black precipitate fell which consisted of sulphide of silver. The filtrate from the precipitate of sulphide of silver was subsequently treated with solution of chloride of ammonium to separate the excess of silver. The solution was found not to be precipitated by hydrochloric acid and chloride of barium, nor by sulphate of cal-

cium. It is therefore evident that when an ammoniacal solution of cystine is heated with ammoniacal solution of oxide of silver, the sulphur is separated entirely as sulphide of silver, none being oxidised; it is also obvious that no oxalic acid is formed.

Action of Caustic Soda and Caustic Baryta on Cystine.

Cystine, when treated with pure solution of pure NaHO, and evaporated in a silver basin, gives a reddish fluid; sulphide of sodium is then produced, blackening the basin, and ammonia is copiously evolved. On treating the residue with water, neither sulphuric nor oxalic acids can be detected. The liquid contains, however, a large quantity of sulphide of sodium with a mere trace of sulphite.

Cystine, when heated to 150° C. with solution of caustic baryta in sealed tubes, gave off ammonia, a large quantity of sulphide of barium, a smaller quantity of sulphite of barium, and a trace of hyposulphite being formed. No trace of sulphocyanide could be detected.

Action of Alcoholic Solution of Potash on Cystine.

Cystine was heated for several hours in a sealed tube at 130° C with an alcoholic solution of potash. At the conclusion of the experiment a small quantity of dark sticky matter was found adhering to the tube, which contained a yellowish fluid. The latter smelt strongly of ammonia, which was separated by distillation. The residue was acidified with dilute sulphuric acid, and shaken up with ether. Ether left a yellow non-crystalline substance, possessed of an indefinite but disagreeable odour. This substance had a strong acid reaction, and was found to contain no sulphur.

Action of Nascent Hydrogen on Cystine

When cystine is added to a mixture of tin or zinc and dilute hydrochloric acid, large quantities of sulphurated hydrogen are given off; the evolution of gas gradually slackens, till even after the action has gone on for several days, traces of sulphuretted hydrogen continue to be given off. When treated in the same manner taurine does not evolve H_2S.

It is to be noted that this evolution of H_2S, when cystine is

treated with tin or zinc and hydrochloric acid, might be used as a test for the substance, care being previously taken to separate any sulphide which might exist.

Action of Nitrous Acid on Cystine.

Cystine was placed in water and a stream of nitrous acid gas passed through it. No action took place until the water was heated; it then commenced and proceeded briskly, with abundant effervescence, until the whole of the substance was dissolved.

The clear solution contained a large quantity of sulphuric acid, but not a trace of oxalic acid. When boiled with an ammoniacal solution of nitrate of silver, considerable reduction took place, a beautiful mirror of silver being deposited on the glass. The fluid was again subjected to the action of nitrous acid; still no oxalic acid could be found, and the reduction of an ammoniacal solution of oxide of silver continued. A portion of the fluid was treated with carbonate of barium and heated; the clear filtrate had an alkaline reaction, and was abundantly precipitated by nitrate of silver and acetate of lead. The remainder of the fluid, after the treatment with $BaCO_3$, was treated with solution of nitrate of silver. An abundant canary-yellow precipitate was formed. This was suspended in water and decomposed with H_2S; the filtrate was evaporated to dryness, and presented the appearance of a sticky solid. It was soluble in water. The aqueous solution was evaporated and treated with absolute ether, which dissolved the greater part. The ethereal solution left on evaporation an acid fluid. This was dissolved in water, neutralised with ammonia, and precipitated with solution of nitrate of silver. The yellow precipitate obtained was amorphous; it was dried in *vacuo*. Two specimens of the silver salt prepared at different times were analysed by us. The following are the results of two analyses:—

Silver, . . .	56·9	57·5
Carbon, . . .	19·43	21·32
Hydrogen, . .	5·29	4·64

In considering the discrepancies of these analyses, it must be borne in mind that we were operating in excessively small quantities of a substance prepared at different times by complicated processes.

Remarks.

Cramer believed that cystine was intimately related to the body called Serin, $C_3H_7NO_3$, which is obtained as one of the products of the action of alkalies on silk. Serin, when treated with nitrous acid, yields glyceric acid, as alanine under the same circumstances yields lactic acid, and therefore serin may be looked upon as amido-glyceric acid.

Cramer further believed that cystine was a sulpho-amido-glyceric acid, *i.e.*, serin in which hydroxyl has been replaced by HS.

This supposed relation is exhibited below—

CH_2OH	CH_2NH_2	CH_2NH_2
$CHOH$	$CHOH$	$CHSH$
CO_2H	CO_2H	CO_2H
Glyceric Acid.	Amido-glyceric Acid or Serin.	Cystine.

Considering that this relation of cystine to serin really exists, some have argued that on treatment with nitrous acid, cystine should yield glyceric acid. We do not, however, admit that this would really be the case. If we examine the case of sulpho-lactic acid, an analogous body to the supposed sulphur derivative of serin, we find that, on oxidation, it gives sulpho-propionic acid, and therefore we should, in the case of cystine, expect that a sulpho-acid would be formed on treatment with nitrous acid, were it built up as Cramer supposed. We have uniformly observed, during the course of our experiments, that, however carefully we attempted to regulate the action of nitrous acid on cystine, or of a nitrite on a salt of cystine, the sulphur separated as sulphuric acid thus pointing to a material difference in its reactions from what we should have expected from its supposed constitution. Although we cannot consider our experiments as definitive, we can assert that glyceric acid is not a product of the action of nitrous acid, and we venture to predict that, in all probability, cystine will be found to be related to pyruvic acid—to be an amido-sulpho-pyruvic acid. We base this supposition on the near approach of the analyses of the silver salt of the acid obtained by the action of nitrous acid on cystine, to the composition of a pyruvate, and on the general character of the oily acid produced.

We intend to pursue this subject further.

9. Notes from the Physical Laboratory of the University By Professor Tait. (With a Plate.)

After passing through the usual routine work of acquiring skill in the fundamental operations, several of my students have remained long enough in the laboratory to make investigations in various branches of Physics. A great many of these inquiries related to matters already thoroughly known; but some have claims to notice as dealing with subjects on which our information is as yet incomplete. These I propose, from time to time, to lay before the Society. Their value as scientific results must depend on the skill and care of the experimenters. For the forms of apparatus employed, and the mode of conducting the experiments, I am, in most cases, responsible.

(1.) Mr J. P. Nichol has made a long series of experiments upon the Radiation and Convection of Heat, mainly to determine the amount of radiation in absolute measure, but incidentally with a view to finding how convection varies with the density of the air. The following is a preliminary notice of his work. The radiating body was a thin spherical shell of copper, filled with hot water. Its surface was sometimes bright, sometimes covered (by means of photographic varnish) with lamp-black. It was suspended by fine wires in a metallic vessel, which was blackened internally, fitted with a pressure-gauge, surrounded by cold water, and connected with an air-pump. An iron cup was let into the top of the shell, and contained a little mercury surrounding the bulb of a thermometer whose stem ascended in a glass tube which was inserted in the lid of the closed vessel. Considerable trouble was caused at first by the water leaking out of the shell when its temperature was high and the vacuum good—but in the later experiments this was entirely got over.

As it was suspected that a difference of thickness of the lamp-black coating might influence the amount of radiation, the mode of experimenting finally adopted was to alter the air pressure in the vessel from time to time; first, for instance, half an hour's cooling at 100^{mm}, then half an hour at 200^{mm}, then at 100^{mm}, and so on. But the portions of the curves of cooling thus found on separate days fitted well together into a single continuous line, as is seen in

the corner of the diagram, where the dotted lines belong to one day's experiments, and the double lines to those of another day.

The numbers (H) given in the following table, which is formed from means of many experiments, and which is shown graphically in the diagram, express in grains the quantity of water which would be heated 1° Centigrade by the heat lost (by radiation and convection jointly) by one square inch of surface in an hour, its temperature being kept constant. With the apparatus employed, it was not easy to keep the pressure lower than 10^{mm}; but the curves for different pressures show that in this case the convection must be small, so that (roughly) we may take the numbers given for that pressure as representing the radiation alone.

	Blackened.		Bright.	
Pressure.	Temperature C.	H.	Temperature C.	H.
760^{mm}	61·2	6258	63·8	3537
	50·2	4875	57·1	3091
	41·6	3867	50·5	2637
	34·4	3082	44·8	2251
	27·3	2294	40·5	2013
	20·5	1629	34·2	1571
			29·6	1353
			23·3	996
			18·6	751
102^{mm}	62·5	4650	67·8	1763
	57·5	4150	61·1	1552
	53·2	3760	55·	1371
	47·5	3220	49·7	1220
	43·	2835	44·9	1082
	28·5	1755	40·8	960
10^{mm}	62·5	4236	65·	1390
	57·5	3847	60·	1273
	54·2	3593	50·	1025
	41·7	2600	40·	786
	37·5	2292	30·	563
	34·	2040	23·5	445
	27·5	1600		
	24·2	1400		

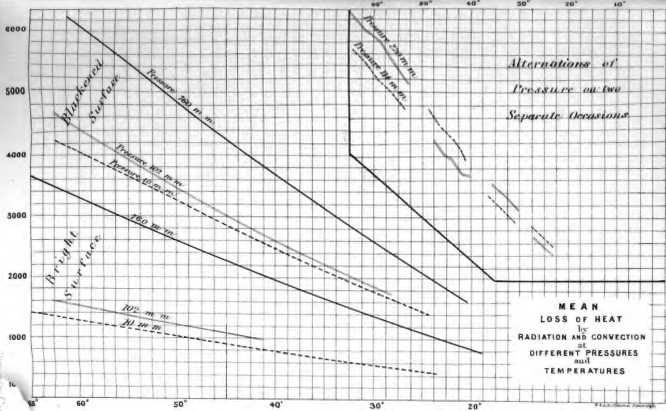
(2.) Mr A. Brebner made during last winter a number of careful determinations of the polarisation of electrodes of various materials in commercial sulphuric acid of various strengths and at various temperatures. The process employed was essentially the same as that described by me in the Proceedings R.S.E. for May 31, 1869. The following are means of many experiments:—

Platinum Electrodes.

Acid to Water.	Temperature C.	Polarisation.	Acid to Water.	Temperature C.	Polarisation.
	8°	224		5°	188
1 : 50,	68°	187	1 : 10,	56°	174
	87°	172		90°	157
	6°	178		7°	202
1 : 40,	54°	169	1 : 0,	66°	169
	84°	157		134°	133
	10°	174		11°	244
1 : 20,	44°	167	1 : 0,	102°	187
	88°	158		201°	137

The results of such experiments cannot be expected to be very accordant, but, if the means above given may be trusted, the polarisation is less for 1 acid to 20 water than for either stronger or weaker acids; and it also falls off more slowly with increase of temperature.

(3.) Messrs P. W. Meik and J. Murray made many observations with an electric balance, and resistance coils, to test the change of electric resistance produced in a wire by extension. The wires tested were of two specimens of copper—one of high, the other of very low, conducting power. They were taken of equal gauge and of such lengths as to have almost equal resistance; one was associated with a 10 B.A. Unit coil as one side of the balance, the other had associated with it a box of resistance coils initially set at 10 B.A.U. The value of the galvanometric scale was determined in each experiment by increasing by a small known amount the resistance of the coils in circuit. The results are not yet quite reduced; as we require to know the linear extension, and (if possible), the cubical contraction, of each wire produced by the appended weights. But, even in their present state, they appear to be of some consequence, as they show changes of conducting power almost exactly proportional to the weights appended, but singularly differing in absolute amount for these dissimilar specimens of copper.



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OF THE

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VOL. VII. | 1870–71. | No. 82.

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Monday, 5th December 1870.

David Milne Home, Esq., Vice-President, read the following Address:—

Gentlemen, Fellows of the Royal Society of Edinburgh,—In compliance with a special request of the Council, I come before you this evening to deliver the Address usually given at the opening of our Winter Session.

This practice of annually taking stock to ascertain what business we are doing, and how we are doing it, seems to me very right and expedient. The whole Society is thus made aware whether it is retrograding or advancing,—whether it is or is not, carrying out the objects of its institution.

I see that in some former Addresses, not only was the existing state of the Society reported on, but occasion was taken to open up general views on science and literature, and sometimes to point out important discoveries recently made in particular fields of knowledge. An Address of that instructive character probably would have been given to-night, had the place I now unworthily hold, been occupied by the distinguished *savant* who stood above me on the roll of Vice-Presidents, as he also stands immeasurably above me in knowledge. That gentleman's numerous engagements elsewhere, and the expectation that he would be in Italy, prevented his guaranteeing to the Council when they applied to him, that he would be here to-night. My own usual avocations are not such as fit me for executing the duty which Dr Lyon Playfair would have so ably performed,—my time being chiefly occupied with the duties incumbent on a landed proprietor resident in the country, who has to attend justice of peace courts, road meetings, cattle plague committees, and parochial boards. My address, therefore, will not be literary or scientific, but of a practical character as more congenial to my habits of life;—containing nevertheless some information and suggestions which I hope may not be entirely useless.

What I shall venture to submit, will be under the following heads:—

1st. The work done by us as a Society, during the past year.
2d. The means we possess, of doing our work.
3d. Suggestions for rendering our Society more efficient.
4th. The usefulness of Societies like ours.
5th. The best way of encouraging and assisting such Societies.

I. *Work of the Society during the past Year.*

The ordinary business of the Society, as we all know, is done during the winter, at evening meetings, when papers are read. These are abstracted into our printed Proceedings, and the most valuable inserted *verbatim* in our Transactions.

The number of our meetings last winter was 13, being on an average two, each month. The number of papers read at these meetings, was 43. The authors of these, were 33 persons.

Of the 43 papers, 5 were literary; the other 38 papers, on matters of physical science.

In the previous year, the total number of papers had been 44, *all* on physical subjects.

The following epitome shows the number of the papers under each branch of science:—

Mathematical papers, ...	11
Chemical „	7
Mechanical or Natural Philosophy papers, .	6
Medical . . . . . „ .	4
Geological . . . . . „ .	3
Zoological . . . . . „ .	3
Geographical . . . . . „	2
Botanical . . . . . „ .	1
Meteorological . . . . „ .	1

In a few instances, and I regret they were so few, discussion occurred on the part of the Fellows present, after the papers were read or described.

I have said that all these papers appear in an abstracted form in our printed Proceedings. Last year's printed Proceedings extend to 209 octavo pages. Those of the year before, contained 200 pages.

Of the 43 papers presented last winter, 11 were selected a

of publication in our Transactions. The titles and authors of these papers were as follows:—

1. Reciprocal Figures, Frames, and Diagrams of Forces. By J. Clerk Maxwell, F.R.S.
2. Scientific Method in the Interpretation of Popular Myths, with Special Reference to Greek Mythology. By Professor Blackie.
3. Extension of Brouncker's Method to the Comparison of Several Magnitudes. By Edward Sang, Esq.
4. Green's and other Allied Theorems. By Professor Tait.
5. Heat developed in the Combination of Acids and Bases. By Dr Thos. Andrews, Hon. F.R.S.E.
6. The Genetic Succession of Zooids in the Hydroida. By Prof. Allman.
7. Influence of the Vagus upon the Vascular System. By Prof. Rutherford, of King's College, London.
8. Old River Terraces of the Earn and Teith, viewed in connection with certain proofs of the Antiquity of Man. By Rev. Thos. Brown.
9. Spectra formed by the Passage of Polarised Light through Double Refracting Crystals. By Francis Deas, LL.B.
10. Oxidation Products of Picoline. By James Dewar, Esq.
11. Account of the Great Finner Whale stranded at Longniddry. By Professor Turner.

I may here add that our volumes of Transactions are rapidly exhibiting an increase in the number,—I hope also in the value of their contents. About ten or twelve years ago, one year's Transactions did not exceed 100 quarto pages. During the three years which followed, their average size was measured by 250 pages; during the last three years by 310 pages.

The Society is aware that we have three prizes in our gift, created by members of our body at different periods,—the Neill prize, the Keith prize, and the Brisbane prize. A period of two years elapses, in the case of the two latter, before bestowal. Last year the Keith prize was awarded, consisting of a gold medal and £50, "for the best communication on a scientific subject." It was awarded to Professor Tait, for a paper, published in our Transactions, on the "Rotation of a Rigid Body about a fixed point."

In alluding to the award of this prize, it is only right to mention the high estimation in which, as I have reason to know, this paper and other mathematical papers by the same author are held by men of science. These papers are examples of the application and use of a new and wonderful instrument of analysis invented by the late Sir William Hamilton of Dublin, one of the profoundest philosophers of his day, known by the name of "*Quaternions*." I am told that there are as yet few mathematicians who can work with it. But Professor Tait has been able, both to work with it, and to improve upon it; and has applied it to the solution of many important physical questions not easily solved by ordinary analysis.

To show that these remarks rest on better testimony than my own, I beg to refer to the high appreciation of Professor Tait's application of "Quaternions," as expressed by the distinguished inventor himself, in a work published shortly after his death. Sir William Hamilton's "Elements of Quaternions" (page 755) contains the following passage:—

> "Professor Tait, who has already published tracts on other applications of Quaternions, mathematical and physical, including some on Electro-dynamics, appears to the writer eminently fitted to carry on, happily and usefully, this new branch of mathematical science, and likely to become in it, if the expression may be allowed, one of the chief successors to its inventor."

To these gracious words of Hamilton, may be added the testimony of Professor Sir William Thomson of Glasgow, himself a mathematician and physicist second to none in Europe, contained in a letter to our General Secretary, from which I am allowed to quote:—

> "My Dear Balfour,—The marked appreciation by Sir William Hamilton of Tait's work in quaternions, is about the highest possible testimonial to its excellence. His book on the subject will constitute, I believe, a permanent monument of the most marvellously ingenious generalisation ever made in mathematical science. It has already done much to render the new instrument available for researches in Natural Philosophy, and I can see signs (witness the two most transcendent and practical naturalists of the age, Helmholtz and Clerk Maxwell) of quaternions becoming, through its teaching, a useful implement, though many years may pass before fruits resulting from quaternionic husbandry can be gathered."

Besides the ordinary business of the Society for the past year

to which I have been adverting, there have been one or two other matters taken up by the Council which it is proper to mention—

(1.) The Council agreed to co-operate with other public bodies in this town, in giving to the British Association for the Advancement of Science, an invitation to hold their next year's meeting in Edinburgh. That invitation was communicated through our general secretary, Professor Balfour, at the Liverpool meeting. We all know the result; but perhaps all do not know how much is due to the efforts of this Society. It must also be matter of congratulation to ourselves to learn, that the President elect of the Association is one of our own members—a member of whom any Society may feel proud—Sir William Thomson of Glasgow; and, moreover, that the local secretaries and treasurer of the meeting are all Fellows of our Society. May I therefore be allowed to express a hope, that the members of this Society will do their utmost to assist in promoting the success of the meeting, and that the Society will be able to give a handsome subscription to the fund for expenses.

(2.) Another matter out of the ordinary business of the Society, is the application which the Council made to Her Majesty's Government, for the establishment of a Chair of Geology in the University of Edinburgh, and for assistance to endow it.

The circumstance which led to this application was the resignation of Professor Allman, and an intimation received about the same time, from that eminent geologist and true-hearted Scotchman, Sir Roderick Impey Murchison, that he was willing to set apart £6000 from his own funds, to yield a moiety of the endowment.

The Council of the Society, feeling that they would go with greater hope of success to Government if backed by other public bodies, obtained the co-operation of the University, the Royal Scottish Society of Arts, the Geological Society, and the Highland and Agricultural Society.

We all know, in consequence of an intimation in the newspapers, that the Premier has so far yielded to these applications, by agreeing that Government should pay £200 yearly to this object; so that, adding the dividends which will be obtained from Sir Roderick Murchison's more generous gift of £6000, there will be

for the support of the chair, a fixed income of £450. I believe there is in existence a separate yearly sum of £35, hitherto drawn by the Professor of Natural History, and which, in the event of a separate Professorship being established for geology and mineralogy, was appointed to be transferred to the latter. This bequest was made a number of years ago by a Scottish gentleman named Thomson, who died at Palermo.

Before taking leave of this subject, I wish to draw attention to the fact that in the other Universities of Scotland the same inconvenience exists, which is about to be remedied in Edinburgh; and perhaps I may be permitted to express from this chair a hope, that in them also, means may be found for removing that inconvenience. I was glad to observe, that the Lord Rector of Aberdeen University, in an address delivered by him about ten days ago, took notice of the multifarious branches of instruction which the Professor of Natural History has there to teach, and is unable to overtake. Mr Grant Duff is a member of the present Government, so that I trust he will call the Premier's attention to the subject. The chair of Natural History at Aberdeen was established by the Crown, and its occupant is appointed by the Crown. I presume the design and intention of the Crown was, that geology, and the other recognised branches of Natural History, should be taught in that University. Therefore if, in consequence of the extension and growth of these branches, it has become impossible for any one man to give instruction in all, there seems to be a sort of moral obligation on the Crown to carry out its own intention and undertaking, by appointing separate Professors for these branches.

These remarks apply equally to the two other Universities of Glasgow and St Andrews; the latter, however, viz., St Andrews, presenting an additional evil of its own, viz., the anomaly, that the Professor of *Natural* History has to lecture on *Civil* History besides.

It humbly appears to me that there should be no great difficulty, both at St Andrews and at Glasgow, of providing means for remedying the evils to which I have been adverting. The Government gives aid to schools to an equal extent with funds supplied locally for their support, even when these schools are of an elementary character, and supply instruction only for a parish. Much more must Government be disposed to assist when

the institution wanting help, draws scholars from a wide area of country, as is the case with a University. What persons are so interested in establishing means of instruction in geology and mining, as proprietors of coal, iron, shale, fire-clay, and building stones? or who more able than they, to provide the amount of funds necessary to warrant an application to Government to assist in endowing professorships for giving that instruction. The counties of Fife and Forfar, near St Andrews;—the counties of Lanark, Renfrew, and Ayr, so intimately connected with Glasgow, are all rich in mines and minerals. Surely the proprietors and manufacturers of both districts will have patriotism enough to raise, by a conjoint effort, the sum which one single individual—their own countryman—though not resident among us, has so cheerfully given.

I have adverted to this subject so fully, because of the interest which our Society, from a very early period, has taken in this particular science. Indeed, it is to geology that our Society is chiefly indebted for the reputation it first acquired in the scientific world, in consequence of the animated and stirring speculations and discussions instituted by its members, among whom were Sir James Hall, Lord Webb Seymour, Col. Imrie, Hutton, Playfair, and Jameson. I believe that little or nothing was known of geology, in Great Britain, before the time to which I have alluded; and that even the Geological Society of London, founded in the year 1808, owed its origin chiefly to Scotsmen resident in England, who had imbibed their taste for the science by taking part in the discussions, or studying the transactions of our Society. When, from various causes, the science of geology at a later period begun to flag in Scotland, our Society lamented and remonstrated, and endeavoured to waken public sympathy on the subject. Thus the late Principal Forbes, in his address from this chair in the year 1862, says:—

"Of all the changes which have befallen Scottish science during the last half century, that which I most deeply deplore, is the progressive decay of our once illustrious geological school."

In the year 1865, our Society presented a memorial to the Government of which Earl Russell was then head, pointing out the inconvenience of there being no separate Professorship of Geology, and asking Government to institute one.

Though our attempt to obtain redress was not then successful, it may be presumed that good was done, by our having kept it before the eye of the public; and that seeds then were sown, which have now produced the results we had so long been desiring.

II. I come now to the next division of this address, which refers to

The means we possess of carrying out the objects of the Society.

I allude to strength of membership, and to available funds.

With regard to funds, I am happy to say that, though not rich, we have now rather more funds, than we have ever had before; thanks to our excellent treasurer, Mr Smith, who does what he can to keep up income, and keep down unnecessary expenditure.

Our income is derived from three main sources:—

(1.) Contributions of ordinary Fellows, about .	£800
(2.) Dividends from capital invested, . .	280
(3.) Annual grant from Government, . .	300
Making a total revenue of	£1380

Our expenditure may be classed under the following five heads:—

(1.) Cost of printing and circulating Proceedings and Transactions, about . . .	£400
(2.) Rent of apartments, taxes, cleaning, &c., .	300
(3.) Books, periodicals, and newspapers, . .	150
(4.) Salaries of officers, ...	240
(5.) Expenses of evening meetings, . .	30
	£1120

With regard to membership—the number of ordinary Fellows—on whom of course we chiefly depend for papers, and for attendance at our evening meetings, stands thus. This time last year, the total number was 303. Since then, 30 new ordinary members have been elected—making altogether 333. But from this number must be deducted five who have died, and two

who have resigned—leaving a balance at this date of 326; which is a larger number of ordinary Fellows than we have had since the institution of the Society. The number of our honorary members is the same as formerly, 36 foreigners, and 20 British—all men of known celebrity.

Before referring more particularly to the individual members who, during the past year, have been taken from us by death, allow me to say that I think the giving of obituary sketches of deceased associates is a practice highly becoming. It should be remembered that our Society is intended, not only to aid science and literature, but also to promote good fellowship among the votaries of both. One object of our association, is to encourage and assist one another by sympathy, and interchange of views; for which purpose we not only listen to papers, and discuss these at our evening meetings, but also hold personal intercourse in our library and reading-room. When, therefore, any of our comrades are removed from our midst by death, it is but fitting we should offer a parting tribute of regret at the dissolution of our connection, and endeavour to fix some traces of our departed associates in our memory, by recounting the part they have taken in helping to carry on the business of the Society, by recording any services rendered to the country, and by noting the leading events of their lives.

Whilst we have reason to be thankful that, during the past year, the number of deceased associates is small—smaller, when regard is had to the total number of members, than in any former year, that circumstance is more than counterbalanced by the worth and preciousness of the lives whose loss we deplore.

The following are the names of deceased Fellows, of each of whom I proceed to give a short obituary notice:—

Adam Hunter.
Edward Francis Maitland.
Robert Nasmyth.
James Young Simpson.
James Syme.

Adam Hunter was born at Greenock on 20th June, 1791. He obtained his classical and mathematical education at Glasgow

University, and afterwards came to Edinburgh for the medical classes. He graduated in the year 1813. He died in Edinburgh, 24th June, 1870.

In the year 1815 he commenced practice in Edinburgh as a family physician, and continued there in the same vocation all his life. He was most attentive to his duties, very gentleman-like in his bearing, and an agreeable, social companion. He possessed the regard and esteem of the late Dr Abercrombie, whose family he attended when any of its members were ailing. He was with Dr Abercrombie himself, during his last illness; and, after his death, he wrote a short biographical memoir of his friend and patient for the newspapers.

In the year 1839 Dr Hunter became a Fellow of this Society. He was a member of the Medico-Chirurgical Society of Edinburgh, and contributed a paper to its Transactions, on "Dislocations of the Shoulder and Hip-Joints." He was a life member of the British Association. In the year 1865, he published an interesting pamphlet of forty-one pages on the subject of Life Insurance; contrasting the London and Edinburgh offices, and showing the superiority of the latter, as regards honest administration and principles. He had been a policy holder in a London office, as well as in the Scottish Widows' Fund, and found how much more advantageous it was to be connected with the latter, than with the former.

Dr Hunter was employed by the Directors of the Scottish National Insurance Company to make a special report on the lives of the assured in that Company. His report, which was printed, received much commendation. He had been the medical adviser of that Company since the year 1843; as also of the English and Scottish Law Life Assurance Association, since the year 1847. On the occasion of his death, the Directors of both Companies passed minutes, expressing the very high regard which they entertained for him. Whilst his health remained, Dr Hunter's practice was extensive; and his patients had not only full confidence in his professional skill, but derived great comfort from his visits. One of them writes thus: "On more than one occasion he was the means, in the hand of God, of saving my life, and many, many times he has lightened my anxieties,

and cheered my heart, in a way no one but himself could do. God was good to me, in giving me such a valuable adviser."

In the year 1865, Dr Hunter underwent an operation for removal of a tumour in the throat. But the disease was not eradicated. The tumour re-appeared, and continued up to the period of his death, which took place suddenly.

Dean Ramsay, to whose congregation Dr Hunter belonged, after his funeral, alluded from the pulpit to him, in these terms: "He had for many years a very extensive medical practice in the families of this city, and no man more conscientiously, more carefully, and more sedulously performed the duties of his profession. From the presence of an impending and fatal malady, death had for some time been familiarised to his mind. But I know how he met that monition, as he met all the trials of life, with a firm trust in the love of his Redeemer, and with unshaken faith in the fulness of His atonement."

Dr Hunter, in October 1820, married Elizabeth, the eldest daughter of John Kircaldy, Esq., and by her had six children.

Edward Francis Maitland, known after his elevation to the judicial bench under the title of Lord Barcaple, was born in Edinburgh, 10th April, 1808, and died there 23d February 1870. He was the youngest son of Adam Maitland of Dundrennan, in the county of Kirkcudbright—a property which a Dr Cairns of London left to his niece, whom Mr Maitland married. Edward Maitland's elder brother was Thomas, who also was raised to the bench, under the title of Lord Dundrennan.

He received his education at the High School, and at the University of Edinburgh, and came to the bar in the year 1831. He was possessed of considerable ability, and also of much general knowledge derived from reading. He was shy and reserved, and had an awkward manner, so that his real merits were less known than they deserved to be. For many years he had little or no business as a lawyer, and at one time in consequence meditated a change of profession. During this period of involuntary professional idleness, he became editor of the "North British Review," and contributed to it several papers, which were characterised by vigour of thought, and correctness of composition. Being a

Whig in politics, when his friends obtained office, he received the appointment of Advocate-Depute. In the year 1851 he was made Sheriff of Argyle. In the year 1855 he was appointed Solicitor-General, which office he lost with the change of Government; but in 1859 it was restored to him. These professional appointments afforded an opportunity of showing his qualifications as a good lawyer, and an accomplished pleader; and business at length flowed in, so as to afford a handsome income. He was thoroughly conscientious in the fulfilment of his professional engagements. When Solicitor-General, it was remarked that he never missed being present in the Justiciary Court, and he was always well prepared with the business of which he had charge. There were several cases of public interest in which he was counsel,—one of them the famous Yelverton case. He was senior counsel for Miss Longworth, and evinced the utmost anxiety to have her claims properly presented. Shortly before her case came on for discussion in the Inner House, he received from the Crown his commission to the bench. But he withheld it for a week, that he might have it in his power to plead once more on Miss Longworth's behalf; and it has been stated, that it took him three days' hard work to prepare for the pleading. He declined to accept of any remuneration for his services in this case. His title of Barcaple was derived from a property of that name which he had purchased from his brother, David, a merchant in New York. It is situated in Kirkcudbrightshire, and I believe not far from the family estate of Dundrennan.

It was in 1862 that Mr Maitland was raised to the bench, and it was in the same year that he became a Fellow of our Society. But he did not contribute any papers, or often attend our meetings. He was the first representative of the Edinburgh University Council in the University Court. He was also the first Rector of Aberdeen University, after the union of King's and Marischal Colleges in 1860. Not being able to understand how Mr Maitland should have been thought of for this appointment, being in no way connected with Aberdeen, I wrote to my friend Principal Campbell for an explanation; and I have much pleasure in making the following extract from his answer:—

"His appointment to the office of Rector was the result of a

severe and bitter contest between the friends and the opponents of the union of the Colleges, or rather a portion of the latter, for the more sensible and disinterested opponents had by that time seen the necessity of acquiescing in the union, and of either facilitating or not impeding the working of the University under the new arrangements. The malcontents, whose object was to bring about a dead-lock and embarrass the Universities' Commissioners, induced a party of the students to set up the late Sir Andrew Leith Hay, who certainly would never have been thought of in other circumstances. The friends of peace and order chose Mr Maitland, *although*—I perhaps ought to say, *because*—he was totally unconnected with this locality and district, and yet well-known as a man combining a cultivated mind with the aptitude for academic business, as well as the firmness which our circumstances required.

"The votes of the *Nations* stood two to two, and the casting vote having fallen to me—the Chancellorship being vacant—I gave it in favour of Mr Maitland, although, owing to local influence and intimidation, the aggregate majority of individual votes was in favour of his opponent. I need not now say anything of the abuse and threats with which my decision was received by many in the town, of the childish and abortive application to the Court of Session for an interdict, or of the violence with which some of Sir A. Leith Hay's supporters attempted to interrupt the installation, and the Rector's address. All was amply repaid, to me, at least, by Lord Barcaple's great services to the University, in circumstances of difficulty which the authorities of a Scotch University have rarely, if ever, encountered—services which eventually gained for Lord Barcaple the esteem of most of his opponents, and the lasting gratitude of the friends of the University. He made the duties of his office a matter of conscience. Notwithstanding the demands on his time, of such a practice at the bar as his, he never hesitated to come to Aberdeen when required; and I can safely say that no Rector in Scotland, during his three years' tenure of office, has ever attended an equal number of meetings of Court and Council. His inaugural address was in a high degree sensible, elegant, and scholarly, but I do not remember that it was remarkable for anything in the topics or mode of discussion.

"Lord Barcaple was a Whig and a Free Churchman. I am neither. But there are few men whose memory I cherish with greater veneration."

Lord Barcaple's inaugural address referred to by Principal Campbell, I have, since receiving the Principal's letter, had an opportunity of reading. It contains an admirable summary of the duties of University students, and also of the temptations to which young men of their age are exposed. The language employed is correct and forcible—clearly indicating that Lord Barcaple was a person of high intellectual powers, and of cultivated mind.

Lord Barcaple, though of decided political views, was too conscientious to be a party man. His friends had looked forward to his holding the office of Lord Advocate, and going into Parliament. It was probably lucky for him that he did not undergo this ordeal, as the exercise of patronage in a party spirit would have been to him a perpetual misery. It is understood that, soon after he became judge, he regretted his elevation, as it not only greatly lessened his emoluments, but imposed on him more onerous duties than he was able comfortably to discharge. The death of Lord Manor, and the unaccountable delay on the part of Government in filling up the vacancy, threw on Lord Barcaple a very large amount of judicial work. The load proved too much, and he broke down; continuing, however, to the very last the performance of duty. If, in consequence of his reserved habits, Lord Barcaple had not many friends, he had no enemies. His amiable dispositions, and strictly truthful character, ensured to him a peaceful life, and the esteem of all who knew him.

Robert Nasmyth was born in Edinburgh in the year 1792. He died there, 12th May, 1870. He was educated first at the High School, and when about fifteen years old went to the University of Edinburgh. Intending to belong to the medical profession, he first became a pupil of Dr Barclay, then an extra-academical lecturer on anatomy. Ultimately he became his prosector, and was always seated beside him during the lecture. At first he seemed inclined to adopt surgery as his profession. In the year 1823 he became a Fellow of the Royal College of

Surgeons—Syme also being elected about the same time. He was intimate with Syme, Liston, Fergusson, and Wardrop, and often assisted these eminent surgeons when they operated. He afterwards went to London, and there was led to study dentistry. He probably foresaw, that there would be a favourable opening in Edinburgh, when Dr Law, who had a large practice as a dentist, died or retired.

Mr Nasmyth, when he began practice in Edinburgh, was the first who united the profession of a dentist, with the education and qualifications of a surgeon. He soon succeeded in obtaining public confidence.

He wrote very few scientific papers. The subject of his inaugural thesis had been "Tic Douleureux;" and, in the year 1843, he communicated to the London and Edinburgh Journal of Medical Science a comprehensive paper on the "Physiology and Pathology of the Teeth." I understand that most of the preparations in the Museum of the Royal College of Surgeons in this town, to illustrate the development of the teeth, were made by Mr Nasmyth.

The late Professor Goodsir was for seven years assistant to Mr Nasmyth, and has publicly acknowledged the valuable instruction he received from him. In 1842 Mr Nasmyth was elected a Fellow of the Royal Society of Edinburgh, but I do not think he contributed any papers or notices to our transactions. He was vice-president of the Odontological Society of London, and had been so for thirteen years before his death. He had held the offices of surgeon-dentist to King George IV., to King William, and also to Queen Victoria. He was a person of affable manners, and easy access. Dr Smith of Wemyss Place informs me that he kindly gave him much assistance in preparing the lectures which he delivered in Surgeon's Hall, and also in establishing the Dental Dispensary of Edinburgh.

Mr Nasmyth had in all four sons and four daughters. Two sons successively followed for a time their father's profession; but both died of consumption, as well as a daughter and another son. His third son was an officer in the artillery, and highly distinguished himself in the defence of Silistria.

Mr Nasmyth had a much larger and longer practice, in his

peculiar vocation, than any one before in Edinburgh. He was an agreeable companion, a fast friend, and possessed of much general knowledge. He will long be remembered as a skilful dentist, and a highly respected citizen of Edinburgh.

James Young Simpson was born 7th June 1811, and died 6th May 1870, being at the time Professor of Midwifery in the University of Edinburgh. His birthplace was Bathgate. The house in which he was born, is, I understand, still standing. It is a two-storeyed slated house, part of which has been converted by his brother Alexander into a hall used for meetings of various kinds. His father kept a baker's shop. His grandfather was a small farmer. He was the youngest of seven sons; and was sent by his father to the parish school.

He was sent to Edinburgh University to study medicine, and his expenses there were paid by his eldest and now only surviving brother, Mr Alexander Simpson of Bathgate, to whose kindness and brotherly care he was infinitely indebted. His parents both died when he was young. Whilst studying in Edinburgh, he lodged with his brother David, then in business as a baker in Stockbridge.

His taste for books in his boyhood was remarkable. He was constantly to be seen sitting at the corner of the fireplace devouring any books he could get, and oblivious of the talking or noise around him.

In the Humanity Class, he attracted the attention and patronage of Professor Pillans, who, learning that he wished to study medicine, but that he was scant of funds, recommended him to compete for a bursary endowed for the support of boys of the name of Stewart or Simpson. This advice he followed. An extended study of Latin and Greek was however required. He succeeded in gaining the bursary, thereby drawing £10 yearly for three years.

In the year 1832 he obtained his medical degree, and was immediately afterwards elected by his fellow-students—among whom he had become a favourite—to be Senior President of the Roya Medical Society of Edinburgh,—an institution which, for about a century and a half, has been supported chiefly by the University medical students.

Young Simpson's graduation thesis so pleased Professor John Thomson, who held the Pathological Chair, that he made him assistant in his house, and employed him in the arrangement of his library; and in this new position he made rapid progress, not only sucking in all the knowledge which the Professor possessed, but venturing on views and speculations of his own. He was permitted occasionally to read the Professor's lecture to the class when the latter was unable from feeble health to do so—the Professor himself, however, being generally present. It seems that young Simpson did not always confine himself to the mere reading of the lecture, but presumed occasionally to introduce verbally an exposition of his own ideas, to the surprise of both students and Professor. The latter, on one occasion, having heard some new and startling propositions from the chair, after the lecture was over, expressed his dissatisfaction in the retiring-room by saying to his young assistant, "I don't believe one word of *it*, sir."

Simpson having acquired some confidence in his own powers, thought of setting up for himself; and seeing in the newspapers an advertisement that a doctor was wanted to attend the poor in the parish of Innerkip on the Clyde, he offered himself. But he was rejected. He used to say that he felt this disappointment more keenly than any he ever met with in after life. I may add here what I think Simpson once told me, that an old-established medical practitioner in a town not far from Edinburgh, wishing to get a young licentiate as an assistant, and who might ultimately become a partner, gave out a subject for an essay among the medical students of the Midwifery Chair, intending to judge of their qualifications partly by their essays and partly by conversation. Simpson gave in an essay, and was one of those sent for, but was again doomed to disappointment; though from this village doctor he received much friendly counsel and a promise of future patronage.

During the next two or three years, he continued to prosecute his studies, chiefly in obstetrics, and read several papers in the Royal Medical Society. He also visited France. He now began to form a museum of preparations and objects bearing on anatomy, and at length announced his intention of giving public lectures. These he continued for three years, and they obtained so much

success, that he probably then conceived the idea, in the event of a vacancy in the University Midwifery Chair, of offering himself as a candidate.

In the year 1839 the venerable Dr Hamilton, who occupied that chair, died, on which event Simpson became a candidate, supporting his claims by an octavo volume of 200 pages of testimonials, and accompanied by a catalogue of the museum which, in the short space of three years, he had formed, containing no less than 700 obstetric preparations. The assiduity with which he plied his canvass, and the steps he took to overcome objections, may be judged of from the circumstance that one of the magistrates (the present Lord Provost of this city) having stated it as a drawback, if not a disqualification, that he was an unmarried man, Dr Simpson replied, "I admit it is a disqualification, but it may perhaps be removed." The next day he started for Liverpool, and contracted a marriage there with the daughter of Mr Walter Grindlay. In about ten days thereafter, he returned to Edinburgh; and having called on Bailie Law, he informed him of the step he had taken in deference to his opinion, and then claimed a promise of his vote—which he at once received. It was by that vote he won the Professorship.

After Simpson was elected, there were confident predictions that the obstetrical class in the University would fall off, and that many fewer patients would come to Edinburgh to be under the Professor's care. Animadversions fell freely on the magistrates, as patrons of the chair, for electing a man without either experience or reputation, instead of his opponent, who had both. These anticipations soon proved to be utterly unfounded. After Simpson's election the Midwifery Class was crowded. Not only did students flock to it in greater numbers even than formerly, but medical officers of the navy and army, when home on furlough, frequently attended to hear the original views of the youthful Professor, and were delighted by the aptness of his illustrations and the earnestness of his style of lecturing.

He also carried on obstetric investigations and experiments on various points of difficulty, accounts of which were given by him from time to time in papers read at Societies, or inserted in medical journals. He soon came to be employed extensively

as a practitioner, so that he had abundant opportunity of seeing cases, both novel and instructive, and of trying improved methods. At the same time, he was acquiring a complete knowledge of all that had been written by others, not only in Europe and America, but even by the Greeks and Romans,—his good classical knowledge in this respect proving serviceable. He allowed himself very little sleep; and even in the houses of his patients, whilst waiting in an adjoining room till his services were required, used to write out papers, or arrange materials for them.

His mind was so exuberant and versatile, that it often flowed over and beyond the pale of his own special department. Thus, one of his papers read before the Medico-Chirurgical Society in 1841 was entitled, "*Antiquarian Notices of Leprosy and Leper Hospitals in Scotland and England.*" Another had this title, "*Was the Roman Army provided with Medical Officers?*"

His great delight, and therefore his incessant aim, was to search out something new; and for this purpose, whilst he ransacked his own brain, he did not disdain to rummage among the rubbish of old authors, or to talk with any one who had anything to communicate on any topic whatever. One of the subjects, in his special department, which interested him greatly, was the use of anæsthetics. He had read of the experiments performed in America by several surgeons and dentists, to render their patients insensible to pain by inhaling sulphuric ether. He did not see why this substance should not be used in obstetric practice. Accordingly, he administered it to one of his patients for the purpose of lessening the pains of parturition. This case occurred on the 19th January 1847. Before that time, no one had ventured on such an experiment. It was entirely successful; and he thought it so important that, next day, he communicated the discovery to his class, and gave a special report of it to the Obstetric Society. The case got into the newspapers, and within ten days the process was repeated successfully in the hospitals of London and Paris. During the following six months, Dr Simpson continued the use of sulphuric ether both in the Edinburgh hospitals and in private practice, resorting to it, however, only in cases where nature had to be assisted. Simpson found several drawbacks in the use of sulphuric ether, and in consequence began to search for something

better. One of the many substances he tried was chloroform,—a liquid discovered in 1832 by two German chemists, and first accurately investigated and described in 1835 by Dumas of Paris. The trials which Professor Simpson made with the vapour of this substance, and which led him to adopt it, took place in November 1847. But it is right to add that, though he discovered its suitableness for the purpose wanted, and was the first to introduce it into surgical practice, the idea of so using it, had occurred to others previously, and trials had even been made with it. Thus Bouchardat, in a book called "*Nouveau Formulaire Magistral,*" published in 1845, and a copy * of which Professor Simpson was possessed of, under the head of "Chloroforme," observes—

"Cependant on peut se croire autorisé à regarder l'effect du Chloroforme comme antispasmodique, et à penser, que si une grande analogie de composition rapprochait cette substance des *ethers*, une *grande analogie d'action était également commune à chacune de ces substances.*"

Another French physician, Flourens, read to the Paris Academy in March 1847 a paper on the properties of chloroform, mentioning a number of experiments he had made of its effects on animals, and adding that "*he did not think it could be used with safety in medical practice.*"

Besides the information or hints derived from these sources, it must be added, that a Mr Waldie of Liverpool, who was chemist to the Apothecaries' Company there, being in Edinburgh during the month of October 1847, called on Professor Simpson; and on the Professor telling him that he was seeking for some better anæsthetic than sulphuric ether, Mr Waldie spoke to him of *chloric ether*, and *advised* him to try *pure chloroform* unmixed with alcohol. He asked Mr Waldie to submit to anæsthesation by chloroform, but Mr Waldie was not willing to risk the experiment.

Acting on this hint, Professor Simpson procured—I believe from Professor Gregory—a small quantity of pure chloroform, which, however, he did not at the moment make use of. It was put aside, to be tried with other substances at some more convenient opportunity. Late one evening—it was the 4th November 1847—to quote from Professor Miller's pamphlet, Professor Simp-

* I state this, on the authority of the Editor of the Edinburgh Medical Journal for Nov. 1870, p. 441.

son resumed his experiments, aided by his two friends and assistants, Drs Keith and Matthews Duncan—

"Having inhaled several substances, but without much effect, it occurred to the Professor to try a ponderous material, which he had formerly set aside on a lumber table, and which, on account of its weight, he had hitherto regarded as of no likelihood whatever. That happened to be a small bottle of chloroform. It was searched for and recovered from beneath a heap of waste paper. With each tumbler newly charged, the inhalers resumed their vocation. Immediately an unwonted hilarity seized the party. They became bright-eyed, very happy, and very loquacious—expatiating on the delicious aroma of the new fluid. The conversation was of unusual intelligence, and quite charmed the listeners—some ladies of the family, and a naval officer, brother-in-law of Dr Simpson. But suddenly there were sounds like those of a cotton mill, louder and louder. A moment more, then all was quiet; and then—a crash. On awaking, Dr Simpson's first perception was mental. 'This is far stronger and better than ether,' said he to himself. His second was, to note that he was prostrate on the floor, and that among the friends about him there was confusion and alarm. Hearing a noise, he turned round and saw Dr Duncan beneath a chair, his jaw dropped, his eyes staring, his head bent half under him,—quite unconscious, and snoring in a most determined manner. More noise still, and much motion, caused by Dr Keith's legs making valorous efforts to overturn the supper-table. By and bye, Dr Simpson having regained his seat, Dr Duncan having finished his uncomfortable slumber, and Dr Keith having come to an arrangement with the table, the sederunt was resumed. Each expressed himself delighted with the new agent, and its inhalation was repeated many times that night—one of the ladies gallantly taking her place at the table—until the supply of chloroform was exhausted. In none of these subsequent inhalations, however, was the experiment pushed to unconsciousness. The first event had quite satisfied them of the agent's full power in that way. The festivities on the occasion did not terminate till three in the morning."

Such is the graphic account given by the late Professor Miller of the way in which Simpson discovered the properties of chloroform vapour. The value of the discovery depends upon the superiority of chloroform to sulphuric ether, the anæsthetic previously employed in medical practice; and its superiority was manifested thus, viz.—1*st*. That a much less quantity of chloroform answered;—2*d*. That insensibility came on more rapidly;—3*d*. That no special instrument for its administration was required;—4*th*. That the odour was more agreeable.

On the 8th November 1847, this new anæsthetic was employed by Professor Simpson in a case of labour for the first time, and with complete success. It soon became known in the profession.

and it has in this country almost superseded every other anæsthetic, both for aiding parturition and for numberless surgical operations. In these operations, especially, it has been of incalculable service, not only by relieving from suffering, but by saving life. I observe a statement by an American army physician made lately at a public meeting in Washington that—*

"In the Crimea and Italian campaigns, chloroform was employed without a single disaster. A similar result attended its use during the seven weeks' Austro-Prussian war. In our own unhappy struggle [he alludes to the American Civil War] chloroform was administered in more than 120,000 cases, and I am unable to learn of more than eight cases in which a fatal result can be fairly traceable to its use."

The immense quantity of chloroform manufactured, is a sufficient proof of the trust universally placed in it, and of the immense amount of human suffering relieved by it. In October 1869, when the freedom of this city was bestowed on Simpson, he mentioned that the distinguished firm of apothecaries in Edinburgh, who manufacture chloroform, were making it in such quantities as to yield about 8000 doses daily. On inquiry last week, I learnt from Mr Flockhart, that the quantity of chloroform now manufactured in this town is about double what it was a year ago, partly in consequence of the sanguinary European war which has raged for the last five months, but chiefly in consequence of the rapidly increasing use of chloroform in general practice. Mr Flockhart told me that just before Paris was invested, he sent to the medical staff there 1000 bottles of 1 lb each,—which he heard had reached their destination. He also sent 800 bottles to the Germans. These went chiefly to the army of the Crown Prince.

Numerous were Simpson's discoveries and improvements, even in departments of medicine which lay outside of his own special field. The stopping of hæmorrhage from cut arteries is effected by ligatures or torsion. He proposed pins or needles, by which to close the artery.

With a view to arrest the spread of epidemics, he urged the complete isolation of the patients affected; maintaining that, as rinderpest could be stamped out by the immediate slaughter of cattle attacked by it, so scarlet fever, measles, hooping-cough, and

* Ed. Med. Journal for Nov. 1870, p. 478.

even small-pox might be, if not extinguished, at all events arrested, and so cease to be epidemic, by strict confinement and complete isolation of the first individual attacked.

His views on the subject of large hospitals were founded on the same principle. He insisted that, where large numbers of sick persons were accommodated in one building, the atmosphere of the building became tainted, so that the patients had less chance of recovery; and this position he attempted to prove, by contrasting the proportion of recoveries in hospitals with those in private dwellings.* On these grounds Simpson advocated the abolition of large hospitals in towns, and the substitution of detached cottages in the country; but if hospitals were to be retained, then instead of wards, with from fifty to one hundred beds in each, and reached by lobbies and staircases inside of the house, he urged that the wards should contain as few beds as possible, and that access should be had to them by stairs outside of the hospital altogether.

That the principle on which these views are based, as to the expediency of isolating persons afflicted with any complaint whatever, is a sound one, none can doubt, who has read the recent discoveries of minute and invisible organic dust in the atmosphere, consisting in many cases of germs—germs which, inhaled, and entering the blood, engender diseases in the body.

I see it stated in a well-informed medical paper that, among

* In the speech which he made on receiving the Freedom of the City, he remarked that—"When such a simple operation as amputation of the *fore-arm* is performed upon a poor man in the country, and in his own cottage home, only about one in 180 dies. But the statistics of our large metropolitan hospitals disclose the stern and terrible truth, that if these men had been inmates of their great wards, thirty of them, or about one in six, would have perished; a fact, among many others, which calls earnestly and strongly for some great reform in our large hospitals, if these institutions are to maintain their ancient character as the homes of charity and beneficence." These statistics applied to the amputation of the *arm*. He gathered similar statistics from the hospitals, and from country practitioners, in regard to amputations of the *leg*, which showed that these amputations in like manner were always more successful in the country than in town hospitals, notwithstanding the greater skill of town surgeons; and he deduced the following conclusions:— "1*st*. That about three times as many patients die after *limb* amputations in our large hospitals, as die from the same operations in private and country practice. 2*d*. That to reduce the death-rate from operations in our surgical hospitals, we should strive to assimilate their form and arrangements to the condition of patients in private and country practice."

Professor Simpson's unpublished papers, some notes have been found bearing on hospital reform. That he felt there was something more which he could have done on that subject, is evident from a remark made during his last illness, when informed that his recovery was doubtful. He said that his principal reason for desiring a prolongation of life, was that he might do a little more service in the cause of hospital reform.

These suggestions for improved practice, in the various departments of the medical profession, exposed Professor Simpson to much controversy. Naturally zealous and ardent, and knowing that energy and perseverance were required for any reform which was likely to disturb old customs, or existing interests, he frequently drew down on himself opposition of a disagreeable and personal character. Thus, with reference to his proposal to substitute acupressure for deligation, the Professor of Clinical Surgery, in the same University, complained bitterly of his interference in matters alien to the midwifery chair; observing that *he* had not interfered, as he might have done, to denounce certain useless and often dangerous innovations introduced in the treatment of diseases of women.

The amount of private practice which Professor Simpson obtained, not only in his own special department, but even in other cases, is probably greater than any one ever before possessed. No other result could be expected, as the discoveries and improved practices which emanated from him, indicated not only knowledge to an immense extent, but inventiveness in meeting the most difficult cases. He had also an agreeable expression of countenance, and a melodious voice, qualities which, in a sick room, made his attendance doubly acceptable. I have often seen in his house, after two o'clock, a levee of patients of all classes, rich and poor, amounting sometimes to hundreds, desirous of consulting him. Not only were the drawing-room, dining-room, and library filled, but even the lobby and passages. Frequently persons had to leave without being able to see the Professor, after waiting two hours. A relative of my own, having succeeded in catching him as he looked into the room where she was waiting, told her case to him. He then, without saying anything, left the room, but immediately returned with a

book, in which he pointed out to her the part where she would find her ailment described. He asked her to read it whilst he went to another patient, promising to come back in a few minutes. Having read the passages, and waited patiently an hour, she rang the bell to inquire for the Professor, and found he had left the house, having forgotten his promise to return.

Professor Simpson was untidy in his dress, and on one occasion much offended a lady of rank who called on him at his house, by coming to see her in his "stocking soles." Frequent complaints were made by patients, as to his want of punctuality in returning to visit them. One lady, having been desired by him to remain in bed till he returned again in a day or two, remained ten days in bed, waiting for his return. He had been called to the country, and had forgotten this town patient altogether.

It was indeed not to be wondered at that, with such multitudes of objects engrossing his thoughts, he should be occasionally distracted and diverted from his professional engagements. Nevertheless, so great was the confidence reposed in his skill, that these breaches seldom caused patients to forsake him. Traps were often laid to catch him for attendance, or a consultation. With that view persons went to his house to breakfast though uninvited, and they were always graciously received. Sometimes when they saw his carriage standing at a door, they used to get into it and wait till the Professor came out from his visit.

It has been estimated, by those who had means of knowing the extent of Simpson's practice, that the number of strangers who came to Edinburgh for his advice and treatment, must have caused an expenditure of at least £80,000 a-year among the hotel and lodging-house keepers.

It is obvious that, on account of Professor Simpson's extensive practice, the instruction which he was capable of giving must have been most valuable. Nor was it only in the class-room and to students, that instruction was given by him. He was ever accessible to his professional brethren, and particularly to country practitioners, when they were at a loss in cases of difficulty. One of this last class,* who frequently resorted to him, having been

* Dr Turnbull of Coldstream. He has allowed me to quote from his letter.

asked by me for any notices of his deceased friend, wrote as follows:—

"My own success in practice has been far beyond anything I ever anticipated when I commenced it, now upwards of a quarter of a century since and, beyond all question, I feel indebted to Simpson, more than to all my other teachers put together. He was loveable and winning to an extent which no words of mine can express. I spent the forenoon of the day on which he returned from the Mordaunt trial with him. Then he performed upon a patient of my own, a difficult operation, on which he showed great resource and skill, probably the last operation of importance he did. He gave me an account of the trial, and of Serjeant Ballantyne's examination. He inquired most anxiously about Dr Watson's lecture given the previous night at the Royal College of Surgeons,* at which I was present, and at his absence from which he expressed great regret. A part of the day on which he died, I spent with Dr Warburton Begbie; and when he told me that I would never see Simpson again, adding 'I know full well how genuine has been your mutual friendship for many long years,' I could give no reply. The tears stole down my cheeks, and I experienced then, and many a time since, a genuine sorrow which I need not describe. To his faults I was not blind, and for them he has assuredly been sufficiently abused by those who think that he only was blameworthy. While I live, I shall never cease to think of him, as I always found him, generous, attractive, and loveable, far beyond any other man whom I ever met."

Let me add, that he did not confine his teachings and counsel to students and to medical practitioners. To all and sundry who chose to consult him, and who could obtain access to him, he was ever ready to open up the stores of his wonderful memory and inventiveness. On the last occasion that I had a lengthened conversation with him, he adverted to the future prospects of medical discovery, and pointed out that these would depend more on the chemists than on any other class of investigators. He remarked, how little we yet knew the reasons why particular medicines were efficacious in arresting disease, and said that he thought no medical student should receive a licence who was not an expert chemist.

Whilst ready to teach verbally, whether in the University, or in medical societies, or in his own house, he had little taste for writing medical books, but it was a recreation to him to write on archæological subjects. The two large volumes on obstetrics, which bear his name, were published, not by him, but by two medical friends, who undertook the labour of collecting and arranging his papers and

* The subject of lecture was Hospital Reform.

notices, published and unpublished. In the few words of preface to the first volume, written to express his gratitude to Dr Priestley and Dr Storer who edited the work, Professor Simpson states that most of the communications, which appeared in it, "were written hurriedly, and amid the incessant distractions of practice." He adds, "If I had attempted to remodel, extend, and correct them, they would never have been published in a collected form." Why not, he explains in his preface to volume second, in these words, "The life of a busy accoucheur, is not a life fitted for literary work. Besides, I am quite deficient in some of the principal qualifications generally laid down as requisite for success in medical authorship; having no heart or habit for the daily written annotation and collection of individual cases and observations—no sufficient industry and endurance for the pursuit of any tedious and protracted investigation, and no great love of lifting my pen, but the very reverse."

The reasons thus assigned by Professor Simpson why he would never have published these two volumes, must, of course, be accepted. But there was probably another and a stronger reason, which it might have been thought ostentatious for him to mention, —and that was his insatiable love of discovery—his constant desire to be ever searching for new truths, and to occupy as much of his time as possible on fields where these truths were likely to be found. He would have considered it a waste of time to have gone back on his own previous researches, in order to present them again before the world in the form of a published work. That was a mechanical labour which he willingly and wisely handed over to the kind friends who voluntarily undertook it, and thus he was left free to apply his time and talents to the nobler business of advancing human knowledge by fresh discoveries.

His active and buoyant mind, not finding enough to occupy it within the circle of medicine, sought more work in other fields, and hence he was led to become a member of various societies of a scientific character. The first that he joined after becoming Professor of Midwifery, was *our own Society*. He joined it in the year 1844, and contributed the following papers, which were read at our evening meetings, and afterwards printed in our Proceedings:—

On the 16th December 1850. Notice of a Roman Practitioner's Medicine Stamp, found near Tranent.

On the 6th March 1857. History of an Anencephalic Child.

On the 19th December 1859. On Acupressure, a New Method of arresting Hæmorrhage.

On the 6th April 1863. Note on the Anatomical Type in the Funis Umbilicalis and Placenta. (Transactions, Vol. XXIII.)

On the same night. Note on a Pictish inscription in the Churchyard of St Vigeans.

On the 2d January 1866. Notices of some Ancient Sculptures on the walls of caves in Fife.

On the 26th January 1868. Pyramidal Structures in Egypt and elsewhere; and the objects of their erection.

With reference to this last paper, the chief purpose of which was to refute Professor Piazzi Smyth's theory about the origin and object of the Great Pyramid of Egypt, it has been publicly stated, by a person who alleges he knew the fact, that to enable him to test the correctness of Professor Smyth's calculations, and to write the papers above referred to, he devoted three weeks to a study of decimals and a perusal of astronomical works;—a proceeding which shows the zeal and energy with which, even at a late period of life, he could take up a new subject.

Another Society, unconnected with the profession which he joined, and in the business of which he took almost inconceivable interest, was that of the *Antiquaries* of Scotland. Every volume of the "Transactions" of that Society, after he joined it in the year 1859, teems with notices from his pen; and a very considerable number of the articles in the Society's instructive museum were donations from him. I have heard that he had formed a kind of map or glossary applicable to both England and Scotland, showing the sites of curious old buildings, camps, or standing stones; so that on the occasion of making any professional visits to districts where these relics occurred, he might contrive to see them.

When made a Vice-President of the Society of Antiquaries, he delivered an address, which for archæological lore and acquaintance with the early history of Scotland, astonished those who had made this subject a special study all their lives. This address was

published, and had a motto from Wordsworth prefixed to it, truly expressive of the heartfelt pleasure which these researches gave to him. The motto was—

> "I have owed to them
> In hours of weariness, sensations sweet
> Felt in the blood."

I remember being so struck with this address, that after reading it, I begged a common friend to ask Sir James, how and when he had found time to compose it. His answer was, that he had written it, after twelve o'clock at night, as he always felt refreshed by writing papers of that kind. There is a paragraph at the conclusion of this address, which deserves to be quoted for its own sake, and because it led to an occurrence which illustrates Professor Simpson's readiness to aid in any good object.

"In the name of this Society, and in the name of my fellow-countrymen generally, I here solemnly protest against the perpetration of any more acts of useless and churlish Vandalism in the needless destruction and removal of our Scotch antiquarian remains. The hearts of all leal Scotchmen, overflowing as they do with a love of their native land, must ever deplore the unnecessary demolition of all such early relics and monuments, as in any degree contribute to the recovery and restoration of the past history of our country and of our ancestors. These ancient relics and monuments are in one sense national property, for historically they belong to Scotland and to Scotsmen in general. more than they belong to the individual proprietors upon whose ground they happen to stand."

Shortly after this address was published, a visit was paid by the Berwickshire Naturalists' Club to a remarkable old fortress in Berwickshire, called Edins Hald, situated among the Lammermuir Hills. Those members of the Club who had known the building in former years, were distressed to see how much it had been mutilated, and to hear, that it was about to be again used as a quarry, for some stone dykes soon to be erected. The Club addressed the proprietor on the subject, with the view of obtaining a promise to prevent farther dilapidation. He, however, showed no disposition to grant our request. We resolved then to submit the matter to Professor Simpson, on the faith of the admirable address to which I have just adverted. It turned out fortunately for us, that the wife of the proprietor, who resided near Edinburgh, was then attended by Professor Simpson. He willingly undertook to intercede with her on behalf of this old relic, and obtained from

her husband a letter containing a written promise to have the ruin protected from further injury; which letter he handed over to the secretary of the Society of Antiquaries.

Professor Simpson made several visits to Northumberland, to examine the sculptured rocks at Old Bewick, Doddington, and Roughting Linn, as well as to inspect the excavations of the British forts, dwellings, and sepulchres on Yevering Bell, among the Cheviot Hills. On one of these occasions, he joined a meeting of the Berwickshire Naturalists' Club—of which club he was a member; but not being able to keep up with the party, walking through long wet brackens, and over rough moorland, he borrowed a horse. Not being a good rider, he soon came to grief, in a bog which had to be crossed. The horse finding himself sinking, reared, and tumbled the Professor into the mud, out of which he was extricated, with some difficulty, and to the no small detriment of garments. After getting through the bog, he valiantly mounted again, glad to have that method of reaching the top of one of the highest of the Cheviots.

One of the archæological topics on which Professor Simpson wrote an interesting paper, was a history of the Oratory on the island of Inchcolm. I understand that he had collected materials for a similar account of all the islands of the Firth of Forth—on most of which there are still traces of ancient ecclesiastical edifices. I know also, that he had begun to write an account of the Roman Wall, extending between the Firths of Forth and Clyde, as he once spoke to me on the subject, wishing to know my opinion of Mr Geikie's theory, that this district of Scotland had risen twenty or thirty feet out of the sea, since the wall was erected. It is to be hoped that if his MSS. on these subjects are found, they will be put into a proper form for publication.

Animal Magnetism, Mesmerism, and Biology, were subjects, which at an early period, he studied; and for a time he was much impressed with the phenomena:—so much so indeed, that he used to hold "seances" in his own house, and show that he himself possessed a certain strange power over others. I have heard of his even performing in the houses of his friends, at evening parties,—when selecting some one, whom by a mere glance he discovered to be particularly nervous or sensitive, he would show

how completely a strong will could so influence the mind of another. as to cause confusion of ideas almost amounting to imbecility.

This meddling with mesmerism brought the Professor into some disrepute; and he was severely attacked in the Medical Journals, for his supposed credulity. At first, he took no notice of these attacks; but in consequence of the solicitation of his friends he in September 1851, published a letter in the "Lancet" explaining the object of his miscalled "mesmeric soirees." In that letter he says—

"During the last ten or fifteen years, I have repeatedly seen experiments, and also made them myself. In the course of them I have witnessed very interesting physiological and psychological results, such as the production of deep sleep, fixture and rigidity of muscles, &c. But I have no belief whatever, that these phenomena are the effects of any power, force, or agency such as is understood by the term '*animal magnetism*.'—passing from the so-called 'mesmeriser' to the so-called 'mesmerised.' They are merely the effects produced by the mind of the 'mesmerised' upon his or her own economy;—self-mental acts so to speak. These may no doubt be produced by the influence of the will of one individual acting on another. But they are no proof of any magnetic, mesmeric, or other supposed agency. In proof of my utter disbelief in *clairvoyance*, I may state that having sometime ago been present at a lecture on the subject, I offered to place L.100 in the hands of the President of the Medico-Chirurgical Society which he was to give to the lecturer, if the latter would bring any clairvoyant, who could read a line of Shakespeare, or two or three words out of a dictionary, which he (Professor Simpson) would shut up in a box."

Professor Simpson had no patience for the quackery and credulity of spirit rapping; and as Faraday condescended to expose "table turning" by a written opinion which he sent to the "Times" newspaper, so in like manner Professor Simpson took occasion, in the course of his address to the Society of Antiquaries, to remark—

"In our own days many sane persons profess to believe in the possibility of summoning the spirits of the departed from the other world back to this sublunary sphere. When they do so they have always hitherto, as far as I have heard, encouraged these spirits to perform such silly, juggling tricks, or requested them to answer such trivial and frivolous questions as would, to my humble apprehension, seem to be almost insulting to the grim dignity and solemn character of any respectable ghost. If, like Mr Home, I had the power to call spirits from the vasty deep, and if the spirits answered the call, I, being a practical man, would fain make a practical use of their presence. Methinks, I should next ask them hosts of questions regarding the state of society, religion, the arts, &c., at the time when they themselves were

living denizens of this earth. Suppose that our Secretaries, in summoning the next meeting of this Society, had the power of announcing in their billets that a very select deputation of ancient Britons and Caledonians, Picts, Celts, Scots, and perhaps of Scottish Juranians, were to be present in our Museum for a short sederunt between midnight and cock-crowing to answer any questions which the Fellows might choose to ply them with, what an excitement would such an announcement create! What a battery of quick questions would be levelled at the members of this deputation on all the endless problems of Scotch archæology."

About the same time Professor Simpson took part in the discussions which agitated the medical world on the subject of *Homœopathy*. At a meeting of the Edinburgh Medico-Chirurgical Society, the following motion was made by Professor Syme, and seconded by Professor Simpson:—"That the public profession of Homœopathy shall be held to disqualify for being admitted, or remaining a member of the Society." Professor Simpson supported this motion by a very able address, which he afterwards expanded into a book. This, as well as the reply to it by Professor Henderson, shows an immense extent of reading and information.

Another subject which deeply engaged Professor Simpson's attention was the so-called *Bathgate coal*, and also the *shales* of the Scotch coal fields, on account of the petroleum which they yielded by proper treatment. I have seen the outer lobby of his house in Queen Street greatly obstructed with huge specimens of the various kinds, and occasionally he spoke to me regarding them; not so much in their geological relations as in their mercantile value and uses. It is matter of notoriety that Professor Simpson joined one or more of the companies which were formed for the purpose of extracting oil from these beds, and it is understood that he suffered considerable losses in consequence.

The number and variety of topics which thus engaged Simpson's attention—professional, scientific, literary, and speculative—implied an activity of mind, a grasp of intellect, and a strength of constitution truly marvellous. His inquisitiveness on almost all subjects was incessant. "Anything new turned up in Berwickshire?" was the first question which he generally put to me when on coming to Edinburgh I happened to meet him,—hoping probably to hear of more Picts' houses discovered, or more relics

found at the old Broch on Cockburn Law. His greatest delight and recreation was to explore ancient ruins, caves, and encampments; to decipher inscriptions or sculptures on standing stones or rocks; and to explore the rubbish of antiquated chronicles or musty parchments. Legends, superstitions, and charm stones were not beneath his notice, and were carefully studied, in the hope of extracting from them some gleam of historical truth. As a ray of sunlight enters a prism colourless and comes out radiant with beauty,—so these old inscriptions, sculpturings, and legends, after passing through Simpson's scrutiny, often appeared in a new light, and gave out a meaning not before suspected.*

His memory was surprising. Notwithstanding the legions of books which he read,—notwithstanding the numbers of places he visited, and the multitudes of facts which he gathered up at these visits,—he made no notes, and kept no diary, as most persons have to do. Any information obtained, whether from his own observation or from other persons; or any new views expressed on subjects which interested him, he seldom forgot; and could at once reproduce or refer to, when necessary.

Professor Simpson, engaged as he was in the teaching of youth, and attentive to subjects of public interest, could scarcely avoid taking some part in the educational discussions which have occurred during the last ten or twelve years in Scotland. The points he chiefly urged for improving public instruction were peculiar, and gave surprise to many of his friends. As President of the Granton Literary Association, he, in November 1867, delivered an address or lecture, which was published, "on the necessity of some change in the mode and object of education in schools, *in reference to modern and ancient languages.*" In this lecture the following pithy sentences occur :—

"Should they teach the *modern* languages, that are throbbing with life and activity? or should they teach the *old* languages of Greece and Rome spoken 2000 years ago?

"Was it right that one-seventh of a man's life should be spent in the acquisition of these dead languages? For the clerical profession, he admitted

* As examples, see Simpson's paper on "*The Cat-stane; Is it not the Tombstone of the Grandfather of Hengist and Horsa?*" Also to his paper "*On Ancient Sculpturings of Cups and Concentric Rings in Scotland.*"

this was a necessary study. But it was no longer necessary for the mass of the people.

"It was said that Latin and Greek were the best training. This he thought a great error; for the faculty called into exercise was chiefly memory. The power of observation required in science and art was called little into play, and the reasoning power of the mind became stunted and deformed;—to such a degree, indeed, that the students were ignorant even of their own ignorance."

In like manner, in his address to the Society of Antiquaries, he took the opportunity of undervaluing classical education, by such declarations as these:—

"Archæology has gained for us a clearer and nearer insight into every-day Roman life and habits, than all that *classic literature* supplies. Archæology, by its study of the old works of art belonging to Greece, has shown that a livelier and more familiar knowledge of that classic land is to be derived from the contemplation of its remaining statues, sculptures, gems, models, and coins, than by any amount of school-grinding at Greek words and Greek quantities."

It is the more surprising that such views as these should have been put forth, considering the frequent and good use to which Professor Simpson put his own classical information. In his papers on "*Roman Medical Stamps*," and "*Was the Roman Army provided with Medical Officers?*" he was able to give information, not only interesting, but instructive and useful, both papers displaying an extensive and intimate acquaintance with Greek and Roman authors. In his work on Anæsthetics, he devotes two chapters to obviate the theological objections taken to their employment to lessen the pains of child-bearing, and in these chapters discusses the true meaning of the Hebrew text of certain scriptural passages.

I have hitherto spoken of Simpson chiefly as regards his professional knowledge and his varied scientific and intellectual attainments. But it would be wrong in me to pass over unnoticed other features of his life and character quite as remarkable. He was a man of strong emotions. It of course depended on the exciting cause, how these influenced him. When attacked professionally or otherwise;—or when, after he had set his heart and hand to the attainment of some object, he found himself opposed, he was like a war-horse in a battle-field. His impetuosity sometimes carried him too far, brought him upon dangerous ground, and caused him to resort to means for accomplishing his ends

which he himself afterwards regretted. He hit his opponents severely, and I think even in this room expressions dropped from him which, in a scientific discussion, were out of place. But he was not of an unforgiving temper. I myself know, that he could offer the hand of reconcilement, after a contest was over. I saw the other day in a medical newspaper * a statement that not long before his death, he sent letters to some of his professional brethren whom he thought he might have hurt in the heat of controversy, expressing regret and asking forgiveness. Being curious to know whether this was really the case, I applied to one of the medical gentlemen who attended him during his last illness, and he informed me that he did not know of any *letters* to that effect; but he knew of a *message* having been sent to one professional gentleman, then also unwell, with whom there had been bitter controversy and long estrangement,—and the result was complete reconciliation.

I have already alluded to the multitudes of patients who every day thronged his house. The poor always could rely on getting advice from him gratuitously. But he was never very exacting from any class; and when persons in a better rank of life, who had come for advice, were discovered by him to be in greatly embarrassed circumstances, he is known to have generously helped them.

Two examples of this generosity may be mentioned. A lady whom he had attended was recommended by him, for the cure of her ailment, to go to a certain watering-place. Tendering to him such a fee as she was able to give, and for the smallness of which she apologised, the lady mentioned that the expense of going there would put it beyond her power. Simpson said nothing at the time, but afterwards in the most delicate way returned the fee, and enclosed £20 to enable her to obtain the means of cure which he had recommended. The other case was the wife of a New York merchant who had come to Scotland to be under his care. Whilst here, her husband died, and in bankrupt circumstances. Shortly after this, intelligence reached her that her only son, whom she had left at New York, was ill with a dangerous fever. She resolved at once to return home, though she was to have remained longer

* Medical Times and Gazette, 14th May 1870.

under the Professor's care. She was obliged to explain to him the cause of her abrupt departure, and to ask him to wait for payment of his services till she returned home. He not only intimated to her that he would accept no fee, but gave her in a present enough to pay her passage to New York.

His kindness was not confined to his patients. From persons who were entire strangers to him, and who were merely passing through Edinburgh, hospitality was never withheld. His breakfast and luncheon table was often crowded by foreigners, who, knowing the Professor no otherwise than by his world-wide reputation, and being told that he was extremely accessible, used to send in their cards, and received from him a cordial welcome.

Professor Simpson, in the spirit of true philanthropy, took much interest in the welfare of that wretched part of the population of Edinburgh occupying cellars, and frequenting haunts of vice in the Old Town. Many a time did he visit them at night, after his day duties were over. Moreover, he tried to interest others in their behalf, forming for that purpose, at his own house, parties of gentlemen and even ladies to accompany him. But the practice gave offence, and was discontinued.

Professor Simpson was imbued with strong religious feelings. Most persons here will probably remember how, in narrating the conversation which he had with Sir David Brewster on his death-bed, he was evidently pleased to be able to testify to the Christian faith of the dying philosopher. Simpson both lived and died a Christian; not only holding fast his trust in the Saviour, but desiring to impart the same comfort to others. His name may therefore well be added to those of Faraday and Brewster, who in our own day have shown that the highest attainments in philosophy and science, are not incompatible with strong religious feeling and the sincere faith of a Christian.

Professor Simpson was so remarkable in his outward appearance and expression, that any one, even happening to meet him in the street, could not fail to take special notice of him. Though short in stature, he had large features, and a shaggy head of unkempt hair. His eye was piercing, and his lips expressive. The energy of his physical constitution was wonderful, and he taxed it severely. Thus, after going to Oxford, to receive a University distinction,

he started next morning with two friends for Devizes, from whence he went on to Avebury to see "the standing stones," not getting back till midnight. On the following morning at five o'clock, he started for Stonehenge, and the same afternoon went to Bath to visit the Roman remains in that neighbourhood. On getting back at midnight, he found a telegram summoning him to a patient in Northumberland. He lay down for a few hours to sleep, and then went by the 4 A.M. train to London, and caught the Scotch "Express," which took him to Northumberland, from which place he went on to Edinburgh to resume his usual professional work.

What constitution could stand such incessant wear and tear? A severe attack of rheumatism followed the fatiguing journeys I have been describing, and this complaint continued frequently to torture him during the last two years of his life. Eventually the action of the heart became impaired, and *angina pectoris* supervened,—causing occasionally intense agony.

The fatigue and cold endured last February, in journeys made to London on the occasion of Lady Mordaunt's trial, brought on the illness which proved fatal. For two months he was confined to the house, and chiefly to bed, though even then he was able to write a letter on the subject of chloroform for publication in an American Medical Journal, the object of which was to refute some one who, in the previous number, had been endeavouring to dispute that he was the first to apply chloroform to anæsthetic purposes.

My sketch of Simpson's life, imperfect as it is, would be still more so, were I to omit notice of the distinctions which were showered upon him from almost every quarter of the globe. I cannot recount all the Academies, Universities, and Societies which bestowed their honours upon him. There was not one nation in Europe from which these honours did not come, and America joined in the general acclaim. Simpson was created a baronet of the United Kingdom. He received the knighthood of the Swedish Royal Order of St Olaf. He was made a laureate of the Imperial Institute of France; and the French Academy of Science bestowed on him what is called the "Monthyon Prize" of 2000 francs, given for any great discovery beneficial to humanity.

Gratifying to Simpson as these honours and distinctions no doubt were, there was one fact which must have been even more gratifying, and that was the introduction of chloroform, for medical purposes, in every civilized country, coupled with the almost universal acknowledgment that he had been the first to suggest and employ it for the relief of human suffering. He must also have felt that the world generally accorded to him the highest eminence in his profession, inasmuch as patients had come to him from every quarter of the globe, and as his works had been translated into every European language. Probably no man ever lived who, at the close of life, had the satisfaction of looking back on the same amount of work done for the benefit of his fellow creatures, and of possessing so largely their approbation and confidence.

In these circumstances, it is not surprising that, at the suggestion of the most eminent of the medical faculty in London, and warmly seconded by men there of high social position, a proposal was made, soon after Simpson's death had been announced, that his remains should be interred in Westminster Abbey,—that last resting-place of Britain's most illustrious sons. But the proposal was modestly, and I think properly declined by the surviving members of his family. Their decision was in this respect in accord with the unostentatious character and habits of the deceased. It was right and becoming that a man of his domestic dispositions should not be separated, even after death, from the other members of his own family, to whom he was deeply attached, but that he should lie beside them in the spot which he himself had selected, and where several had already been buried. Moreover, his interment at home allowed of an honour being conferred on him at his funeral, which, to my mind, was far greater than entombment in Westminster Abbey;—for his funeral was attended by all the public bodies and corporations of Edinburgh, and was thronged by thousands of sorrowing mourners, who, even from distant parts of the country, came to pay the last tribute of respect to one who had been so great a benefactor of the human race.

We have all to lament that our deceased friend and associate should have been cut off in the meridian of his fame, and whilst still running a career of usefulness. But we have reason to be thankful that his life, short if reckoned by years, was long, if

reckoned by good deeds and great services, not the least of which was the example he bequeathed of a man devoted to noble pursuits, characterised by incessant industry, imbued with benevolent dispositions, animated by Christian faith. In the letter already referred to, written on his death-bed, for the American Journal, he concluded it by saying, that he regarded the friendship of his medical brethren in America so highly, that he would not think this last effort at professional writing, altogether useless, if it tended to fix his memory in their love and esteem. It was to friends abroad, that this appeal was made. To friends at home, no such appeal was required. He knew that he had accomplished, what would for ever fix his memory in *their* love and esteem. To that sentiment, sure I am that his own countrymen and countrywomen cordially respond; and not less sure am I that the Fellows of this Society will ever remember with respect the eminent and diversified talents, as well as the signal services to science and humanity, of their distinguished associate.

James Syme was born 7th November 1799, and died 26th June 1870. Up to within a year of his death, he was Professor of Clinical Surgery in the University of Edinburgh, which chair he had held for thirty-six years. His father had originally followed the profession of a Writer to the Signet, but had retired at an early period with his family to the estate of Gartmore and Lochore in Fife. It is understood that, in consequence of there being no public school in the country which he could conveniently attend, Mr Syme obtained a tutor for his son whilst resident in Fife, so that he had in his early days no opportunity of associating with other boys,—a circumstance which may perhaps account for his shy and reserved manner in after life. Whilst a boy, it is said that he indicated a taste for anatomy, by frequently resorting to a butcher's shop, where he watched with interest the cutting up of sheep and oxen. His father at length seeing the necessity of giving to his son a better education and training than he was receiving in the country, sent him to Edinburgh to attend the High School. Afterwards, at the age of sixteen, he passed to the College, and became much interested in chemistry. When he returned during the holidays to Fife, he generally brought with him a supply of

apparatus—purchased with his own pocket-money—to enable him to carry on chemical experiments for his amusement.

So early as the year 1818 he had discovered a solvent for caoutchouc in the naphtha obtained by distillation from coal-tar, and in March of that year addressed a letter describing his discovery to Dr Thomson, then editor of the "Annals of Philosophy," which appeared in that publication in August following. Mr Syme in this letter states that "he had, by means of the discovery, waterproofed a *silk cloak*, so that it afforded complete protection from the heaviest rain, and could be employed as a pitcher by turning up its skirt." He adds that he had "constructed flexible tubes of the same substance." It appears that he had worked at this subject for two years before the discovery. The discovery was deemed so important, that Dr Thomson and some of his friends recommended young Syme to take out a patent, assuring him that it would make his fortune. But by this time he had determined on following the medical profession, which he thought more respectable than that of a manufacturer. He therefore contented himself with publishing his discovery, and receiving general commendation for his disinterestedness. Not long afterwards the discovery was turned to good account, as we all know, by Mr Macintosh of Glasgow, who made a large fortune by means of it, and who gave his name to the cloth, though in reality invented by Syme.

Syme became a pupil of Dr Barclay in order to study anatomy; and in 1818 he went into Liston's dissecting-rooms, as his assistant. He was a distant cousin of Liston's.

In 1820 he obtained the appointment of Medical Superintendent of the Fever Hospital,—an appointment entailing much personal risk, as Mr Syme soon discovered; for he caught the infection, and nearly died.

In 1821 he became one of the dressers in the Edinburgh Royal Infirmary. As such, it was his duty to carry out the instructions of the acting surgeon. In this position he showed the possession of considerable courage and self-reliance, by disobeying some instructions which his judgment condemned. The system of blood-letting was then in full operation, and every evening at a certain hour, the dressers had to bleed the patients whose names were entered in a book, and take from each the number of ounces of

blood there specified. On one occasion Syme had to take from a patient in one of his wards so much as 65 ounces, to be followed next day by other 35 ounces. Another patient was a boy, one of whose legs had a compound fracture, which gave rise to profuse suppuration. About three weeks after the injury, the boy's strength being much exhausted, Syme took it upon him to order porter and a beef-steak. Next day the acting surgeon, then one of the most largely employed medical men in Edinburgh, expressed disapproval of this regime, as he said it would feed the disease, and directed Syme to take 14 ounces of blood from the boy's arm. Syme obeyed with reluctance, and not without remonstrating. Before the end of forty-eight hours, the boy was dead.

In 1821 Syme was elected a member of the Royal College of Surgeons of London, and in 1823 a Fellow of the Edinburgh College of Surgeons. About the same time he went abroad to Germany and France, visiting different hospitals, and forming useful acquaintances. He also entered into a sort of partnership with Mr Liston, and occasionally took Liston's place in the lecture-room. This partnership, however, did not continue long. A quarrel occurred, which caused an estrangement of many years' duration.

But Syme, notwithstanding that he thereby lost an advantageous position, was not discouraged. He entered into another partnership with Dr Macintosh (who then lectured on midwifery and the practice of medicine), for the purpose of establishing a new medical school, with an anatomical theatre, dissecting-rooms, and museums,—he himself intending to lecture on anatomy and surgery. The very boldness of the undertaking arrested public attention. The school, however, failed; but Syme himself, fortunately by zeal, talent, and complete knowledge of his subject, coupled with an indication of views which were innovations on established practice, soon attracted a large number of students. His chief difficulty arose from the scarcity of subjects for dissection, except by dealing with the "Resurrection-men," as they were profanely called,—a course which Syme detested. In order to pursue his anatomical researches, he took advantage of the holidays to go over to Dublin. When there, he made acquaintance with several eminent surgeons, and was so delighted with their modes of operation—which he thought superior to those of Edinburgh—

that he resolved to abandon anatomy, and confine his teachings to surgery.

In 1829 he had as many as 250 pupils attending his surgical lectures, a success the more remarkable, considering that Liston, Lizars, and Turner, were rival lecturers. This well-attended class he kept up for several years.

Syme had been most anxious to get on the surgical staff of the Royal Infirmary. But Liston was one of the surgeons; and the managers knowing the animosity which existed between him and Mr Syme, felt that by admitting both into their institution, there would be every probability of dispeace. They refused Syme's application. He therefore resolved to set up a rival institution, and took Minto House, with 15 rooms in it. These he converted into wards. He also formed an out-patient department. This was a still bolder exploit than any before ventured on, but it was rewarded with complete success. On the very first day that the new hospital was opened several patients sought admission, and in the next two days as many as ten young medical men applied for the house surgeoncy, though £100 was required as a fee. The report for the first year tells of 265 in-door cases, 1900 out-door cases, and 95 operations. For four years this new institution was carried on, with unvarying success, vieing with the old established Royal Infirmary in the number and importance of its operations, and presenting a striking proof of what could be done by one young man, not only unsupported by local influence, but overcoming local and social influence arrayed against him, by dint of indomitable zeal, natural talents, and great professional knowledge.

Syme's seminary for instruction in Clinical Surgery, was recognised by the College of Surgeons in London, as qualified to give instruction for medical students. The Edinburgh College of Surgeons refused to recognise the new hospital, but agreed to recognise a course of lectures on Clinical Surgery, if Syme chose to give them, on the condition, however, that the pupils attending these lectures did not exceed 40 in number, and that they paid the same fees as were received by Mr Russell, the Professor of Clinical Surgery in the University. To these terms Syme acceded; and by his admirable lectures soon laid the foundation of subsequent brilliant reputation as a clinical teacher.

It was during this period, when he was an extra-academical lecturer, that he published two books, one "A Treatise on Excision of Diseased Joints;" the other "The Principles of Surgery." These books, which embraced numerous cases of successful operations by the author,—many of them indicating new and improved practices, extended Syme's fame over Europe, and paved the way for another distinction. This was his appointment to the Chair of Clinical Surgery in the University of Edinburgh, which Mr Russell (now in his 83d year) resigned. It was obtained in spite of the opposition of his former master and jealous rival, Liston, who wished it for himself, but would not accede to the conditions required by the Patron, the Crown, that Mr Russell should have from his successor £300 a year of retiring pension. Mr Liston had, up to this time, succeeded in shutting Syme out from access to the Infirmary. That exclusion, however, the managers saw could scarcely be continued after Syme had become Clinical Professor in the University. It was a fortunate event for both parties that, about this time, an invitation came to Liston to remove to London to become Professor of Clinical Surgery in University College, an invitation which he gladly accepted. Shortly after this event Liston wrote to Syme expressing a wish to be reconciled—a wish to which the latter readily acceded.

Liston died in 1847, and Syme was then invited to succeed him as Clinical Professor in University College, London. Syme felt flattered by the proposal, and was pleased at the prospect of going to a capital where private practice would be far greater and more remunerative. He was, however, exchanging a certainty for an uncertainty. He had L.700 a-year from his class in Edinburgh, and full employment as consulting surgeon, whereas all that was offered to be ensured to him in London was a fixed salary of L.150 independently of class fees. Nevertheless he resolved on throwing up his position in Edinburgh, where he commanded both respect and emoluments, and in February 1848 repaired to London. He soon found that he had taken a wrong step. His class was less numerous, and though his practice might eventually become great, he felt that it would be long before that pecuniary advantage was arrived at, and perhaps still longer before he could attain the social position which he held in Edinburgh. His manner was also rather

reserved for acceptance in London society. Hence, though he was making rapid progress in surgical practice, he soon began to wish he had never left Scotland. It was when in this mood that he received a request from the council of the London University to deliver lectures on systematic as well as on clinical surgery. Thereupon he at once sent in his resignation. In fact, before leaving Edinburgh he had stipulated that he should be exempted from this additional duty. The month of July 1848 found him back again in Edinburgh, after only a four months' stay in London, during which time, however, he had succeeded in acquiring the entire confidence and esteem of the medical students; insomuch that, when they heard of his intention to leave them, a committee of their number waited upon him, beseeching him to remain, and saying that an address was about to be presented, signed by every individual student. But he declined the entreaty, flattering though it was. He felt he had made a mistake when he left Edinburgh, and he was resolved to correct it before it was too late. Fortunately for Syme, the Chair of Clinical Surgery in the Edinburgh University, vacated by his going to London, had not been filled up. He was again appointed to it, and his return to the scene of his former success was greeted by general acclamation alike from students and old friends.

In subsequent years Professor Syme, besides teaching his class and attending the Infirmary, took part in the proceedings of various medical and scientific societies. He became President of the Edinburgh Medico-Chirurgical Society in 1848. He had previously become a Fellow of our own Society, and communicated to it a very important discovery, that the formation of bone is due to the Periosteum—a discovery which was the subject of a paper published in our Transactions. The importance of this discovery is great, as it often renders amputation of a limb unnecessary. in the case of diseased bones, if the disease be not in the periosteum.

At a later period, Mr Syme's active mind led him to pay attention to subjects of more general interest connected with the medical profession. In the year 1854 he took up the question of medical reform, and addressed a letter to Lord Palmerston and Lord Elcho, recommending the appointment of a General Council to

pass regulations for the granting of medical licenses in the United Kingdom. He continued for several years to take part in the public discussion of this question. His views were very generally approved of, and, I believe, formed the basis of much of the Legislation which has since taken place.

Another subject of much local interest in Edinburgh, which engaged Professor Syme's attention, was the best site for a new Infirmary. At first he advocated the old site; but, on farther consideration, he confessed he was in error, and ultimately energetically assisted those who wished the new hospital to be built in the suburbs of the town, where purer air for the patients would be secured.

During the winter of 1868–9 Mr Syme's health was not what it had been. He was less able for the fatigues of lecturing. He was also much harassed by the frequent meetings he had to attend about the new Infirmary, and he was greatly annoyed and irritated by a disagreeable professional controversy in which he was involved. The spring of 1869 also brought heavy domestic affliction. On the 6th April, after performing an operation in the Infirmary, he had a bad attack of paralysis, which, however, left his mind unclouded. He so far recovered that he was able once or twice to walk from his villa of Millbank to see patients in his consulting rooms in Edinburgh, and even to give advice in the Infirmary as a consulting surgeon. He resigned his chair in July 1869. In the spring of 1870 he still continued to see patients, but another worse attack of paralysis occurred in May, and he died on the 26th of June. He was interred in St John's Episcopal Church, of which he had long been a member, followed to the grave by very many of his old friends and pupils.

I will of course not attempt any account of the services rendered by Professor Syme to the special branch of the medical art to which he attached himself. All authorities concur in saying that, in virtue of the many important discoveries made by him, his skill as an operator, his diagnostic sagacity, and his accurate teaching, he was the greatest surgeon of his time. His services were twofold. He abolished, or assisted to abolish, many bad practices in surgery, and he was the means of introducing many new practices which have been generally adopted. Among this

last class may be mentioned his diminishing the frequency of *amputations*, and substituting *excision* instead, whereby many a person now retains an arm or a leg, which surgeons previously had been in the habit of cutting off. The like good effect followed from his discovery, that the formation of bone was due to the periosteum. His treatment of aneurisms was very successful. He had an almost instinctive faculty in discerning the true character of tumours, of which one example, not generally known, may be mentioned. A Scotch nobleman was suffering from polypus in the nose. He had consulted the most eminent surgeons in Paris and London. In both of these capitals he received the same opinion, that the tumour being of the malignant type, it could not be extracted with any probability of saving life. Some of this nobleman's friends suggested a visit to Edinburgh, to obtain Professor Syme's opinion. He accordingly came here, and a consultation took place. Mr Syme thought the tumour not malignant, and he gave an opinion that it might be radically extirpated. The operation was performed, and with complete success. The nobleman alluded to is now alive, and in good health.

Syme's manner was reserved and sometimes abrupt to his patients, of which the following anecdote, related to me the other day by a medical friend, is an illustration. A landed proprietor in Northumberland had been thrown out of his dog-cart, and was so severely bruised that he feared his shoulder had been dislocated. His medical attendant had a doubt about it. He therefore resolved to go at once to Edinburgh that Syme might see it. At the hour appointed he called on Syme, and was shown into a room where the Professor was standing before the fire. As the gentleman advanced, Syme bowed stiffly, but did not speak. The gentleman, who was lame from gout,—as he hobbled into the room, by way of beginning conversation, intimated that he was very gouty, on which Syme said, "If that 's all that 's the matter with you, you need not come to me; I don't cure gout." The gentleman next said, "But I think my shoulder is dislocated, and I want you to examine it, if you will help me off with my coat." Syme replied, "I need do nothing of the kind;—your shoulder is not dislocated. Take my word for that. I don't need to see it." The decided tone in which Syme spoke, so impressed the old gentleman that

he obeyed, and bid Mr Syme good morning, but not before giving him a double fee for his welcome opinion. He told his medical man, when he returned home, that he thought Mr Syme the most self-possessed man he had met with, and would assuredly go back to him if he ever had again to consult a surgeon.

Syme was remarkable not only for self-possession, but for the more noble qualities of professional sincerity and honesty. When he found himself in the wrong, he never hesitated to alter his course, nor was he ashamed to confess it. When the late Sir David Baird of Newbyth was severely hurt by a kick from a horse in Berwickshire, Dr Turnbull of Coldstream, who attended him, becoming somewhat anxious, brought Mr Syme out to see him. Mr Syme, after inspecting the broken leg, and considering the case, gave a decided opinion that there was no reasonable ground of apprehension, and returned to Edinburgh the same day. But that night Sir David Baird became restless and feverish, and Dr Turnbull, notwithstanding Syme's opinion, on the following morning thought of again sending for Syme. Early that forenoon he was surprised to see a carriage drive up to the door, and to find that Syme was in it. Dr Turnbull expressed his happiness at seeing him so soon again, but asked what had brought him back; on which Syme said, "I never closed my eyes last night, because I began to fear I had given you a wrong opinion, and I have come back to see your patient again." Syme, after another examination, satisfied himself that there was too good reason for anxiety, and intimated that he thought Sir David Baird would not recover. He died two days afterwards.

Syme, though he published very many papers in the medical journals, was not a voluminous writer. As in his operations he got through his work quickly, never drawing from his patient an unnecessary drop of blood, so in his publications he wrote concisely, and seldom wasted a drop of ink on illustration. His most important work, "The Principles of Surgery," went through five editions, the last edition being in bulk smaller than any of its predecessors. His aim, both in his books and in his lectures, seemed always to be, to give a maximum of instruction in a minimum of words.

Syme was proud of his profession, and proud of his own posi-

tion at the head of it. Perhaps it was from this cause that he was charged with unwillingness to admit and adopt the improvements suggested by others in surgical practice. On the other hand, he was quite indifferent about pressing his claims to any honorary distinction. Nevertheless, from various public bodies, he did receive, unasked for, acknowledgments of his merit; as when there was conferred the M.D. degree from the Universities of Dublin and of Bonn, the D.C.L. degree from Oxford, and the Knighthood of the Dannebrog from the King of Denmark, an honour rarely granted to a foreigner. On a General Medical Council for the United Kingdom being appointed, he was chosen a member of it, to represent the Universities of Edinburgh and Aberdeen. For ten years he took a lively interest in its proceedings, and his opinion was always listened to with respect. It was probable that Syme would have been elected President of the General Medical Council on the retirement of Dr Burrows in 1869, but Mr Syme about this time became unwell, and his friends saw he would be unable to fulfil the duties of the office.

After Syme resigned his professorship in July 1869, a movement among his professional brethren, who knew his merits as a surgeon, was commenced, for the purpose of raising a testimonial which might keep his name before future generations. It was all the more striking and gratifying that this movement commenced in London, and was warmly supported in America, because indicating the judgment of those who could estimate his services free from the influence of local feelings. The testimonial will embrace a scholarship to bear Syme's name of L.100 a year for students of surgery in Edinburgh University, and a marble bust of Mr Syme for the great hall of the library. The funds for the testimonial have been nearly all subscribed. Should there be any deficiency, I understand it will be made up by the University Endowment Association.

Besides testimonies from abroad to his professional services, several from his countrymen in Scotland, of a very gratifying kind, were not wanting. From many provincial associations of medical men, there came addresses expressing regret that he should have found it necessary to resign his professorship, and conveying to him the respect and gratitude of those who had benefited by

his advice, teaching, and example. One of those addresses, from the Border Medical Association, dated at Kelso, on the 18th August 1869, runs as follows:—

"At the twenty-third annual meeting of the Border Medical Association, we, the undersigned members, unanimously resolved to ask you to receive from us a short address on the occasion of your resignation of the Professorship of Clinical Surgery in the University of Edinburgh.

"We desire to convey to you our warmest thanks for the very kind manner in which you have at all times discharged your duties towards our patients and ourselves. We beg also to thank you sincerely for innumerable acts of personal kindness and attention, for which we shall ever feel grateful. Although the members of our profession generally have resolved to offer you some testimonial in recognition of your inestimable services, and although you have already received a most hearty expression of sympathy and regard from the profession practising in far distant lands, we trust that it will not be otherwise than agreeable to you to know that the medical and surgical practitioners in your own Border-land are equally sensible of and grateful for the great advantages they have derived from your precepts and example. It was with unmingled feelings of sorrow and regret that we heard of your illness, and we now most heartily rejoice to know that you have so far recovered as to be able, in some degree, to resume those professional duties which we have all learned to value so highly. We desire to express the earnest hope that you may yet be long spared to give us the benefit of that eminent wisdom, vast knowledge, and matchless diagnostic tact and skill which have rendered your name famous wherever the science and art of surgery are known. It is to us a source of pleasure that, on the very day of our assembling here, it has become known that you are to be succeeded in your chair by your son-in-law, Mr Lister, believing as we do that his appointment will be peculiarly gratifying to yourself, in the highest degree acceptable to the profession at home and abroad, and highly calculated to maintain the celebrity of the Edinburgh surgical school, in which you have so long been the distinguished master."

If there was any taste or pursuit beyond that of his own special profession for which Mr Syme had a predilection, it was gardening. He long cultivated with great success the rarest plants of distant temperate and tropical countries, and annually carried off the highest prizes at the exhibitions of the Horticultural Society of Scotland. He was equally successful with tropical fruits, among others the banana, which he was one of the first in this country to ripen in perfection. In his later years, at his villa of Millbank, he formed a large collection of Orchids. Among these he spent much of his leisure hours. To his friends and former pupils, when they came to see him, he was ever ready to show kindness and hospitality; and the friendships which he made were lasting, warm-hearted, and disinterested.

Perhaps the leading qualities of Syme's character, and which ensured his success in life, were clearness of perception, fearless honesty of purpose, and strength of will. He was always able to see clearly the point at which to aim, and by steadiness both of eye and hand, to reach it, in spite of obstacles and difficulties which would have made most other men flinch. Self-reliance was *his* chief stepping-stone to fame,—the honourable fame of having greatly advanced the science which tends to save life and limb, and also to assuage human suffering.

III. I come now to the third head, which *is to offer a few suggestions for increasing the efficiency of our Society.*

Under this head there are two points which demand attention.

1*st.* Can our present arrangements be improved?

2*d.* Are there any drawbacks which can be counteracted?

(1.) In regard to our present arrangements for carrying on the Society's business, the most important is undoubtedly the publication of papers in our Proceedings and Transactions. Its importance cannot well be over-estimated. Probably but for this mode of recording discoveries, speculations, and inventions, and also of publishing them, half of these would never have become known to the world. It is no disparagement to the papers which appear in our Proceedings and Transactions to say of them, that to only one person out of a thousand are they of any interest, and therefore that they would not be read, and would not pay to be published by the authors at their own expense. But next to the pleasure of effecting discovery, is that of making known the discovery to others. This last pleasure can therefore in many cases be obtained only through means of societies like ours. But there is another and a separate good done: not only are investigators stimulated, but when the results of their investigations become widely known, these often suggest new views to other inquirers, who make use of these published results as stepping-stones for overcoming some difficulty which had obstructed their own inquiries. In that way, also, men of science and literature in different countries become acquainted, so as to aid one another in their respective labours.

I have surely said enough to show how useful these publications

are, and it is no small proof of this when we find, as I have already stated, that our Transactions are almost every year becoming more bulky.

The only practical suggestion which it occurs to me to offer under this head is, that means should be taken to ensure early publication. I am sorry to find that the volume containing last year's papers has not yet been published, though the Society's law expressly states that "the Transactions shall be *published at the close* of each Session."

(2.) Another part of our proceedings to which I respectfully invite attention is the best mode of conducting our evening meetings. What is the object and use of these meetings? From a paper published in the first volume of our Transactions, entitled, "*History of the Society*," drawn up, I believe, by the first secretary, Dr Robison, it is stated that these meetings were held in order that—

"Essays and observations of members or their correspondents may be read publicly, and become the *subjects* of *conversation*. The author is likewise to furnish an abstract of his dissertation, to be read at the next meeting, when the *conversation* is renewed with increased advantage.

"Several papers have been communicated with the sole view of furnishing an occasional *entertainment* to members, which do not afterwards appear in the Transactions. Essays and cases are often read at the meetings in order to obtain *the opinions* of members on interesting or intricate subjects. Some papers intended for future publication have been withdrawn for the present by their authors, in order to profit by what has occurred in the *conversations* which the reading of the papers has suggested."

The original intention, therefore, of our evening meetings was to encourage discussion among the members on the papers read, and this object we have ever since kept in view, though on account of the length and number of the papers put down to be read in one evening, there has often been no time for any discussion of them.

I suppose it had been with the view of remedying this inconvenience that in October 1836 the Council of the Society made a remit to the three secretaries—

"To report as to the possibility of economising time by some change in the present order of the business of the general meetings, and by inducing the authors of papers to give (when necessary) condensed abstracts of them, leaving the details for being printed when their publication in the Transactions may be determined on."

The three secretaries accordingly, in December 1836, reported how this object might be brought about, viz., that

"The members of Council to whom papers are referred for preliminary examination shall, after perusal, *advise with the authors* in what manner they may be *shortened* in reading them to the Society. The secretaries farther submit, that some course of this kind is imperiously called for, by the increasing number and value of the communications presented to the Society. They farther express their conviction, that the change in question, if acted on by authors, will add greatly to the spirit of the Society's meetings, and to the interest of the members in its proceedings."

They add in their report, "That the public business, if time enough be left, should be concluded with verbal communications of scientific news."

This report was adopted and approved of by the Council, and ordered to be printed, so that I have no doubt it was communicated to the Society generally, and attempted to be carried out.

In now therefore bespeaking renewed attention to this subject, I only desire to urge what seems to have been alike intended by the founders of the Society, and aimed at by those who have preceded us in the Society's management.

The advantages of a good attendance of members at our meetings, and also of a discussion of the papers read at them, are obvious. It is for the credit of the Society, that its members should take an interest in its objects, and show that interest by attending its meetings. It is an encouragement to literary and scientific authors to bring forward papers, when they know that these will be read, not to dead benches, but to living associates, and to associates who will listen, and some of whom will state, after hearing the papers, whether they appreciate the views contained in them. It is also an advantage to members to have an opportunity of meeting one another, for the purpose of cultivating friendly intercourse, and obtaining information.

In the Geological Society of London—the only Society there, whose meetings I have had an opportunity of attending—special means are taken to induce a good attendance, and also to induce verbal *discussion* at evening meetings. As papers are more intelligible and attractive when illustrated by diagrams, authors of papers are encouraged to exhibit diagrams whenever that is possible, the Society paying the cost of them, subject to certain

checks. Discussion almost invariably takes place; though whether any previous arrangement to ensure this is made, I cannot tell. After the public business is over, there is an adjournment to an adjoining apartment for refreshments; in which apartment there are comfortable chairs and sofas, where members and their friends can chat together if they like. There is also at these meetings a greater variety of refreshments than we provide.

I trust I may be excused for referring to these common-place details, but I attach so much importance to a good attendance at our evening meetings, that I would desire to leave no means untried to secure it.

What are the means which, for this purpose, I suggest?

1st, I think that papers of so abstruse a nature as not to be intelligible to three-fourths of the members, ought not to be read, nor even an abstract of them,—but only a verbal account given of the nature of the paper, and its bearings.

2d, No paper, however intelligible, should be read *verbatim*, unless it occupy only a few minutes, say fifteen or twenty, but only an abstract of it shall be read or verbally stated.

3d, The members of Council to whom the paper has been referred to report on its fitness for the Society should be prepared, after the author has read his paper or stated its substance, to give their opinion of the merits of the paper, the President for the night also adding a few remarks.

4th, Diagrams, where possible, ought to be exhibited, one-half of the cost of which should be paid from the Society's funds, subject to the check of a committee.

5th, It shall be competent for a Fellow at the commencement of business, with the leave of the Secretary and President for the night, to exhibit any article or object, organic or inorganic, or any instrument of scientific interest recently discovered or invented, and give a short verbal explanation, it being understood that such verbal explanations shall be concluded before 8.15 P.M., so that the written papers announced in the billet may then be proceeded with.

6th, There ought to be in the retiring-room something better provided, in the way of refreshment, than a cup of tea, as also chairs or sofas for the convenience of those who attend the meetings.

2. The next point to which I advert is the existence of certain

drawbacks to the efficiency and influence of our Society, and the possibility of counteracting these.

When our Royal Society was established, now nearly ninety years ago, no other society devoted to literature or to science existed in Edinburgh. It was therefore natural and right that the Society should embrace, among its objects, all the departments of knowledge which were then known, or were beginning to be cultivated.

The rapid extension of different sciences soon rendered it impossible for one society to give due attention to all these, or to assist investigators in each, to the full extent that they desired.

Hence separate societies came to be formed, devoted to particular sciences; and these societies were naturally joined by many persons who, but for them, would have probably become members of our Royal Society.

What has been the consequence? We have in Edinburgh, and our other large towns, very many institutions, both literary and scientific, which are strong in membership; and even in our provinces, we have societies and clubs, devoted to botany, geology, zoology, and archæology, some of which also possess a large staff of members.

Let me enumerate the membership of some of the Edinburgh societies :—

The Medico-Chirurgical Society, instituted 1821, has about	300	Members.	
The Philosophical Institution, about . . .	2000	„	
The Geological Society, instituted in 1834, has	180	Ordinary	Members.
The Royal Physical Society, ...	250	„	„
The Botanical Society, instituted 1836, . .	360	„	„
The Arboricultural Society, ...	500	„	„
The Society of Antiquaries, ...	300	„	„
The Royal Society of Arts, instituted 1821, has	420	„	„
The Meteorological Society, instituted 1856, .	600	„	„

With regard to provincial societies, I may mention that Sir Walter Elliot* of Wolfelee has lately been making out a list of Natural History Societies and Field Clubs, existing not

* The list here referred to will be found in an address delivered by Sir Walter Elliot to the Botanical Society of Edinburgh on 10th November 1870; and is to be printed in that Society's Transactions for 1870–71.

only in Scotland, but in England and Ireland. This list will be exceedingly instructive, as I understand it specifies the objects of each Society or Club, the nature of its operations, and the district of country with which it is connected. He has had the kindness to send to me an account of twelve of these provincial societies, the most northern being in Orkney and Shetland, the most southern in Berwickshire, Dumfries, and Galloway. About one-half of these societies publish proceedings or reports in some form or other, for circulation among their own members. To one of these last-mentioned provincial societies, connected with the Eastern Borders of England and Scotland, "The Berwickshire Naturalists' Club," Sir Walter Elliot and I belong. It has a membership of 250 persons, and has published six octavo volumes of reports on topics—Botanical, Geological, Zoological, Entomological, and Archæological.

Though it is chiefly the Edinburgh societies which keep members from our Royal Society Roll, and papers from our Transactions, there can be no doubt that the societies of other towns, and of the provinces, act more or less in the same direction. But in saying this of any of these separate societies, I mean no disparagement of them; nor, in spite of their interference with our usefulness and influence, do I regret their multiplication. On the principle of the division of labour, the more societies the better, for the sake of the stimulus they give to scientific investigations. The late Principal Forbes, in his address from this chair in the year 1862, in alluding to the effect which these societies had on us, thought that they "fostered (to use his own words) a *spirit of rivalry* towards the larger, more national, and more permanent Institution, which the Royal Society of Edinburgh might fairly claim to be." I have never seen indications of a spirit of rivalry, in the sense of hostility. All the length I can go is to admit—as, indeed, I affirm—that the existence of so many separate scientific societies in Scotland has the effect of curtailing our membership and our operations, and that this effect will increase unless means be devised to counteract it.

I think such means may be devised, and with advantage, not only to our own and other societies, but to the cause of science. There are many researches and inquiries which can be pro-

secuted successfully only by the co-operation of many persons acting together, or acting in different districts. Opportunity for such co-operation might be afforded by separate societies. Thus the Committee of the British Association on Luminous Meteors lately applied to the Scottish Meteorological Society to have a certain number of their observers, situated in different parts of the country, told off to watch on particular nights the occurrence of meteors, and mark down on maps furnished to them their positions, the direction of their movements, and other particulars. That is an example of two independent scientific bodies co-operating together. What I next mention shows the co-operation of six or eight societies. In Switzerland, and in the South of France, the various Natural History and Physical Societies have been carrying on a joint investigation to record the exact position of the most remarkable "boulders" in the districts with which they are severally connected. For this purpose one central society—the Helvetic Society—has issued to the societies at Neufchatel, Berne, Aargau, Geneva, Lyons, and Grenoble, suitable maps and schedules. These societies have already made great advances in ascertaining and marking down the exact position of numerous boulders above 100 tons in weight. They have done more, for they have succeeded in stopping the wholesale destruction of boulders, which were being victimised to agricultural improvements; and so much have their objects been appreciated by the municipal and State authorities, that the latter pay the cost of the necessary printing, and other expenses required for the investigation.*

Another case of co-operation nearer home may be mentioned. Professor Roscoe of Manchester is forming what he calls a "National Science Union," embracing not only scientific investigations, but also, and even more especially, action on the Legislature and the Government. With reference to this last object, he observes, that "although those who are engaged in scientific investigation or instruction, undoubtedly form one of the most intelligent professions in the kingdom; yet, for want of union,

* Professor Faure of Geneva has had the kindness to send to me several of the Maps, Schedules, and Reports, showing the progress made by the different societies aiding in this investigation.

they have no commensurate influence on the Legislature. The interests of commerce, manufactures, agriculture, railways, and the clerical, legal, naval, and military professions are represented by strong parties in Parliament, yet there are very few members of either House who can be said to represent the high interests of science. It is therefore urged that no time should be lost in creating an organisation, which will enable those interested in the progress of science to use their proper influence, and when the time arrives, to press their legitimate claims upon the Legislature." A programme has been widely circulated for the purpose of ascertaining how far the proposals contained in it meet with the support of men cultivating all branches of science, and living in all parts of the country. Professor Roscoe adds, that "the present moment appears to be well suited for action in this matter, as the establishment of a union amongst men of science must strengthen the hands of the Royal Commission now considering the whole subject of State aid to science."

The movement thus commenced, and going on in various quarters for co-operation and confederation, deserves our consideration. We see the important purposes which may be thereby attained, not only by facilitating important physical investigations, but also by giving to scientific bodies a greater power and influence in the country to which they are well entitled.

If it be asked how co-operation and confederation can best be secured, I may perhaps be told that it will be enough to trust to sympathy with each other, created by the pursuit of common objects, and that no special or formal alliance is necessary. As among all the branches of human knowledge relationship prevails, so it is said there is naturally and unavoidably a similar connection among societies. But the well-known Roman aphorism which speaks of this relationship, speaks also of a bond to cement it, "Omnes artes quae ad humanitatem pertinent, habent commune vinculum, et quasi cognatione quadem inter se continentur." The "commune vinculum" here referred to, is, I think, desirable; and that bond may fitly be constituted by a central society, which, embracing in its own programme of operations various sciences, holds out a hand of welcome and co-operation to other societies, severally devoted to some one of these sciences. The

late Principal Forbes strongly maintained the expediency of a central society on a separate ground, which is explained in the following paragraphs of his address. He urged that—

"To maintain the character for energy and stability of one central society, is in reality the common interest of all who cultivate science. Delightful and instructive meetings may be held by a local body of geologists, or chemists, or naturalists. But such local associations require immense vitality to be permanent. Generally they fall into abeyance in twenty or thirty years; and if they attempt to record their labours by publications, these publications having never attained more than a very limited circulation, become inaccessible and forgotten. The matured written reports of these labours in minor societies, are best consigned for preservation to the publications of a central and enduring association."

All these views evidently point to our own Society, as being one well qualified to undertake the duties and position of a central body in order to promote co-operation and confederation among the various scientific bodies in Scotland; and if it be objected that my views could not be carried out without some considerable change in our established customs, I have only to say, that as in Governments, it is wise to make from time to time such reforms as are called for, in order to retain public confidence, or promote more efficient action; so in other institutions, it is equally expedient to watch the progress of events, which may necessitate from time to time some changes in their modes of operation.

The changes, however, which would benefit both our own Society and others, are really not so important, as that the Council of its own authority may not competently adopt them. They are as follows:—

(1*st*.) That should any society in Scotland having literary or scientific objects, desire to be connected with the Royal Society of Edinburgh, it shall, if our Council approves, be held to be affiliated with us, and to be entitled to the privileges of an affiliated society.

(2*d*.) That any member of an affiliated society, on intimating to our secretary his name and address, shall receive a billet, entitling him to free access to our meetings, as well as to our library and reading-room.

(3*d*.) That an affiliated society shall have right to send to us, through its office-bearers, reports or papers by any of its members, on literary or scientific subjects, which if approved by the Council,

may be read at our evening meetings, and may be published in our Transactions.

(*4th.*) That our Council, on the other hand, shall be entitled to appeal to any affiliated society for co-operation in the ascertainment of facts, or the investigation of phenomena, lying within the compass of its objects, and also within the field of its operations; and if, in response to this appeal, a report is made, we may, if approved by the Council, have it read or noticed at our meetings, and published in our Transactions.

(*5th.*) That in the event of any important investigations or experiments being wished to be made by the members of an affiliated society, which however cannot be made by them on account of the expense thereof, it shall be competent for the office-bearers of such affiliated society to apply to the Council of our Society to defray a portion of the expense, out of the funds of our Society, or out of an annual grant, should such be obtained from Government, to aid scientific investigations in Scotland.

Some such arrangements as those I have now suggested, would probably produce co-operation among most of the societies in Scotland devoted to science or literature, a co-operation which would be attended by advantages, both to them and to the advancement of their objects.

IV. In adverting, under the next head of this address, *to the usefulness of such societies as ours*, it is only necessary to observe that they have been established to aid philosophers in the peculiar work to which they devote themselves. Whether we regard the work they accomplish, or the motives which inspire them, these philosophers deserve all the encouragement and aid which can be given. They love knowledge for its own sake;—their chief pleasure consists in searching for knowledge;—and their highest happiness is to discover some new truth. Fortunately for the world, there have been in all ages, and among almost every people, individuals who have cherished those noble aspirations. The old Hebrew king has recorded, how he "applied his heart to know and seek out the reasons of things," and avouched from experience, how "Happy is the man who findeth wisdom." The enlightened Roman expressed the same sentiment when he exclaimed, "Felix

qui potuit rerum cognoscere causas." The Greek mathematician, on discovering that the square of the hypothenuse in a right-angled triangle is equal to the sum of the squares of the other two sides, in testimony of his happiness offered a hecatomb to the gods; whilst a Sicilian philosopher, when he found how to ascertain the specific gravity of bodies, was so overjoyed, that he rushed out of his bath naked into the streets, mad with delight. Our own Sir Isaac Newton became so elated or agitated when approaching the end of his calculations, which he saw would prove that the planetary movements were all governed by the law of gravitation,—that law which he was the first to discover,—that he was obliged to hand over his calculations to a friend to complete them. These men, and thousands more of the same stamp, were all animated by a heaven-born instinct to pry into the mysteries of nature, to study the mechanism of the universe, and deduce the rules or principles which the Almighty had followed in the work of creation, and still follows in the equally great work of upholding the universe. Their tastes were noble, because pure; their researches and labours also were noble, because disinterested. They worked not for their own individual benefit, nor even for that of their own kin or country, but for that of the human race. Men characterised by such tastes, such motives, and such pursuits, surely deserve encouragement, and if scientific societies afford it—their usefulness is unquestionable.

How these societies afford this encouragement I have already partly explained, when adverting to our own operations, and in particular to the stimulus given to men of science, when by means of our meetings, and our Transactions, they obtain an opportunity of intimating their discoveries and publishing them. It is probable that there are thousands of discoveries—the groundwork of important inventions,—which never would have become known,—nay, which never would have been made, but for the existence of such societies as ours. For example, the Principia of Newton would not have been given to the world at the time they were given, had the Royal Society of London not agreed to print them; for Newton was so poor, that he could not afford to continue his subscription as a member of the Society, small as that was.

Whilst philosophers are encouraged by these societies to investigate, by knowing that their discoveries will be recorded and published by the societies of which they are members, others who may or may not be members, when they see these discoveries and study their bearings, are often able to turn them to account, and in a way never anticipated by the authors. Hundreds of cases can be stated, where papers published in scientific transactions, on being perused and studied by other inquirers often in a distant part of the world, have been to them as bridges, enabling them to pass over difficulties which previously had obstructed progress, and on the brink of which they had been sitting in despair.

That scientific societies contribute immensely to the advancement of knowledge, may be farther inferred from this circumstance, that as it is during the last fifty years that discoveries and inventions have been more plentiful than in any former age, so it is during the last fifty years that these societies have multiplied, and a wide circulation given to their published transactions.

To these societies mainly, mankind is therefore indebted for the marvellous contrivances and processes which distinguish the present age above all that have preceded it. Most of these—such as electro-magnetism, electro-plating, photography, artificial light, improved telescopes and microscopes, steam machinery, anæsthetical agents and medical disinfectants—sprung out of experiments, observations, or speculations, were very unpromising as regarded any practical utility when first announced, but ultimately became sources of incalculable material wealth, as well as of vastly increased comfort and enjoyment to man.

These triumphs of modern science, are also the chief elements of our present civilisation, and for them the world is indebted chiefly to scientific bodies such as ours.

V. In adverting to the last head of this address, viz.:—on *the best way of encouraging and aiding such societies as ours*, I have to remark that it may be effected in two ways, viz,—*directly*, by grants and accommodations from the State; and *indirectly*, by creating among all classes of the population a greater taste for scientific pursuits.

1. Taking the indirect method first, it is hardly necessary to

point out how, as this scientific taste increases, persons will be more inclined to join societies of a scientific nature. The practical question then arises how this taste can be increased?

At a former period I had the faith which many others had in the efficacy of mechanics' institutes. But having had some experience of the working of these institutions, I am now satisfied that popular lectures do very little else than afford amusement,—though in that respect they are not altogether useless. But if they are to give instruction, and promote habits of observation, or a taste for scientific pursuits, they must inculcate and administer the hard discipline of personal study. Accordingly, many mechanics' institutes have established classes for different branches of study, and with much advantage.

I confess, however, that I have more faith in the instruction which begins at an earlier period of life than can be conveniently given at mechanics' institutes. I have seen that boys even under fourteen or fifteen years of age may acquire a taste for scientific pursuits, and habits of accurate observation—very serviceable, in whatever field of useful industry they may afterwards engage. No interference with essential branches of study would be necessary. In our Scottish parish schools, the time now spent in teaching French and German* to the children of the working classes, would perhaps be more usefully spent in teaching the elements of physiology, botany, chemistry, or geology; and as it is now the general practice in all primary schools to have an entire holiday on Saturday, that day of idleness or mischief would be more beneficially spent in a walk along the sea coast, or up a hill side, or through a rocky dell, or even along hedges and ditches, accompanied by a master competent to point out objects of interest. Who can doubt that in the course of such rambles, aided by a small amount of indoor instruction, seed would be sown in many a boy's mind and disposition, which would bear good fruit of a scientific kind in after years. I am glad to be able to say, that I know of several parish schools in East Lothian and in Perthshire, where the masters, having themselves a turn for science, have a class for instruction in the particular branch with which they are conversant. In one school,

* I see from this year's Education Report, that in the parochial schools, the number learning these languages is 2500.

chemical experiments are made once or twice in the month. In another school, the teacher has a telescope, through which he shows to the older boys of his school the moon and larger planets. In another school, a small collection of specimens has been formed to illustrate the rocks and minerals of the neighbourhood. The chief drawback in this matter, next to the want of teachers competent and well-disposed, has been the want of suitable text-books. But I am glad to find from the Secretary of the Education Committee, that this last drawback is being removed, as he has himself been preparing Elementary Science School Books, with the assistance of Professor Kelland, Professor Balfour, Mr Archer, Mr Geikie, and other eminent scientific men.

Whilst on the subject of scientific instruction in schools, I cannot avoid referring to the very gratifying encouragement given by the Government Department at South Kensington. That encouragement is very considerable, consisting not only of money rewards to pupils and teachers, but also of apparatus and books to schools. It is already producing fruit; for whilst last year, the number of schools in Scotland which received these Government grants amounted to 24, this year they are 45, being an increase of nearly 100 per cent.

Therefore, as these science and art classes in schools are multiplying, a taste for science will no doubt quickly germinate among the working and middle classes, thus supplying candidates in greater numbers for scientific pursuits and scientific societies.*

2. The foregoing remarks apply to the aids given *indirectly* to societies. I next notice the amount of aid given *directly* by the State.

Here it is proper to distinguish the aid given to science classes in schools, from the aid given to scientific societies. In the former

* Since this address was delivered, I see (*Nature*, Dec. 22, 1870) that an address has been presented by the President of the British Association for the Advancement of Science, supported by the office-bearers and an influential deputation, comprehending Sir Charles Lyell, Sir John Lubbock, Dr Lyon Playfair, and Mr Francis Galton,—to the Vice-President of the Privy Council Committee on Education, pointing out the expediency of authorising, in the new national elementary schools, systematic instruction in elementary science, so as to create a taste among the pupils, whereby they may be induced to follow out scientific studies in the more advanced schools.

case, aid is given for instruction in facts and principles which are already known. In the latter case, aid is given for searching new facts and new principles. It is very evident that the latter object is all important, if any advances in knowledge are to be made. Moreover, it is an object which needs more help from external sources. The student who obtains technical knowledge, or the knowledge which fits him for a profitable trade or profession, may not unfairly be left to pay the expense of his instruction, in consideration of the gains which that trade or profession will bring to him. With an investigator of scientific phenomena, who hopes to discover some new principle, the case is widely different. As his impelling motive is not the prospect of gain, so in nine cases out of ten the original discoverer of a new law, or a new principle, or a new product, is not the man who ever benefits by it in a pecuniary sense. Whilst he sows the seed, others reap the fruit, and yet, to procure the seed, probably much capital had to be spent and years of study endured, at the sacrifice of both health and fortune. Therefore the man who devotes his time to the discovery of new truths, and who bravely adheres to that pursuit in spite of difficulties and embarrassments, is surely a man standing in more need of help and encouragement than the engineer or artisan or mechanic who is receiving instruction which will enable him to follow a profitable profession. If the latter deserves assistance from the State, much more should the former. These investigators of science are the men of whom a country, when it possesses them, should be proud; and it would be a bad sign of the age if such men did not exist, or if no interest was felt about them. When ancient Rome was becoming degenerate, the question was significantly asked—"Quis nunc virtutem amplectitur, *præmia* si tollas?" So also it would be a sign of the degeneracy of a people, were no one to embrace science, except from the hope of profit; and it would be equally a sign of a degenerate Government, if it refused to encourage men of science and scientific societies.

In all civilised countries such encouragement is given in a greater or less degree, and in one form or another. Whether the amount of the encouragement given by the British Government is sufficient, is a point on which I at present offer no opinion.

But one thing is obvious, viz., that whatever were the difficulties which, thirty or forty years ago, investigators of new facts and new principles had to encounter, these difficulties are tenfold greater now, and therefore help to overcome these difficulties ought now to be more ample. The first discoveries in all the sciences were made by methods and processes far more simple than are now serviceable. The first steps in astronomy were made by the human eye alone. After all the knowledge was collected, which the unaided eye could supply, the next advances in the science were made by telescopes—telescopes simple and rude at first, but soon superseded by others of greater size and more accurate construction, so as to admit of a farther penetration into the depths of ethereal space, and a more minute examination of the movements and forms of the planetary bodies. When an eclipse of the sun has to be observed, the only way of now proceeding is, besides employing highly improved telescopes, to have also the spectroscope, the polariscope, and photographic apparatus; and, in order to use these instruments to the best advantage, large parties of observers must co-operate, and, at a great sacrifice of time and money, repair to favourable and probably remote spots on the earth's surface. So it is with all the other sciences. To enable a chemist to make discoveries now in his science, he must have apparatus and instruments ten times more numerous and expensive than those with which chemists formerly worked. The botanical physiologist can make no farther advances, except by means of powerful microscopes, which to his predecessor were unknown. For progress in meteorology, observations by individuals, in a few districts once or twice a day, are no longer of much avail. There must be a complete network of observations made over large portions of the earth's surface—and at least three or four times in the twenty-four hours. There must be self-recording instruments in particular districts, besides occasional ascents in a balloon. In short, there is no one science which can now be advanced by the same simple means which were available formerly. Science would stand still if improved methods were not resorted to. The difficulties, therefore, which men of science and scientific societies have to encounter in their researches are far greater than formerly, and what may have been a sufficient

amount of aid and encouragement to them twenty or thirty years ago is now manifestly quite inadequate.

Another obstacle in the way of farther discovery must not be overlooked. A great proportion of the philosophers who search after new truths and new principles are teachers, whose income as such alone enables them to obtain the means, scanty and precarious as it is, of prosecuting original investigations. But as knowledge advances, the labours of instruction increase;—and if the teacher does his duty in that capacity very little time is left to allow of extraneous investigations. Yet these persons are often better qualified to be investigators of new truths, than teachers of old truths. I have in my own experience met with professors in our universities whose occupation in the work of teaching deprived science of those who most probably would have been instrumental in making great discoveries.

The circumstances to which I have been adverting, as obstacles to the future advancement of science, were felt to be so serious, that two years ago they engaged the attention of the British Association—an association whose chief object it is "to give a stronger impulse and more systematic direction to scientific inquiry," and "to remove any disadvantages of a public kind which impede its progress." The view submitted to the Association by those who brought the subject before it was, that as there are institutions for teaching old truths, so there ought to be institutions for discovering new truths, and that, as this last work had now become so difficult and costly, that few individuals could enter on it from their own resources, the State—which, on behalf of the great interests of the country, is interested to encourage discoveries and investigations—ought to come forward and establish institutions, in which men with an aptitude for original investigations might have facilities for carrying them on, without being distracted by any other vocation.

The British Association so far entered into these views as to appoint a committee, consisting of some of its most eminent and influential members, and the two following questions were put to the committee for consideration:—

"(1.) Does there exist in the United Kingdom of Great Britain and Ireland sufficient provision for the vigorous prosecution of physical research?

"(2.) If not, what further provision is needed, and what measures should be taken to secure it?"

At the meeting of the Association in 1869 that committee reported—

"(1.) That the provision now existing in the United Kingdom of Great Britain and Ireland is far from sufficient for the vigorous prosecution of physical research.

"(2.) That, whilst greatly increased facilities for extending and systematising physical research are required, your committee do not consider it expedient that they should attempt to define how these facilities should be provided."

In explanation of this last finding, the committee observed that—

"Any scheme of scientific extension should be based on a full and accurate knowledge of the amount of aid now given to science, of the sources from which that aid is derived, and of the functions performed by individuals and institutions receiving such aid. Your committee have found it impossible, with the means and powers at their command, to acquire this knowledge. Moreover, as the whole question of the relation of the State to science, at present in a very unsettled and unsatisfactory position, is involved, they urge that a Royal Commission alone is competent to deal with the subject."

The Association approved of this report, and appointed application to be made to her Majesty's Government to appoint a Royal Commission to consider the whole subject. This application was successful; for, in May 1870, the *Gazette* announced the names of nine Commissioners, with power "to make inquiry with regard to Scientific Instruction and the Advancement of Science, and to inquire what aid thereto is derived from grants voted by Parliament, or from endowments belonging to the several Universities in Great Britain and Ireland, and the Colleges thereof, and whether such aid could be rendered in a manner more effectual for the purpose."

The importance of this measure I need not dwell upon. The backwardness of the British Government to aid institutions and individuals devoted to scientific investigations, and the miserable amount of any pittances conceded to them, affect the credit and prosperity of the country quite as much as the interests of science. Great Britain, whose influence in the world depends almost more on moral than on physical power, ought not to be behind other

nations in its patronage of science. Yet what has happened within the last six weeks? A remarkable eclipse of the sun, to take place on the 22d of this month, had been looked forward to by astronomers as affording an excellent opportunity for solving many important questions regarding the constitution of that great orb on which all living things in our planet, and in other planets also, depend; but, for the proper observation of which eclipse, expeditions were necessary, requiring much previous preparation and great expense. The United States Government, even eight months ago, began preparations, a sum of L.6400 having been unanimously voted by Congress,* and a Government officer despatched to visit Spain and Sicily, to find proper places of observation, and to make suitable arrangements for the reception of a party of astronomers. A ship of the United States navy was appointed to convey them, accompanied by two eminent engineer officers, representing the Government, to take a general charge.

In England what were the arrangements for this interesting astronomical phenomenon? Early last spring, on the suggestion of the Astronomer Royal, a committee was formed, consisting of himself and the Presidents of the Royal Astronomical Society, and of the Royal Society of London, to organise an expedition. A party of astronomers soon volunteered, about sixty in number, who were to be divided into two parties, one for Spain and another for Sicily, each subdivided into sections, to make different kinds of observations, with suitable instruments. As total obscuration would last only two minutes, the more that the work could be

* The following appropriations, under the head of Astronomy and Meteorology, were made by Congress, as given in "*Nature*," Jan. 26, 1871:—

Observations of Eclipse, Dec. 1870, under Coast Survey,	29,000	dols.
U. S. Nautical Almanac, ...	20,000	,,
National Observatory, ...	19,800	,,
New Telescope for do, ...	50,000	,,
Telegraphic Notices of Storms, . . .	50,000	,,

In the same Congress there were additional appropriations to the amount of no less than 1,377,766 dollars, for the support of Museums, Botanic Gardens, Mining Statistics, Polar Explorations, Surveys, and other objects of a scientific nature. These appropriations, be it observed, were by the Federal Government. Similar appropriations, but larger altogether in amount, are made by the different States in aid of their own societies.

distributed among different observers the better. The Committee had entertained no doubt that her Majesty's Government would give ready, if not liberal, assistance. On the last occasion of a solar eclipse—viz., in 1868—several European Governments sent expeditions to India, where it could best be viewed. Ours gave the use of a ship, besides appointing officers, and paying expenses. But when the committee, last summer, applied to the Admiralty to ascertain if one of her Majesty's ships would be allowed to convey the English astronomers, the answer they received was that Parliament had not placed either ships or funds at the disposal of the Admiralty for any such purpose. This was a rebuff little anticipated; and, I may add, little deserved by those of our countrymen, who, in a noble spirit of disinterestedness, had offered to give up their time, and leave their homes, to undergo fatigue and risk in the cause of science. In consequence of this answer some delay arose, to consider what was to be done. An appeal against the decision of the Admiralty, to the Premier and the Chancellor of the Exchequer, was resolved on. Some farther delay occurred in consequence of the absence of these high functionaries from London. Meanwhile, the United States ship arrived in England, bringing with them the American astronomers. They soon learnt the unsatisfactory position of the negotiation with our Government; and, in consequence of it, they sent a formal invitation through their director, inviting the English astronomers to accompany them in their ship to Spain and Sicily. This letter was published in the London newspapers; and severe comments were made by the press on our executive, if they should oblige the English party to avail themselves of the invitation, and be beholden to a foreign Government for assistance. Fortunately for the credit of the country, our Government at length yielded to the pressure. A sum of L.3000 was agreed to be set apart to pay expenses, and a troop ship was appointed to convey the party and their instruments. But no Government astronomer received authority to accompany the expedition, and no engineer officer, or other official representing the Government, was appointed to take charge of the expedition, and give assistance. In all these respects the British Government fell far short of what had been done by the United States Government, to aid in the cause.

I have related thus fully the circumstances connected with this Solar Eclipse Expedition, because it has occurred recently, and therefore shows too plainly the indifference to science, and to men of science, which actuates those who manage the affairs of this country. It is, however, a charge which unfortunately does not lie at the door of the present executive alone. The same indifference has been too clearly manifested by almost all preceding Governments. Unmistakable evidence of this indifference is afforded by the treatment of the societies and associations formed for the advancement of science. What aid is given to any of these? The only part of the United Kingdom in which such aid is liberally given is in Ireland.* Except to the Academy of Music in London, which receives annually a grant of L.500, I know of no Society of a scientific character, either in England or in Scotland, which receives any grant to carry out its special objects. The only patronage to English scientific societies consists in the free use of Government apartments in London to seven of these societies, and the free use of Government apartments in Edinburgh to two Scotch societies—viz., the Royal Society and the Society of Antiquaries.† There is another society which has been very kindly allowed to occupy two small apartments in the General Post-Office Buildings; but for the use of these a rent is exacted; and, moreover, from this society statistical information is obtained by Government, for which, however, Government does not pay, and declines to pay.

This illiberal feature of the British Government in not aiding voluntary associations for scientific objects, is the more remarkable considering the principle which our Government adopts for

* In Dublin there are six societies, two of which are for the encouragement of the fine arts, particularly painting, which receive about L.13,000 yearly, to enable them to carry out their special objects and to keep their buildings in repair. (See Report of Royal Commissioners on Aid given to Irish Societies, presented to Parliament in 1869.)

† The Royal Society of Edinburgh has, since the year 1836, received from the Exchequer a yearly sum of L.300 to enable them to pay rent, taxes, and maintenance of the apartments they occupy. The rent charged by Government for these apartments is L.260. The Society of Antiquaries receives L.300, which is all applied to pay the officers who take charge of the Museum, and the necessary repairs and cleaning. The Museum belongs to the Government.

other associations having objects not more beneficial to the public. The principle is, that when funds are voluntarily supplied from local sources, the State supplements these by an addition of as much money from the Exchequer. The local subscriptions are justly taken as evidence that the objects are praiseworthy, and that they are appreciated by the community; whilst any risk of misapplication or mismanagement is avoided by an annual report to Government. This principle has been applied to schools and various other educational institutions, to volunteer corps, to county constabulary, &c.

Whilst pointing out the illiberal, short-sighted, and inconsistent policy of the British Government in not assisting scientific societies with pecuniary grants to aid them, it would be wrong in me not to take grateful notice of a parliamentary grant of L.1000 a year given to encourage scientific investigations carried on anywhere in the United Kingdom or colonies of Great Britain. Of this grant I could find no authentic account in any publication. General rumour only was my authority for believing that such a grant existed, and that it was at the disposal of the Royal Society of London. On my speaking to Professor Balfour on the subject, I found that he could give me no information, but he kindly undertook to apply to Dr Sharpey, the secretary of the Royal Society of London. Dr Sharpey at once responded, by sending a memorandum explanatory of the grant—a memorandum which appears to me of sufficient importance to be now laid before our Society:—

"MEMORANDUM as to the 'Government Grant' placed annually at the disposal of the Royal Society.—Nov. 30, 1870.

"In 1849 the First Lord of the Treasury (Lord John Russell) *offered*, on the part of the Government, to place L.1000 at the disposal of the Royal Society, to be by them applied towards the advancement of science.

"This offer was accepted. The first payment was made in 1850, and it has been repeated annually up to the present time. Up to 1855 the grant was paid from a special fund at the disposal of the Treasury, but since then it has been annually voted by Parliament.

"The Council of the Royal Society consider the grant as a contribution on the part of the nation towards the promotion of science

generally in her Majesty's dominions, regarding themselves as trustees of the grant, and accountable to the public for its due administration, as long as it shall be continued.

"To aid the Council in the distribution of the fund, a committee is annually appointed, consisting of the 21 members of the Council and 21 Fellows of the Society not on the Council, selected on account of their acquaintance with the different branches of science which the Society cultivates. All applications for grants from the fund are submitted to this committee, and the appropriations are made by the Council on the committee's recommendation.

"The grants are commonly made to individuals engaged in some definite scientific investigation, chiefly to meet the expense of apparatus and materials, and not as remuneration for time or labour bestowed by the inquirer. To a less extent appropriations have been made for like purposes to scientific institutions, and, more rarely, to aid in the publication of valuable scientific results.

"The distribution of the fund is not restricted to Fellows of the Royal Society, nor have they any privilege in regard to it; men of science, whether belonging to the Society or not, and wherever they may carry on their researches, in this country or the colonies, have an equal title to participate, and their claims have been in all cases equally recognised.

"No part of the fund is applied towards the expenses of the Royal Society, and the Society neither asks nor would accept any remuneration for its stewardship.

"It is to be noted that, in 1864, the Council, finding that the unappropriated balance, together with other funds at their disposal, would meet the probable demands for scientific objects, repaid the grant of that year into the Exchequer.

"A return was made to Parliament in 1855, stating the application of the fund for the five years ending 5th April 1855. This statement will be found printed in the 'Proceedings of the Royal Society,' vol. vii. page 512. A second return was made in 1862, showing the distribution of the fund from 1855 to 1862. No later return has been called for, although the Council would be glad to make it if ordered.

"It is proposed hereafter to publish an annual statement of the disposal of the grant in the Proceedings. W. S."

Dr Sharpey, besides drawing out the foregoing memorandum, explaining the origin and objects of this parliamentary grant, has been so obliging as to send two printed returns, giving for the first twelve years the names of the persons who have shared in the grant, and the nature of the researches aided. Besides these returns (to Parliament), he has sent a statement—apparently not yet published—containing similar information for the years 1869 and 1870. For the years from 1862 to 1869, no information is given, except that in the year 1864, as the memorandum mentions, the remarkable circumstance occurred, of the Society having paid back to Government the L.1000, in consequence of there being no claims on it which could not be otherwise met.

Now, no one who looks at the returns showing how these annual grants were expended, will question the judicious and impartial manner in which they have been administered. I would, however, venture to remark, that as the grant was intended to assist scientific researches in all parts of her Majesty's dominions, colonies included, some means should have been taken to make the existence and the objects of the grant publicly known. The grant would, of course, be known to the Fellows of the Royal Society of London, but it has remained ever since its institution, now twenty years ago, generally unknown to men of science, and especially to persons resident in Scotland and Ireland. It is therefore not surprising that, in the year 1864, there being no demands on the grant, it had to be paid back to Government; and that out of the L.14,000 embraced by the returns, no more than L.610 should have been expended on researches in Scotland. The great part of these researches was made by two individuals, both of them Fellows of the Royal Society of London.

It appears to me that, so far as the interests of science in Scotland are concerned, these interests, if intended to be aided by a pecuniary grant from the State, would be better promoted were the grant administered by a suitable board in Scotland, instead of by one in London. Any researches and experiments carried on in Scotland, and the scientific character of the men who carry them on, must surely be better known in Edinburgh than in London. Limited as are my own opportunities of knowing of such researches and experiments, I may refer to some on the difficult

and important subject of ozone, which, after being carried on for some time in the Edinburgh Botanic Garden last year,* had to be discontinued on account of the want of apparatus and instruments which those who instituted them had no means of paying for.†

I certainly do not wish, however, that the grant of L.1000, which is at the disposal of the Royal Society of London, should be split up, so that a part of it may be administered to a Scotch Society, if the London Royal Society think that they can apply it all usefully in England. All that I contend for is, that when parliamentary grants are voted for aiding scientific researches throughout the United Kingdom, it is not a judicious arrangement for the object in view to place these grants at the exclusive disposal of a society in London, when there are societies in Scotland and in Ireland competent to be intrusted with the duty. A committee of the Royal Society of London are also intrusted with the administration of the still larger parliamentary grant of L.10,000 a year for meteorological purposes,—a considerable part of which grant is devoted to the obtaining of meteorological returns from Scotland, and of establishing self-recording instruments in Scotland, besides upholding other stations. Our own Royal Society has from time to time done a good deal to promote meteorology in Scotland,—Sir David Brewster, Sir Thomas M. Brisbane, and Principal Forbes, having been distinguished meteorologists, and published largely in our Transactions. There is also a society in Scotland specially devoted to that science, which is allowed to be doing useful work. Yet neither society has any voice in the administration of that large grant of L.10,000 a year.

Whilst as regards the interests of science it seems more expe-

* See an account of these experiments in the "Journal of the Scottish Meterological Society" for January 1869.

† The test papers for ozone indications are affected by the varying force of wind, as also by the varying humidity of the atmosphere, insomuch that at several Observatories ozone observations have been discontinued. When I was at Rome last winter, Padre Secchi told me he had ceased to take notice of ozone for these reasons, not having been able to devise any method for eliminating the effects of wind and moisture. The object of the experiments in the Edinburgh Botanic Garden was to construct an apparatus which should allow only dry air to reach the test papers, and in certain quantities.

dient that the board intrusted with the expenditure in Scotland should be in Edinburgh rather than in London, is it not also a slur on Scotch scientific societies that they should be altogether ignored, and a London society selected, as if the former were unworthy, or could not be trusted?

I therefore regret this system of centralisation in London, and cannot help thinking that our Society ought not so tacitly to acquiesce in it. In one of his addresses from this chair, Sir David Brewster, in alluding to the annual grant of L.1000, as well as the two royal medals, placed at the disposal of the Royal Society of London, expressed his belief "that an earnest representation made to the Government would obtain for us a similar, though probably a smaller grant;" and it humbly appears to me that such a representation ought to be made without farther delay.

The expediency of energetic action on our part is more manifest because of a proposal made lately in an influential quarter to enlarge the amount of the grant to the Royal Society of London. Professor Balfour Stewart a few weeks ago, at the inauguration of Owen's College, Manchester, in his opening address there, made the following remarks:—

"If Government be disposed to grant pecuniary aid to physical researches, an extension of the allowance made annually to the Government Grant Committee of the Royal Society, would be a very legitimate way of accomplishing this object. No one can doubt that the small sum of L.1000 annually intrusted by Government to that Society for miscellaneous experiments is administered in a praiseworthy manner; and if the Government would be ready to grant, and the Royal Society willing to undertake, an extension of this trust, it would be a great point gained." *

This suggestion will no doubt obtain consideration from the Royal Commissioners appointed to report whether the State now gives enough for the encouragement of science. All or most of these commissioners are Fellows of the Royal Society of London, and two of them are office-bearers of the Society. A fairer selection of eminent men for the object in view could not have been made; and though none of them are Fellows of the Royal Society of Edinburgh, I am sure that they will not on that account be less

* Lieutenant-Colonel Strange, an influential member of the British Association, sends a letter to "*Nature*," Nov. 3, 1870, in which he adverts to Professor Balfour Stewart's idea of enlarging the grant of L.1000 administered by the Royal Society of London, and expresses cordial concurrence.

disposed, perhaps the more disposed, to listen to any representation which we may lay before them.

But, apart from our own interest as a society in the deliberations of these Royal Commissioners, I entertain a very sanguine hope that much good will accrue from them. The very concession of a Commission on the part of Government seems to imply a conviction and acknowledgment, that the patronage hitherto given in this country to science is not what it should have been, and that reform in this respect is quite as much needed as in other matters. We have been lately confessing our shortcomings as regards national schools, and are endeavouring to remedy these; but we ought not to be satisfied with merely teaching old truths and well-known facts. The investigation of new truths and new facts, and the opening out of new pathways in the wide field of knowledge, are also necessary if we are to help in extending civilisation, and if we are to uphold our position in the family of nations. It should no longer be left to the chance of individuals being found to carry on, from their own resources, the great and noble work of making fresh discoveries in science and art. That work is worthy of State patronage, as it also more than ever needs State assistance; and unless that work is carried on energetically and successfully, we shall lose caste as an enlightened people, and see the chief sources of our prosperity and power dried up.

Therefore I look forward, with no small anxiety, to the report of these Royal Commissioners. But I confidently anticipate favourable results; and in pointing out the best channels through which aid to science from the State may flow, I have no doubt that our own past services, and our present efficiency as a society, will not be overlooked.

In these expectations I may possibly be over-sanguine, and therefore allow me to add, in conclusion, a single remark as to our own duty in this matter:—As a society, and so far as our scanty funds enable us, we will continue to encourage scientific researches in Scotland, not forgetting, however, that we have also literary objects; and as Fellows of the Society,—a Society which during its time has done much in the cause of science, and something too on behalf of literature, we will do what we can to uphold its reputation, and extend its influence and usefulness.

The following Gentleman was elected a Fellow of the Society:—

John Auld, Esq., W.S.

Monday, 19th December 1870.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. Additional Remarks on the Theory of Capillary Attraction. By Edward Sang, Esq.

2. Laboratory Notes: On Thermo-Electricity. By Professor Tait.

In a paper presented to the Society in 1867–8 I deduced from certain hypothetical considerations regarding Dissipation of Energy results connected with the thermal and electric conductivity of bodies, the electric convection of heat, &c. As these were all of a confessedly somewhat speculative character, I printed at the time only that connected with thermal conductivity, which I had the means of comparing with experiment, and which seemed to accord fairly with Forbes' experimental results. But the assumption on which this was based was essentially involved in all the other portions of the paper.

With a view to the testing of my hypothetical result as to electric convection of heat, several of my students, especially Messrs May and Straker, last summer made a careful determination of the electromotive force in various thermo-electric circuits through wide ranges of temperature. Their results for a standard iron-wire, connected successively with two very different specimens of copper, when plotted, showed curves so closely resembling parabolas that I was led to look over my former investigations and determine what, on my hypothetical reasoning, the curves should be. This I had entirely omitted to do. I easily found that the parabola ought, on my hypothesis, to be the curve in every case, and I made last August a numerous and careful set of determinations with Kew standard mercurial thermometers as an additional verification.

My hypothetical result was to the effect that what Thomson (Trans. R.S.E. 1854, Phil. Trans. 1856) calls the specific heat of electricity, should be, like thermal and electric resistance, directly proportional in pure metals to the absolute temperature, the coefficient of proportionality being, for some substances, negative.

Hence, using Thomson's notation as in Trans. R.S.E., we have for any two metals

$$J\sigma_1 = k_1 t\ ,\ J\sigma_2 = k_2 t\ ,$$

where k_1 and k_2 are constants, whose sign as well as value depends on the properties of each metal, σ_1, σ_2 are the specific heats of electricity, and J is Joule's Equivalent.

Thus, introducing these values into Thomson's formulæ, we have

$$(k_1 - k_2)t = J(\sigma_1 - \sigma_2) = J\left(\frac{\Pi}{t} - \frac{d\Pi}{dt}\right),$$

where Π is the Peltier effect at a junction at absolute temperature t. Integrating, we have

$$C - (k_1 - k_2)\,t = J\frac{\Pi}{t}\,,$$

or

$$J\frac{\Pi}{t} = (k_1 - k_2)(t_0 - t)\,,$$

where t_0 is the constant of integration, obviously in this case the temperature at which the two metals are thermo-electrically neutral to one another. Hence the Peltier effect may be represented by the ordinates of a parabola of which temperatures are the abscissæ; the ordinates being parallel to the axis of the curve.

The electromotive force in a circuit whose junctions are at absolute temperatures t and t' is then represented by

$$E = J\int_{t'}^{t} \frac{\Pi}{t}\,dt = \tfrac{1}{2}(k_1 - k_2)[2t_0(t - t') - (t^2 - t'^2)]$$

$$= (k_1 - k_2)(t - t')\left[t_0 - \frac{t + t'}{2}\right].$$

This, of course, is again the equation of a parabola. That $t - t'$ is a factor of E has long been known, and Thomson has given the results of many experiments tending to show that $t_0 - \frac{t + t'}{2}$ is also

a factor. But it was not till the experiments in my Laboratory had been carried on for some months that I was referred by Thomson to a paper by Avenarius (*Pogg. Ann.* 119), in which it is experimentally proved (partly in contradiction of an assertion of Becquerel) that in a series of five different thermo-electric circuits the electro-motive force can be very accurately expressed by *two* terms of the assumed series

$$E = b(t - t_2) + c(t_1^2 - t_2^2) + \quad . \quad . \quad .$$

where t_1 and t_2 are temperatures as shown by the ordinary mercurial thermometer. It follows from this that (neglecting the difference between absolute temperatures and those given by the mercurial thermometer) E has no other variable factor than those above given.

Curiously enough, Avenarius, whose paper seems to have been written mainly for the purpose of attempting to explain (by the consideration merely of the effect of heat on electricity of contact of two metals) the production of thermo-electric currents, does not allude to the fact that the above equation represents a parabola. In fact he gives several figures, in all of which it is represented as a very accurately drawn *semicircle.* He makes no application of his empirical formula to the determination of the amount of the Peltier effect, nor does he seem to recognise the existence of what Le Roux has called "l'effet Thomson," which is indispensable to the explanation of the observed phenomena.

All the curves plotted by Messrs May and Straker, which were derived from iron, copper, and platinum alone, as well as my own, which included cadmium, zinc, tin, lead, brass, silver, and various other substances (sometimes arranged with a double arc of two different metals connecting the hot and cold junctions) were excellent parabolas. When the temperatures were very high, the parabola was slightly steeper on the hotter than on the colder side. This, however, was a deviation of very small amount, and quite within the limits of error introduced by the altered resistance of the circuit at the hotter parts, the deviations of the mercury thermometers from absolute temperature, and the non-correction of the indication of the thermometers for the long column of mercury not immersed in the hot oil round the junction.

To settle the question rigorously, I have been for some time ex-

perimenting with an arrangement sometimes of double metallic arcs, sometimes of two separate thermo-electric circuits acting on a differential galvanometer—a second object being to obtain, if it be possible, an arrangement capable of replacing with sufficient accuracy the air-thermometer in the measurement of very high temperatures, and where very exact results are not required.

In fact, if the formula above be correct, we have for two circuits with their junctions immersed in the same vessels

$$E = a(t - t_1)\left(t_0 - \frac{t + t_1}{2}\right)$$

$$E' = a'(t - t_1)\left(t'_0 - \frac{t + t_1}{2}\right)$$

so that if the resistances in the circuits be made as a to a' their resultant effect on the differential galvanometer will be proportional to

$$(t_0 - t'_0)(t - t_1) .$$

It is obvious that so far as these factors are concerned the most sensitive arrangements will be such as have their neutral points farthest apart. On a future occasion I hope to lay the results of my new experiments before the Society. They appear to promise to be of great use in furnishing an easily working and approximately accurate substitute for the air-thermometer in an inquiry on which I am engaged respecting specific heats and melting points of various igneous rocks, &c., while the comparison of the indications of two such arrangements at very high temperatures will give the means of determining whether the quantities called k above are really constants.

3. Note on Linear Differential Equations in Quaternions. By Professor Tait.

The generally non-commutative character of quaternion multiplication introduces into the solution even of linear differential equations with constant (quaternion) coefficients, difficulties of a somewhat novel character. To some of these which have presented themselves to me in many investigations, I wish to draw attention in the following note, but want of leisure prevents my attempting at present either to classify the numerous curious forms which may be met with in physical inquiries, even when these lead to mere

vector equations of an order no higher than the second, or to develope the subject of the curious functional equations which are incidentally involved.

1. The integration of an equation such as

$$\dot{q} + mq = a \, ,$$

where m is a *scalar* (usually a function of t, which is assumed throughout as the independent variable), and q an unknown quaternion, is obviously to be effected by the ordinary method, multiplication by $\epsilon^{\int m dt}$.

2. But if a be a *quaternion*, the integration of

$$\dot{q} + aq = a' ,$$

even when a is constant, requires a little care, unless we boldly treat a as m was treated in the preceding section. This, no doubt, gives the correct result, but the process requires to be defended. Assume therefore r to be a factor which makes the left hand member integrable. Then we must have

$$\dot{r} = ra \, ,$$

or, if r' be a proximate value of r,

$$r' = r + \dot{r}\delta t = r(1 + a\delta t) \, .$$

Hence, dividing the finite interval t into a great number of equal parts, and taking the limit

$$r = r_0 \, \mathrm{L}_\infty \left(1 + \frac{at}{n}\right)^n$$

$$= r_0 \, \epsilon^{at}$$

where r_0 is an arbitrary but constant quaternion.

Now we have

$$\epsilon^{at} = \epsilon^{t(Sa + TVa \, . \, UVa)} = \epsilon^{t(m + n\alpha)}, \text{ suppose}$$

$$= \epsilon^{mt} \alpha^{\frac{2nt}{\pi}} \, .$$

Hence the solution of the given equation is

$$\epsilon^{mt} \alpha^{\frac{2nt}{\pi}} q = \int \epsilon^{mt} \alpha^{\frac{2nt}{\pi}} a' \, dt \, ,$$

the arbitrary quaternion constant r_0 having disappeared, but a new one being introduced by the integration on the right.

When a is variable, the tensor of r is easily seen to be $\epsilon^{\int Sadt}$, but its versor, s, is to be found from the equation

$$\dot{s} = sVa$$

the fundamental relation between the instantaneous axis and the versor of rotation of a rigid body (*Trans. R.S.E.*, 1868).

When r is a vector, θ suppose, we have

$$\dot{\theta} = V\theta a\,,$$

whence, as above,

$$\theta = V\theta_0 \epsilon^{\int adt}\,.$$

3. In the succeeding examples we restrict ourselves to equations for the determination of unknown *vectors*, as we thus avoid the introduction of the quartic equation which has been shown by Hamilton to be satisfied by a linear function of a *quaternion*. This would appear, for instance, in the solution of even the simple equation

$$\dot{q} + aqb = c$$

where a and b are constant quaternions; though, of course, its use may be avoided by employing a somewhat more cumbrous process.

4. Suppose we have

$$\dot{\rho} + \phi\rho = a$$

where ϕ is a self-conjugate linear and vector function with constant constituents. Operate by $S\,.\,\delta$, and we have

$$S\delta\dot{\rho} + S\,.\,\rho\phi\delta = S\delta a\,.$$

The left hand side is a complete differential if

$$\dot{\delta} = \phi\delta\,.$$

The general integral of this equation may be written as

$$\delta = \epsilon^{t\phi}\delta_0$$

where ϵ^{ϕ} is another linear and vector function; but it is not necessary to discuss here the validity of such a result, deduced as it must be by a process of separation of symbols. [See Tait's *Quaternions*, § 290.] For, on account of the properties of ϕ, we may

assume (since but three distinct and non-coplanar values of δ are required)

$$\delta = x\,\eta$$

where η is a constant unit-vector, and x a scalar function of t. This gives

$$\frac{\dot{x}}{x}\eta = \phi\eta\,.$$

The values of η are therefore unit-vectors parallel to the axes of the surface

$$S\,\rho\phi\rho = 1\,,$$

and those of $\frac{\dot{x}}{x}$ are the roots of the auxiliary cubic in ϕ. Call them η_1, η_2, η_3 and g_1, g_2, g_3 respectively, then the values of δ (into which no arbitrary constant need be introduced), are of the form

$$\epsilon^{gt}\eta\,.$$

Thus, finally,

$$\begin{aligned} \rho &= -\,\Sigma\eta S\eta\rho \\ &= -\,\Sigma\epsilon^{-gt}\eta\left[\int\epsilon^{gt}S\eta\alpha dt + C\right]. \end{aligned}$$

5. If, in the equation of (4), we suppose α constant, we may easily apply a process similar to that of (2).

For

$$\rho' = \rho + \dot{\rho}\delta t = (1 - \delta t\,.\,\phi)\,\rho + \alpha\delta t\,.$$

Hence, as α is constant,

$$\begin{aligned} \rho &= L_\infty\left(1 - \frac{t\phi}{n}\right)^n\rho_0 + L_\infty\frac{\left(1 - \frac{t\phi}{n}\right)^n - 1}{\left(1 - \frac{t\phi}{n}\right) - 1}\cdot\frac{\alpha t}{n} \\ &= \epsilon^{-t\phi}\,\rho_0 + \phi^{-1}\alpha \end{aligned}$$

where ρ_0 (which is arbitrary) has been increased by $\phi^{-1}\alpha$. It is easy to show that this agrees with the final result of (4), and the coincidence is so far a justification of the use of the method of separation of symbols.

The verification of the general result of (4), where α is variable, can also be effected by this method, but not so readily.

6. Let us take the linear equation of the second order with

constant coefficients (equivalent to three simultaneous linear equations in scalars of a very general form)

$$\ddot{\rho} + \phi\dot{\rho} + \psi\rho = 0 ,$$

where ϕ and ψ may, or may not, be self-conjugate.

If they be self-conjugate, this represents oscillation under the action of a force whose components, in each of three rectangular directions, are made up of parts proportional to (though not necessarily equimultiples of) the displacements in these directions. The resistance parallel to each of three other rectangular directions depends in a similar manner on the corresponding components of the velocity.

The operator in the left hand member may be written

$$\left(\frac{d}{dt}\right)^2 + \phi . \frac{d}{dt} + \psi = \left(\frac{d}{dt} + \chi\right)\left(\frac{d}{dt} + \theta\right),$$

suppose, where χ and θ are two new linear and vector functions.

Hence, comparing, we must have

$$\chi + \theta = \phi$$
$$\chi\theta = \psi ,$$

or, eliminating θ,

$$\chi^2 + \psi = \chi\phi$$

a curious and apparently novel species of equation from which to determine the function χ.

[We might have arrived at it, by a somewhat more perilous but shorter route, by assuming as a particular integral of the given equation the expression

$$\rho = \epsilon^{-t\chi}\rho_0 .]$$

If we take their conjugates in addition to the two equations connecting θ and χ, we see at once that all four are satisfied by assuming these two functions to be conjugate to one another, provided ϕ and ψ are self-conjugate. Hence in this special case we may write

$$\left.\begin{aligned}\chi &= \tfrac{1}{2}\phi + V.\epsilon \\ \theta &= \tfrac{1}{2}\phi - V.\epsilon\end{aligned}\right\} .$$

It only remains that we should find ϵ, and the rest of the solution is to be effected as in (4) or (5).

We have

$$\psi = \chi\theta = \frac{\varphi^2}{4} + \frac{1}{2}(\mathrm{V}.\epsilon\varphi - \varphi\mathrm{V}.\epsilon) - \mathrm{V}.\epsilon\mathrm{V}.\epsilon\,.$$

When φ is a constant scalar, *i.e.*, when the resistance is in the direction of motion (which is the case generally in physical applications) the middle term vanishes, and we have

$$\mathrm{V}.\epsilon\mathrm{V}.\epsilon = \frac{\varphi^2}{4} - \psi\,,$$

or, as it may be written,

$$\mathrm{V}.\epsilon = \left(\frac{\varphi^2}{4} - \psi\right)^{\frac{1}{2}}.$$

In fact, in this case, φ and χ are commutative in multiplication, so that the equation in χ may be solved as an ordinary quadratic.

Even this very particular case involves a singular question, though not one of such difficulty as that of the general problem above. We have, in fact, to solve an equation of the form

$$\varpi^2 = \omega\,,$$

where ω is a given, and ϖ a sought, linear and vector function. This leads to an equation of the sixth degree in ϖ with pairs of roots equal but of opposite signs. The coefficients of the cubic in ϖ are formed by the solution of a biquadratic equation.*

* Suppose the cubic in ϖ to be

$$\varpi^3 + g\varpi^2 + g_1\varpi + g_2 = 0\,,$$

the given equation enables us to write it in either of the (really identical) forms

$$(\varpi + g)\omega + g_1\varpi + g_2 = 0\,,$$

or

$$\varpi(\omega + g_1) + g\omega + g_2 = 0\,;$$

whence

$$\omega = \left(\frac{g\omega + g_2}{\omega + g_1}\right)^2,$$

or

$$\omega^3 + (2g_1 - g^2)\omega^2 + (g_1^2 - 2gg_2)\omega - g_2^2 = 0\,.$$

If the cubic in ω be

$$\omega^3 + m\omega^2 + m_1\omega + m_2 = 0\,,$$

we have by comparison of co-efficients

$$2g_1 - g^2 = m\,.\quad g_1^2 - 2gg_2 = m_1\,.\quad g_2^2 = -m_2$$

so that g_2 is known and

$$g = \frac{g_1^2 - m_1}{2\sqrt{-m_2}}$$

In fact, if we apply the members of the general equation above to ϵ, we have

$$V.\epsilon\phi\epsilon = 2\left(\psi - \frac{\phi^2}{4}\right)\epsilon .$$

This leads to the two equations

$$S.\epsilon\left(\psi - \frac{\phi^2}{4}\right)\epsilon = 0 ,$$

$$S.\epsilon\phi\left(\psi - \frac{\phi^2}{4}\right)\epsilon = 0 ,$$

which, belonging to two cones of the second degree, give in general four values of ϵ.

7. The interest of the general question before us, from the analytical point of view, lies mainly in the determination of the two unknown linear and vector functions χ and θ from the equations

$$\chi + \theta = \phi ,$$
$$\chi\theta = \psi ,$$

each of which is in general equivalent to *nine* or in certain cases *six* (not, as in ordinary quaternion equations, *four*, or as in vector equations *three*) simultaneous scalar equations. They have also a

where

$$2g_1 = m - \frac{(g_1^2 - m_1)^2}{4m_2} .$$

The values of g being found, ϖ is given by the expression above.

A similar process may easily be applied to the general equation of (6), but it may be well to exhibit the present simple case in its Cartesian form.

Let
$$Si\omega i = p_1 ,\quad Si\omega j = p_2 ,\quad Si\omega k = p_3 ,$$
$$Sj\omega i = q_1 ,\quad Sj\omega j = q_2 ,\quad Sj\omega k = q_3 ,$$
$$Sk\omega i = r_1 ,\quad Sk\omega j = r_2 ,\quad Sk\omega k = r_3 .$$

Also let
$$\varpi = \alpha Si + \beta Sj + \gamma Sk ,$$
where
$$\alpha = ix_1 + jx_2 + kx_3 ,$$
$$\beta = iy_1 + jy_2 + ky_3 ,$$
$$\gamma = iz_1 + jz_2 + kz_3 ,$$

then the problem reduces itself to the determination of the nine scalars x, y, z, &c., from nine equations of the second degree, of which we write only the first three :—viz.

$$x_1^2 + y_1x_2 + z_1x_3 = p_1 ,$$
$$x_2x_1 + y_2x_2 + z_2x_3 = p_2 ,$$
$$x_3x_1 + y_3x_2 + z_3x_3 = p_3 .$$

physical interest, inasmuch as they include the problem of finding two homogeneous strains, such that the vector-sum of their effects on any vector shall represent the effect of one given strain on that vector, while the effect of their *successive* performance in a given order on any vector shall be equivalent to that of another given strain. It is curious to compare this with the physical meaning of the differential equation from which these forms are derived.

If g be one of the roots of the symbolical cubic in χ (of which two will in this case generally be imaginary) and η the corresponding unit vector, such that we have three conditions of the type

$$(\chi - g)\eta = 0\,,$$

we have

$$(g^2 - g\phi + \psi)\eta = 0\,.$$

The vectors, which satisfy this and the two similar equations, are (all three) sides (real or imaginary) of the cone of the third order

$$S.\rho\phi\rho\psi\rho = 0\,.$$

One curious result, which is easily derived from the equations above, is that, if a solid experience a pure strain, the planes in which any three, originally rectangular, vectors are displaced intersect in one line.

4. On some Quaternion Integrals. By Professor Tait.

(*Abstract.*)

In my paper on "Green's and other allied theorems" (*Trans. R. S. E.* 1869–70), I showed that

$$\int P d\rho = \iint ds\, V.U\nu\nabla P\,,$$

where P is any scalar function of ρ, and the single integral is extended round any closed curve, while the double integral extends over any surface bounded by the curve, ν being its normal vector.

Writing

$$\sigma = iP + jQ + kR$$

this gives at once

$$\int \sigma d\rho = \iint ds (S.U\nu\nabla\sigma - V.(V.U\nu\nabla)\sigma)\,,$$

of which the scalar and vector parts respectively were, in the paper referred to, shown to be equal.

From these equations many very singular results may be derived, some of which form the first part of the subject of the present communication.

Let σ be a vector which, having continuously varying values over the surface in question, becomes $Ud\rho$ at its edge. Then

$$-\int Td\rho = \iint ds\, S\,.\,U\nu\nabla\sigma,$$

there being no vector part on the left-hand side. This gives the *length* of any closed curve in terms of an integral taken over any surface bounded by it.

We have evidently

$$T\rho\, dT\rho = -S\rho d\rho,$$

whence

$$\int PdT\rho = -\int PS\,.\,U\rho d\rho = -\iint ds S\,.\,U\nu\nabla(PU\rho).$$

Hence

$$\int \sigma dT\rho = -\iint ds\, S\,.(U\rho U\nu\nabla)\sigma,$$

for

$$\nabla U\rho = -\frac{2}{T\rho}.$$

Now if $T\rho$ be constant over the boundary, *i.e.*, if the bounding curve lie on a sphere whose centre is the origin, we have for any surface bounded by it

$$\iint ds\, S\,.(U\rho U\nu\nabla)\sigma = 0,$$

whatever be the value of the vector σ.

Again, if σ be a function of $T\rho$ only, we have

$$\int \sigma\, dT\rho = 0$$

for all closed curves. Hence, whatever be the vector-function ϕ, and whatever the surface and its bounding curve, we have always

$$\iint ds\, S\,.(U\rho U\nu\nabla)\,\phi(T\rho) = 0.$$

Another very simple but fundamental theorem, in addition to those given in the paper above referred to, may be stated as follows:—Let P be the potential of masses external to a space Σ. Then throughout Σ we have

$$\nabla^2 P = 0,$$

so that

$$\iiint \nabla^2 P ds = \iint SU\nu\nabla P\,.\,ds = 0.$$

The double integral is therefore of constant value for all non-closed surfaces having, as common boundary, a closed curve and not extending into space occupied by any part of the masses. To find its value in terms of a single integral taken round this curve, let

$$\nabla^2\tau = \nabla P.$$

As P is known, the constituents of τ are perfectly definite, being the potentials of given distributions of matter. And the substitution of functions of τ for those of P gives us, by means of the general formula at the beginning of this paper,

$$\iint SU\nu\nabla P\,.\,ds = S\int V\,(d\rho\nabla)\,\tau\,,$$

with the condition

$$S\nabla\tau = 0\,.$$

Again, we have obviously, as $\nabla^2\sigma$ is necessarily a vector,

$$\iint S\,.\,U\nu\nabla^2\sigma ds = \int S\,.\,\nabla\sigma d\rho\,.$$

Now, let $\sigma = iP$, then

$$\iint S\,.\,iU\nu\,.\,\nabla^2 P ds = \int S(id\rho\nabla)P\,.$$

From this

$$\iint U\nu\nabla^2 P ds = \int V(d\rho\nabla)P\,.$$

A particular case of this, for a curve in the plane of xy and the surface bounded by it, is

$$\iint\left(\frac{d^2P}{dx^2}+\frac{d^2P}{dy^2}\right)dxdy = \int\left(\frac{dP}{dx}dy - \frac{dP}{dy}dx\right)$$

which has obvious applications to fluid motion parallel to a plane.

But, generally, we have also

$$\iint U\nu\nabla^2\sigma ds = \int V(d\rho\nabla)\,.\,\sigma\,.$$

If we take the vector of this, or if we subtract from each side the corresponding member of our first equation above, we have

$$\iint V\,.\,U\nu\nabla^2\sigma ds = \int V\,.\,(V\,.\,d\rho\nabla)\sigma\,.$$

These results appear to be of considerable importance for physical applications, and are particularly interesting, because they involve the operator (indicated merely in my former paper).

$$V(d\rho\nabla)\,.$$

The paper contains several applications and modifications of these theorems.

5. Note on an Ice Calorimeter. By Dr A. Crum Brown.

The principal upon which this calorimeter is founded is, that a contraction of a definite amount takes place on the conversion of ice at 0° C. into water at 0° C., and that a definite amount of heat is required for this conversion. Early in the year 1866 I sent a description and drawing of the instrument to Messrs Kemp & Co., instrument-makers here, with an order to have it constructed. Some mechanical difficulties occurred which prevented its completion at the time. I should not have laid before the Society an account of an unfinished instrument were it not that Professor Bunsen has recently published * an account of a calorimeter founded on the same principle. The two instruments are quite different in detail, and are primarily intended for different purposes—Professor Bunsen's for the estimation of specific heat, and mine for the estimation of the heat produced during chemical changes.

While, of course, fully acknowledging Professor Bunsen's priority, I lay this note before the Society for the purpose of preserving to myself the right to use my own instrument.

It consists of a cylindrical vessel A, the *calorimeter*, furnished with a tightly-fighting flanged lid of a conical form. This is fixed to the corresponding flange on the calorimeter by means of binding screws, and has a small hole at its apex, which can be completely closed by means of a screw D.

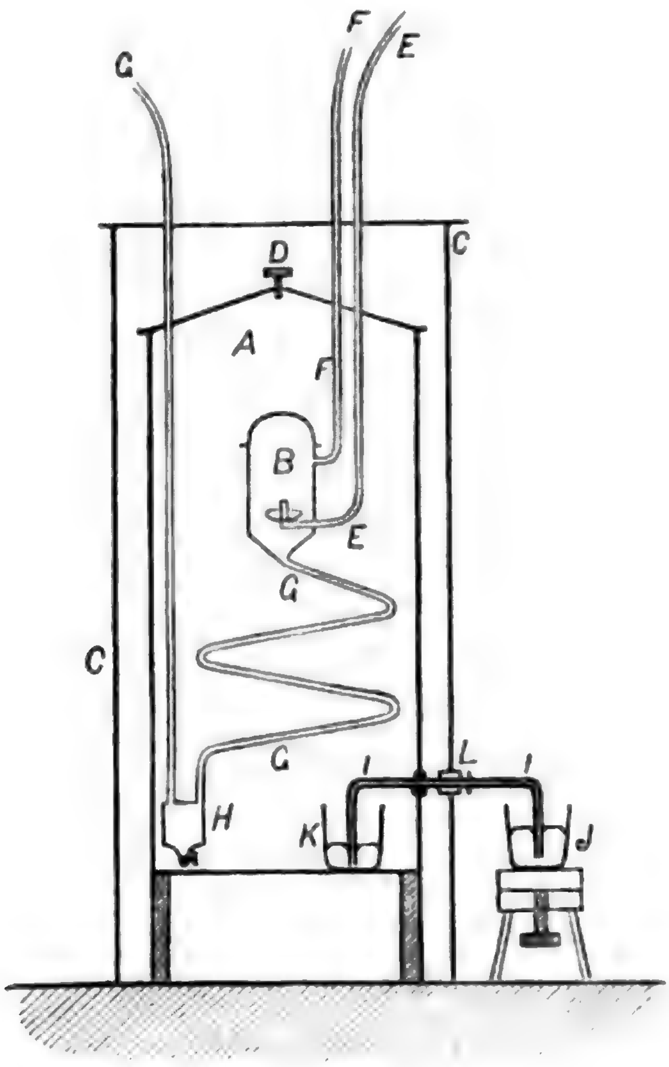
Within the calorimeter is contained a smaller cylindrical vessel B, the *laboratory*, closed above by means of a flanged lid. Into it open two tubes, EE and FF. One of these, EE, carries a small plate, upon which apparatus may be placed. From the bottom of the laboratory a tube, GGG, passes, spirally bent in its descending part, and having a reservoir with a stop-cock between its descending and ascending parts. All these tubes pass tightly through the lid of the calorimeter.

The whole apparatus is enclosed in an outer cylinder CC.

The doubly bent glass tube II connects the vessel K within the calorimeter, and the vessel J without. It passes through a tight stuffing-box in the wall of the calorimeter, and through a perforated

* Poggendorff's Annalen, vol. cxli. p. 1. 1870.

cork in the wall of the vessel C; it is formed of two pieces, which can be disconnected at L, so as to allow of the removal of the calorimeter from the jacket. The calorimeter A is to be filled with ice and water, both free from air; the tubes EE and FF supply the gases (previously cooled to 0° C.) necessary for the chemical



operation taking place in the laboratory B; while GG removes the products of combustion, those which condense collecting in H. The vessels J and K contain mercury, and it is obvious that the quantity of mercury transferred from the one to the other is the measure of the thermal change accompanying the chemical action. The space between the calorimeter and the jacket C is filled with melting ice.

The following Gentleman was elected a Fellow of the Society:—

Rev. Thomas Lindsay, M.A.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VII. 1870–71. No. 83.

Eighty-Eighth Session.

Monday, 16th January 1871.

Dr CHRISTISON, President, in the Chair.

At the request of the Council, Principal Sir Alex. Grant, Bart., delivered an address "On the Educational System of Prussia."

Mr President and Gentlemen,—If I were addressing almost any other assembly, I should probably begin by saying that the subject of the educational system of Prussia possesses a peculiar interest at the present moment for two reasons—1*st*, Because the wonderful successes of Prussia make one curious to know all the methods which have been applied to bring that nation to its present state; 2*dly*, Because public instruction is just now one of the chief questions of the day for the inhabitants of Great Britain and Ireland.

But in this Society considerations of the temporary and the contingent would be out of place. And therefore, omitting altogether such allusions, I propose to submit some account and estimate of the Prussian educational system merely as a sort of contribution to human natural history.

Probably no human institution is perfect, and yet I think we may see nature working in and by means of human societies towards constant improvement—that is, towards the best. While a large portion of mankind seem content to remain stationary.

without any desire for progress, there have always been progressive races who have respectively devoted themselves to working out different problems of civilisation. Among these is the problem of national education, for the working out of which Prussia has made great, and, as it is generally thought, successful efforts. At all events, she has accumulated so great a mass of experience on the subject, as to make the history of her efforts worthy of being studied.

It is a common, but erroneous, notion to suppose that education in Prussia is the product of the arbitrary will of modern despotic governments—that it was conceived as a whole by some Minister of Instruction, drawn out on the foolscap paper of a bureau, and then issued by the *fiat* of the State to be accepted by the people. Such an account would be as far as possible from historical truth. But some notion of the kind has obtained currency, perhaps partly under the authority of M. Cousin, who visited Prussia in 1831, and made a report on the state of education there for the French Government. His account of the primary educational system was translated by Mrs Austin, and so became tolerably well known in this country. M. Cousin got hold of a scheme for the organisation of education throughout Prussia, which had been drawn up in 1819 by Von Altenstein, then Minister of Instruction. Viewing matters rather superficially, Cousin referred all he saw to this scheme, as if it had been the cause and origin of the school system which he found. But the fact is that Von Altenstein's document was merely what we would call a "draft bill." It was never carried in the Chambers, and never became law, and it had no more influence on education in Prussia than the several abortive bills for education in Scotland have had on our parochial schools. The curious thing is that Prussia, up to the present day, has never had a substantive Educational Act. Several bills have been drawn up, as for instance in 1819, in 1850, and in 1869, but they have always been ultimately rejected. And the Liberals in Germany are looking forward to the actual passing of an educational law, after more than fifty years of unsuccessful attempts at legislation in this department, as one of the first internal results which will be achieved after the conclusion of the present war.

It is true that the administration of public instruction in Prussia is bureaucratic in the extreme; but this is not the same as saying

that the educational system has been created in a bureau. The schools grew up in accordance with the ideas of the people; the character of the schools has been modified from time to time by public opinion; till within the last sixteen years the schools varied according to the difference of the different provinces; in short, the central Government has only gradually and lately got its grasp on that which it found, but did not create.

The *Volksschulen*, or people's schools, in Prussia were in the outset a product of the Reformation. The great characteristic of Prussian popular education is universality of school attendance under legal compulsion. Now, the legal compulsion is of comparatively late introduction. It was only brought in after the sending of children to school had long been recognised as a religious duty incumbent on all, and had thoroughly become a habit of the people. Just as John Knox was the author of the parochial school system of Scotland, so Martin Luther was the author of the universal school attendance of Germany. The custom dates from a circular letter which, in the year 1524, Luther addressed to the burgomasters and councillors of all the towns in Germany. It was a manly, earnest, powerful appeal, painting in strong colours the neglected condition of the children, and urging that schools should be provided for them. Luther pleaded that each child should go to school for at least two hours a day, giving the rest of its time, if absolutely necessary, to work. This letter had a striking and permanent effect. The town councils, the landowners, and the princes of Germany were stirred up to action; new schools were provided, and the old ones improved all over the country, and the people gradually took up the idea and never dropt it, that to send their children to school was a plain Christian duty.

At the beginning of the eighteenth century, in 1716, King Frederick William, issuing certain ordinances for the regulation of schools, assumes the universal attendance of unconfirmed persons; he merely gives his royal sanction to an existing practice. In 1763 an *Allgemeines Landschulreglement*, or general regulation for country schools, was issued, which for the first time defined the age of school attendance, namely, from five to fourteen. Thus the law was merely an expression, a ratification, and a definition of the custom of the people.

I will now mention the way in which the compulsion is carried out. Compulsory school attendance may be of two kinds—either (1) the parent may be obliged to show that the child is taught somewhere; or (2) the child may be compelled to attend a particular school for which it is registered. The second is, of course, the harsher and more bureaucratic method, and it is distinctively called *Schulzwang*, or school compulsion; while the first and milder obligation is *Schulpflichtichkeit*, or school duty. The second method, while leaving less liberty to the parent, is more efficient from the point of view of the State; and as such it was adopted in Prussia in 1857, and is now the law of the kingdom. The police-office of each place makes out a list of children as they arrive at school age—that is, five years old. It registers each child for the school nearest its dwelling-place, and sends the list to the school board, which now becomes responsible for the child not only joining the school, but also regularly attending for the next eight years—that is, up to the time of its confirmation. The master keeps a register of attendances, and in some places it is the custom, after the first school hour, to send round a messenger to inquire after missing children and the reason of their absence. Each case of absence is marked by the master as "excused" or "unexcused." When unexcused absences occur, it becomes the duty of the clergyman, as chairman of the school board, or of some deputed member of the board, to use moral suasion with the parent or guardian, with the view of obtaining greater regularity. If these means fail, the name of the parent or guardian is sent to the police-office, and he is mulcted with a small fine for each unexcused absence, and, in case of non-payment, is sent to gaol. Mr Mark Pattison (from whose admirable report on the primary schools of Germany most of my details for this part of the subject are taken) mentions that in Berlin, in the year 1856, there were 1780 convictions for irregular attendance, being rather more than three per cent. on the whole number of children on the rolls of the schools. This was thought a very large proportion, and was attributed to the growth of pauperism, and consequent demoralisation in a large city. I am sorry that I have not more recent statistics to offer, but the system remains the same, and I think that we can see its general working.

In that same year, 1856, there were 2,943,251 children of school age in all the Prussian provinces. Of these, 2,828,692 were in attendance at elementary schools, public and private. Of the remainder, 114,559, many were in attendance at the lower classes of grammar schools and real schools, which are open to pupils of nine years of age; others were being educated at home; a few were doubtless invalids, or physically or mentally incapacitated; the residue, which must be small, represents the children of itinerating families who manage to escape getting upon any school register. Even if we suppose that 100,000 children escaped school attendance altogether, that would give less than three and a half per cent. on the entire population of school-going age. But the proportion for most of the provinces is nothing like so large. Out of the recruits that joined the Prussian army during the past year, it is true that exactly three and a-half per cent. of the troops had never had any schooling. But the great bulk of the unfavourable returns is made up of recruits from Posen, a Polish province which has been called "the Ireland of Prussia," and from the natives of East Prussia, whose vicinity to the frontier facilitates their evasion of school attendance. From the province of Brandenburg, only one-eighteenth per cent. of the recruits had not attended school.

On the whole, the law of compulsory attendance in Prussia may be said to be perfectly efficacious in producing the result at which it aims, and it appears to be very seldom complained of. Even in the political disturbances of 1848, this law was not put forward as one of the grievances against the Government. The law is thoroughly in harmony with popular custom; and just as in this country it is a matter of course for the well-to-do classes to send their children without any exception to school, so in Germany it is equally a matter of course for the peasant and the labourer to send off his children every morning to the school which the community has provided. Day schools throughout Germany (as in Edinburgh) are the rule for rich and poor alike, and there is an air of equality given by the spectacle of rich children, as well as poor, going off each day to their respective schools.

The *Schulzwang*, or compulsion to attend a particular school, is of course relaxed in favour of the rich. The parent applies for exemption, stating his reasons, and naming the school (generally a

private one) to which his child is to be sent. In some places he has to pay the school fee all the same to the school for which his child was registered. In two parts of Germany there used to be no law of compulsion, namely, in the free towns of Hamburg and Frankfort-on-the-Maine. Frankfort, however, has now become Prussian. It was said that in these places the attendance of children at school was quite as universal as in Prussia itself; and some persons argue that the custom of the people might be relied on everywhere in Germany, and the law dispensed with. But we have already seen that the growing pauperism of places like Berlin tends to invalidate the custom. The law, at all events, helps to keep the custom straight, else it might well be doubted whether the ideas of the sixteenth century as to the duty of school attendance could be kept alive in manufacturing centres, and in very poor neighbourhoods. In the agricultural districts, it is said that the farmers dislike schools because they raise wages; in manufacturing districts, the parents dislike schools because they deprive them of a certain amount of wages which their children might otherwise be earning. In the cotton manufacturing districts of Saxony, the Government has made an equitable compromise between the claims of industry and of school learning, by allowing a system of half-time schools for children employed in the factories. The children under this system appear to be ultimately as well instructed as those under a whole time system. I think that this experiment deserves particular attention. For I believe that children up to nine or ten years' old can learn as much in three hours per diem as they could learn in six hours per diem, and that light industrial tasks for the remainder of the day would rather tend to develope the intelligence of the child. In Prussia the minimum age for children being employed in a factory is twelve, and up to fourteen no child must work more than six hours per diem. Thus plenty of time is still left for attendance at a three hours' school.

We have now to consider the funds by which the elementary schools of Prussia are supported. There are very few endowments available for them. The Government has at its disposal for educational purposes about L.50,000 per annum, derived from sequestrated Church property, and from charitable bequests. But this is almost entirely devoted to higher education. The elementary

schools may be said, in a word, to be supported wholly by contributions from the annual income of the community, in the shape of —1st, school fees; 2d, local rate; 3d, general taxation. The first step towards providing for the maintenance of a *Volksschule* is, that the proper authorities of the *gemeinde*, or commune, register each family as assessed at a certain rate of school fees for any children that may be of school-going age. In this country there appears to be a sort of repugnance to the idea of a graded scale of fees in proportion to the income of parents. But in Prussia this is the first principle of public instruction. Fees are assessed upon families not in relation to the cost of the school, but solely in relation to the circumstances of those who are to pay the fees. Government, however, fixes a maximum and a minimum rate. No child is to pay more than fifteen thalers, or about forty-four shillings per annum; and the lowest rate (from which there would only be exemption in the case of extreme poverty) is one groschen, that is about three halfpence, per week. Between these extremes the assessment takes place.

The next source of revenue for the school consists in the collections made in the parish church during one Sunday in each year. Then there is a small capitation tax on poor and rich alike, and, finally, a rating on property, estimated by a loose valuation.

Grants from the general taxation of the country for elementary schools are only made in cases where the commune can show real inability, on account of the poverty of its inhabitants, to meet the necessary cost. The Government, however, has occasionally allowed grants for increasing schoolmasters' salaries. It is clear, then, that as the fees are almost always extremely low, the burden of maintaining the primary schools falls mainly upon the rate-payers. This principle was introduced by the *Allgemeines Landrecht*, or general code of Prussia of the year 1794, which lays down that "where there are no endowments for the support of the common schools, then the maintenance of the teacher falls upon the collective householders, without distinction of religion. The contributions requisite for this purpose, whether they be paid in money or kind, must be equitably divided among the householders, in the proportion of their property and holdings."

To show the working of this system in a large city, it may be

mentioned that in Berlin (which has about three times the population of Edinburgh) there were some time ago about 55,000 children in the elementary schools, and it was estimated that each of these children, in addition to the school fees, cost the municipality about L.1 sterling per annum,—the total expenditure on this object being about twelve per cent. on the municipal budget.

We have seen how the primary schools in Prussia are filled, and how they are supported; we have now to inquire how they are managed. The *Volksschule* has never forgotten the tradition of its origin, at the time of the Reformation, as an ecclesiastical institution. The immediate and local management of all the schools is practically in the hands of the clergy. The clergyman of the parish is *ex officio* local inspector of the common school. He is chairman of the school board, which consists of representatives of the householders. He has really onerous duties in connection with the school. He is expected to visit it constantly, in some places as often as once a week. He is not merely the inspector of the school in the sense of examiner and critic, but he is responsible for its management and superintendence. He has to prepare the children for confirmation by a religious lesson of at least an hour a day for the two or three months preceding Easter.

The central power is said to regard the clergy as useful in repressing the instinct of self-government in the commune. The clergy are said generally to take a bureaucratic and centralising point of view in the discharge of their functions as school inspectors. But they have a difficult and thankless office. They have to encounter the jealousy of the school board, and often the discontent and mutiny of the schoolmaster, who has, perhaps, the chronic grievance of an inadequate salary, and who, having been professionally prepared in a training college, finds himself controlled by one who has no technical acquaintance with the details of school management.

In the political disturbances of 1848–49 (which were designated as "the schoolmasters' rebellion"), one of the great cries was for the autonomy of schools, that is, for greater freedom from the control of the Church. And this is one of the things which the Prussian Liberals expect from the Educational Bill of the future. They do not seem to ask for a secular system of instruction, but

rather for emancipation from clerical management. The Government depends much on the moral influence of the clergy in promoting regular school attendance among the people, and generally in playing a conciliatory part in relation both to the school board and the master. In many cases the clergy appear to perform these offices in a most Christian and self-denying spirit. But, on the other hand, they appear frequently to fall into a state of apathy and indifference about the schools. Their labours, as school inspectors, are an unremunerated addition to their proper functions, and are such as often, individually, they have no taste for.

The present system is recommended by its cheapness, as under it school inspection costs nothing to the Government. But, on the whole, it can hardly be called successful, and it is probably doomed to alteration. It is not only the clergy themselves, who in many cases exhibit a want of interest in the schools, but the local communities also have their sympathies chilled, in the first place, by an over predominance of the clergy in school management, and, secondly, by the excessive interference of bureaucratic action from above. The nature of this bureaucratic action has now to be described.

The kingdom of Prussia is divided into provinces, each province into departments, each department into circles or districts, each circle into parishes or communes. For the whole kingdom, the central educational authority is, of course, the minister of public worship, and medical and educational affairs. Beneath him there is a gradually descending scale of officers, for the superintendence of instruction on the system that a civil authority is always associated with clerical or scholastic affairs. Thus for the province, the president of the province is associated with a provincial school council. For the department, the prefect of the department is associated with a departmental school councillor. For the circle or district, the landrath, or district councillor, is associated with the superintendent, who is an ecclesiastic of about the same dignity as an archdeacon in England, and who supervises the inspection of schools in from twenty to forty parishes. In the parish there is the school board associated with the local clergyman, who, as we have seen, is *ex officio* school inspector and school manager.

The provincial school council, in conjunction with the president of the province, manages higher education alone.

All reports on primary instruction are sent up by the superintendents of districts to the departmental school councillor, who, in conjunction with the prefect of the department, forwards them direct to the minister of instruction. The superintendent, though an ecclesiastic, is said to act invariably in a bureaucratic, and not a clerical spirit. It may easily be supposed that, with all this network of reports radiating towards the centre, there is little scope left for local action in the matter of the common schools. Though the rate-payers furnish the funds, they have little to say on their expenditure. The schoolmasters appear to be appointed, not by the parish school boards, but in each case by the departmental school councillor. For some time there was a certain liberty left to individual masters and to local feeling in the kind of teaching to be given in the schools; but, in 1854, certain famous *Regulative*, or Minutes of the Bureau of Public Instruction, were issued, absolutely defining the subjects and manner of teaching. Of these minutes I will speak presently. They gave final extinction to anything like local and characteristic life in connection with the country schools.

In large towns they have another board called the *Schul-deputation*, or school delegacy, for the collective management of the city schools. These bodies were first created in 1808, when, under Stein's advice, every possible means was being adopted for calling forth the energies of the nation, and, amongst other things, it was thought desirable to awaken municipal life. In Berlin, the school delegacy, consisting of chosen members of the town council, have the management of all the schools, both higher and primary, within the city, except a few which are of an exceptional character. But the school delegacy has to report to the provincial council of Brandenburg, and Mr Pattison mentions that on one occasion they were reproved for too much independence, for having examined some candidates as teachers in needle-work without having sought the permission of the provincial government. In short, the central power has of late evinced much jealousy of the school delegacies, and has apparently wished to take back, or neutralise, the dangerous concession of 1808.

In Prussia the so-called "religious difficulty" has never existed. The schools of every kind are religious and denominational. The religious difficulty arises from a multiplicity of sects, and from antagonism between established and non-established churches. But in Prussia there are three leading confessions, all endowed respectively in different localities, which cover almost the entire population,—the Lutheran, the Reformed, and the Catholic. The two first are conjoined for school purposes; and thus we have the denominational proportions of population stated some little time ago, as follows:—

Protestant	. .	64·64 per cent.
Catholic	. .	32·71 „
Other creeds	. .	2·65 „

Of these other creeds five-sixths were Jews, the remainder Dissenters—such as Baptists, Mennonites, Irvingites, &c. This phenomenon of more than ninety-seven per cent. of the population belonging to established churches may remind us of the case of Scotland, where, I believe, about eighty-eight per cent. of the population belong, if not to one establishment, at all events to one confession, without material doctrinal differences.

The Jews in Prussia, whenever congregated in sufficient numbers, have schools of their own, with their own religious teaching. If they exist in isolated families, their children attend the Christian schools, and are generally not withdrawn even from the religious teaching. They are said to look on instruction in Christianity as a piece of useful or curious information, and to be quite above the fear of conversion. In this respect they are like a certain Brahmin of Bengal, who, having attended a missionary school, reassured his caste by telling them that "he had gone through the whole Bible, and it had done him no harm."

The Dissenters are obliged to attend the public schools, but they are under the protection of a conscience clause. The authorities require evidence that the children of Dissenters are taught religion according to their own formulæ by their respective clergy. The Prussian constitution of 1851 contained the following article:—"In the ordering of public schools for the people, regard shall be had to denominational relations. The religious instruction in the people's school is under the conduct of the respective religious

bodies." The conscience clause dates back from the Prussian code of 1794, which lays down that "admittance into the public schools shall not be refused to any one on the ground of diversity of religious confession. Children whom the laws of the State allow to be brought up in any other religion than that which is being taught in the public school, cannot be compelled to attend the religious instruction given in the same." This order, however, except in the numerically insignificant case of the Dissenters, appears seldom to have been put in force. Mixed schools, where teachers of different confessions are associated together, have been tried occasionally, but have not been found successful. It has long been an established maxim in Prussia, that all schools must be denominational, and, as a rule, every child appears to find him or herself at a school belonging to his or her religious denomination.

The obstacles in the way of legislating for the instruction of the people in this country arise *in limine* from differences of opinion as to the questions of religious teaching, school management, rating, and compulsory attendance. The obstacles in the way of educational legislation in Prussia arise from differences of opinion as to the relation of Church and State to local communities. But in Prussia the difficulty is only about altering the character of a system. The system is there, and is complete enough in itself. The only question is, Could not a better and freer system be introduced? We have seen how the Prussian people, following the advice of Luther, adopted universal school attendance as a national habit; how this habit was ratified and confirmed by law in the eighteenth century; how the support of people's schools was thrown on the householders by the code of 1794; and how, by common consent, and by law, the schools have remained denominational, with a conscience clause for the benefit of a very small section of the population. Thus has Prussia, in the march of time, quietly stepped over all those preliminary and merely parliamentary difficulties, which in this country have so long prevented large numbers of the people from getting any school education at all, while Lords and Commons have been wrangling as to the exact form under which the schools were to be started.

But all this touches merely the external politics of public instruction. The question remains, What is the teaching in the

people's school when you have got it established? On this point the experience of Prussia is not uninteresting. The elementary school in Prussia was, in its origin, a catechetical instruction; it was a repetition by some subordinate ecclesiastic of the Sunday catechising of the pastor. Gradually the teaching of reading and singing was added, but only as a means to a religious end, namely, reading the Bible and singing in church. By the middle of the eighteenth century more secular elements of instruction were grafted on; and Frederick II., in 1763, orders that "the people shall be Christianly brought up in reading, praying, chanting, writing and arithmetic, catechism, and Bible history. The Prussian code of 1794 lays down that schools and universities are "institutions of the State." It prescribes the teaching of religion as a part of useful knowledge, and as tending to make good and obedient citizens. At the end of the last century the Prussian elementary schools appear to have been easy-going mechanical institutions, with nothing about them specially to call for remark. But an immense ferment in relation to them was preparing, a passionate upstirring of the whole question of popular education, endless theory and counter theory, action and reaction, the history of which constitutes a whole literature, and the effects of which have all been felt upon the character of the Prussian *Volksschulen*, which now remain like the fossilised result and record of the storms of the past.

All this commotion rose from the fervid brain and heart of one man, Henry Pestalozzi, a Swiss, who was born at Zurich in 1746. Pestalozzi was a loving enthusiast; of a most unpractical turn of mind; always embarking in visionary schemes for the good of others; of a large and noble heart, living a life of poverty and struggle himself, but always spending his whole strength in efforts for the welfare of the poor. He lived to be eighty-one years old, and long before his death he had been publicly visited and honoured by emperors, kings, and statesmen, and had seen his ideas warmly received and widely spread over the continent of Europe. Pestalozzi was much influenced in early youth by reading the "Emile" of Rousseau. In 1780 and subsequent years, after many failures in life, he began to bring out books on education. The chief of these were, "The Evening Hour of a Hermit," con-

taining educational and religious aphorisms; and "Leonard and Gertrude," a story to illustrate what might be done by a particular method of teaching children. These and other writings of his excited great attention. He had successively different schools under his management, in which he developed his system by practical experiment. Finally, at Yverdun, in the year 1805, he had obtained care of an institution which has now become a classical name in the history of pedagogy.

Pestalozzi's fundamental idea was that the children of the poor. in a public school, should be taught as if by an affectionate mother, who entered into all their feelings, and anticipated their difficulties. His conception was that primary instruction should not consist in giving knowledge verbally, mechanically, or by rote, but in drawing out the powers of the child. He laid it down that no child should be taught anything which it could not understand. The first development of this idea resulted in lessons upon form, number, and language. At Yverdun, Pestalozzi would carry his class through a lesson of the following kind:—Pointing to the wall, he would say,—

"Boys, what do you see?"

(*Answer*) "A hole in the wainscot."

"Very good; now repeat after me—

"I see a hole in the wainscot.

"I see a long hole in the wainscot.

"Through the hole I see the wall.

"Through the long narrow hole I see the wall.

"I see figures on the paperhangings.

"I see black figures on the paperhangings.

"I see round black figures on the paperhangings.

"I see a square yellow figure on the paperhangings.

"Beside the square yellow figure, I see a black round figure.

"The square figure is joined to the round one by a thick black stroke." And so on.

It was said that Pestalozzi used to shout out sentences of this kind without any explanation, and was echoed in chorus by the class. It is true that words in this way became associated with impressions of the sense. But if this were all, we should say that Pestalozzi was incapable of developing his own theoretical idea

A trace of such teaching reached this country in the shape of the so-called "object lessons," which, without much fruit, were once in vogue in England.

But the Pestalozzian method had in reality far greater results. A swarm of enthusiastic assistants, perhaps more clear-headed than their master, came to serve under him; and by them there was worked out—

(1.) All sorts of methods for conveying in an easy manner to the child the arts of spelling, reading, ciphering, and so on.

(2.) The practice of a sort of Socratic dialogue, for developing the intelligence of the class upon the subject of the lesson, whatever it might be.

(3.) The idea of pedagogy as a science, based upon psychological *data*.

(4.) The idea that religion, which with Pestalozzi was made the basis of all, must not be taught dogmatically and confessionally, but rather universally; in short, that the first teachings must be of natural religion, and not of the religion of any Church.

All this was new, and it had a peculiar fascination for several of the greatest minds of the age. When, in 1806, Prussia was crushed by Napoleon, and went through afflictions strikingly analogous to those that have now befallen France, Stein and Fichte, the statesman and the philosopher, both earnestly proclaimed that the moral energies of the nation must be regenerated by the universal adoption of the Pestalozzian ideas. Pestalozzian schools were established over the country, and in subsequent years the system was thoroughly exploited; all its strength and weakness were brought to the full light of trial and experience.

The result of fifty years' exhibition and discussion of the Pestalozzian system has been as follows:—

(1.) There is a considerable residuum in the shape of excellent technical methods for teaching the elements of knowledge. Thus each child is taught to read easily, alone, within twelve months. The old plan of first learning the names of the letters, and then spelling, is abandoned. In arithmetic, the child is taken through the operations of the four rules, both in integers and fractions in the tens, before he reaches the hundreds. The magnitudes to be dealt with form the only distinction between the classes in arith-

metic. These and other methods are the result of the immense attention which has been bestowed on the question of primary teaching.

(2.) Public opinion has pronounced against much that was characteristic of the Pestalozzian system. From the principle that children should be taught nothing that they could not understand, there was deduced the practice of much abstract and formal lecturing, totally unsuited to children from six to nine years of age. Thus, lessons on the theory of number were made to precede empirical teaching of arithmetic. While much stilted talk was used both about the children and to the children, it was found that, in many cases, they were suffered to go through school without learning to read and write. A general reaction set in against the idea of intellectual training in common schools.

(3.) This tendency of public opinion was taken up and ratified by the Government. In October 1854, *Regulative*, or Minutes from the Office of Public Instruction in Berlin, were issued, which bear a close analogy in some points to the revised code of Mr Lowe. The object of these minutes was to restrict the teaching in elementary schools to a few humble and necessary subjects, and to ensure these subjects being efficiently taught. In direct opposition to Pestalozzi, the *Regulative* proceeded on the principle that, in an elementary school, it is *not* the object to develope the child's reasoning faculties, or to give him knowledge, but only to give him the power of doing certain things;—*Können*, and not *wissen*, was to be the result to be produced. The schools were to turn out the children in possession of the actual capacities (*fertigkeiten*) of reading, writing, and ordinary ciphering, and everything outside of this range was to be sternly excluded. Thus the children were on no account to learn grammar, as this is an abstract, logical thing, suited to the high school; whereas, in an elementary school, children should learn to use their own language correctly by practice, and not by rules. Even mental arithmetic was to be excluded, as being a needless fatigue of the brain. Of secular subjects, in addition to the three R-s, only singing was as a general rule to be taught, for the sake of practising the voice and ear. Only church tunes and national songs were to be permitted, the words being previously well studied and explained. History and geography were

discouraged; if taught at all, they must be limited to *Heimathskunde*, or information about the child's native land. Drawing, if introduced, must be confined to linear freehand copying from the flat.

Religion remained an essential and prominent element for the people's schools, but the *Regulative* made a great change in regard to the mode of imparting it. Under the Pestalozzian system, religion had been taught not confessionally, but universally; not as a matter of Church formulæ, but in a free and spiritual way, which, of course, depended for its characteristics very much on the individual master. When the time for confirmation arrived, the clergyman would find the children furnished with ideas, more or less orthodox, of natural religion and of Christianity, but perhaps never having seen the Church Catechism, and the labour would devolve on him of making them learn this. It appeared to the Government that the schools, though denominational in their foundation, were too independent of the Church in their religious teaching. The *Regulative*, by one stroke, altered all this. They laid down exactly what was to be taught in the shape of religion, namely, some fifty hymns were to be learnt by heart, the whole of the gospel portions which are read in the Lutheran churches were to be committed to memory, and the Catechism (either Luther's or the Heidelberg) was to be learned off by rote, without any explanation. All explanation of the doctrine contained in it was to be reserved for the pastor, when the time of confirmation drew nigh. By these rules, the relative positions of the clergyman and the schoolmaster were completely subverted. All the charm of teaching religion to the children was taken away from the master, whose task was, in this respect, made mechanical, while he himself was made completely subordinate to the clergyman.

The minutes on religious teaching had, doubtless, a political and ecclesiastical motive, and a reaction against them is possibly in preparation. Those regulating the secular subjects in the people's schools are a specimen of the Prussian Government, as a powerful decisive will, proposing to itself certain definite ends, and going straight at these ends without compromise or collateral considerations.

In the case of the elementary schools, there can be no doubt that the end aimed at is attained; for the schools embrace the entire population, and the result is, that the children of every

peasant and labourer have, as a matter of course, the arts of reading, writing, and cyphering, know the Church formulæ and a good deal of the Bible, and can take part in singing a hymn or national chorus.

But I think that one misses in these schools anything calculated to raise the intelligence of the people, anything analogous to the influence of the parochial schools of Scotland. The repression of the high-flown Pestalozzian aspirations has been too absolute. The definition of an elementary school has been too logical. There is nothing to lead on towards the higher grades of education. The people's school seems sharply separated off, and to give the children of the people no encouragement or opportunity to rise. One proof of this may be found in the fact that pupils who, at fourteen years of age, have passed eight years in the primary school, and who then have two years further preparation under a public schoolmaster or clergyman, are, at sixteen years of age, commonly unfit to enter upon the very simple curriculum of the training college.

It may be asked whether industrial or technical instruction does not form part of the Prussian system? But in the ordinary people's school nothing of this kind is attempted. The Prussian Educational Department conceives that it has a particular function to discharge for the people, and of this it acquits itself, and does no more. It is argued that seven or eight years' schooling, at the rate of twenty-six hours per week, is not more than sufficient for imparting to all with certainty the elements of common knowledge and religion, and that any attempt at technical instruction would only interfere with this; and everything technical must be learnt practically, or otherwise, after the age of fourteen. One means of supplementing the meagre results of the people's schools, consists in the *Fort-bildungsanstalten*, or "improvement schools." These exist generally in the shape of evening classes in mathematics, French, &c., for youths and adults. They have not been organised systematically, and even if they were, could hardly supply the want of a more early awakening of the intellect.

But, of course, many children, and some even of the poor, quit the elementary school at nine years of age, to enter on the course of higher instruction.

In all the departments of higher instruction, Prussia seems to me to be distinctly ahead of England, and still more so of Scotland. But I have already take up so much of your time, that I must now confine myself to a few aphorisms on this subject. In Prussia education is considered to be so completely a matter of national concern, as always to call for the supervision of the State. No man may start a private school, whether primary, middle, or higher, without a license from the educational office. And this license is only given after the passing of prescribed examinations. The too common charlatanry of private schoolmasters in England is thus avoided. A useful censorship of schoolbooks is exercised by the minister of instruction. By this the crotchets of schoolmasters in the use of eccentric and useless books are checked.

The minister of instruction is not only a man of science or learning himself, but he has the advice of councillors of the highest scientific and literary reputation. The opinions of such a central board on questions of higher instruction are not merely bureaucratic edicts, but constitute a valuable intellectual guidance.

With regard to resources, the following distinction is to be observed in Prussia. The elementary schools get very little money from Government, only a small contribution from school fees, and the great bulk of their expenses from parish and municipal rating. The support of the higher schools of all kinds appear to be as follows:—

From Fees, a proportion of . .	5·4
From Municipal assignments, . .	2
From Grants by Government, . .	1·6
From Endowments, . . .	1·

Thus the fees of scholars pay considerably more than half the cost of the higher schools. Municipal contributions amount to one-fifth, and grants from general taxation to nearly one-fifth, endowments to one-tenth. Fees in the high schools are often remitted wholly or partially on the ground of the circumstances of the parents. Out of about 90,000 scholars attending the superior schools of Prussia, about 20,000 appear to be wholly or partially free scholars.

The higher education goes in Prussia, the more entirely does it

become recognised as a proper object for State maintenance. Thus the universities, so far as their own resources fall short, are fully supplied by the Government. The University of Berlin, in the year 1864, had an income of about L.30,000. Of this, L.24 only was the interest of funded property of the University; L.1133 was the amount of entrance and examination fees; L.28,842 was the grant from Government.

If we compare with this the University of Edinburgh, we find the income for the current year to be L.20,351, of which L.4153 are fees of various kinds, L.9869 funds from private endowments and other sources in the hands of the Senatus, L.6329 parliamentary grants. This shows how comparatively small is the proportion of State assistance to our University.

The higher schools of Prussia consist of two distinct branches—the *Gymnasien*, or grammar schools, with their *Pro-Gymnasien*, or preparatory grammar schools, and the *Real-schulen*, or scientific schools, with the "higher burgher schools" in preparation for them. The *Gymnasien* are, of course, the product of the Middle Ages, the Renaissance, and the Reformation. The *Real-schulen* sprang from the modern protest on behalf of science against the predominant claims of classics. The Gymnasium is a first-rate classical day school, with a time-table of 30 hours per week. It has six classes, *Prima* being the highest. The 30 hours in *Prima* are thus allotted:—Religion, 2; German, 3; Latin, 8; Greek, 6; French, 2; History and Geography, 3; Mathematics, 4; Physics, 2. Besides these school hours there is extra-time instruction in singing and gymnastics; and those who propose subsequently to study theology or philology in the University are required to learn Hebrew, also in extra hours.

The time-table, though thus definitely prescribed, is not rigidly adhered to; for promising pupils in the first class are allowed a good deal of liberty for private study in lieu of the stated lessons.

Many enter the Gymnasium irrespective of an intention to proceed to the University, for the sake of the privileges which it holds out. For, those who have gone through the classes and passed the leaving examination, besides qualifying for the public service, are allowed to serve for one year as volunteers in the army, instead of three years according to the ordinary course.

But yet it is endeavoured to keep up a thoroughly intellectual atmosphere in the *Gymnasiens*. The Prussian Government lays it down that culture for its own sake, and not with any premature regard to the practical exigencies of life, is to be the object of these schools. And it expressly forbids that those who propose to enter the army as a profession, should abate any of the higher classical studies of the first class. This is certainly very different from the principle adopted in English public schools.

The crowning result, and the most distinctive feature of the *Gymnasium* is the *abiturienten-examen*, or leaving examination. The certificate of having passed this examination is, of course, ardently desired by the pupils, as it is the key to entry into any of the learned professions, and gives important exemption in military service. This being the case, it may be affirmed that in this country an analogous examination would often lead to over-strenuous preparation on the part of the pupils when the time of the examination drew nigh. But the Prussian Government takes the greatest care to obviate a result which they would deem utterly unsatisfactory. They lay down the strictest rules, both in general terms and in detail, to prevent the examination being of a kind for which any special preparation, spasmodic efforts, or cram would be of any avail. It is by no means to turn upon the learning up of names, dates, and isolated facts; but it is to exhibit (as the educational minute says) "the slowly ripened fruit of a regular and contant industry throughout the whole school course."

With this object, one of the grounds for the certificate is made to consist in a record of the pupil's work throughout perhaps the nine previous years in all the classes of the Gymnasium from *sexta* to *prima*. In addition to this, the examination is to show how much of the school study has really been assimilated by the pupil, and has become part of himself. The Prussians are much wiser than some other countries in the matter of examinations. They always keep in view the exact end they are aiming at. In the *abiturienten-examen* they don't want a paper, but a man; and they certainly adopt the best means of testing the man's real acquirements and deserts, when, on the one hand, the examiners have before them a continuous record of his previous work for years, and, on the other hand, submit him to such general exercises in

languages and mathematics as show in each subject what amount of proficiency he has really available. The examiners consist of the upper masters of the school itself, with certain commissioners from the Government associated with then. Persons who have been brought up in private high schools, and who wish to proceed to the University, must present themselves at the examination of the Gymnasium, where they will be equitably examined. But on the whole the public schools are most popular in Prussia, and the scholars of private schools are quite in a minority. The paper work of the examination occupies a week. The chief subjects are —(1.) An essay in German, which is intended to exhibit general culture, taste, and correct writing. It is analogous to the English composition in the Indian Civil Service competition. (2.) A Latin essay. (3.) A piece of simple Greek prose to be written. (4.) A translation of German into French. (5.) Two geometrical and two arithmetical problems to be solved. A *viva voce* examination follows, consisting of translation from pieces, not prepared in class, of the Latin and Greek authors, questions in metre, mythology, history combined with geography, and antiquities; conversation in Latin; examination in Bible history and the Church Catechism; and for future philologists and theologians, an examination in Hebrew.

The certificate which each candidate receives is marked either "insufficient," "sufficient," "good," or "excellent." The mark "insufficient" is meant to indicate unripeness for the University. The pupil receiving it is recommended to prolong his attendance at school, or to seek some other career in life for which University study is not required. But if he and his parents wish it, he may still enter the University, with his certificate of "unripeness." In that case he will be restricted to the faculty of philosophy, and not allowed to enter any learned profession, unless he can, by subsequently presenting himself at the gymnasial examination, obtain a certificate of being "ripe;" and in the meantime he will be debarred from holding any University scholarships or stipends. The holders of favourable certificates, with "good" or "excellent" for their examination, and a full record of previous conduct and performances, carry with them an important testimonial for the outset of life.

In all these arrangements of the leaving examination of high schools, we see, I think, that Prussia dares to be thorough in a matter of this kind. She insists that high schools should do their work, and by giving the universities, the public service, and the learned professions an organic connection with these schools, she makes it a very serious matter for all the pupils to take advantage of their opportunities. Without any apparent strain upon the pupils, she succeeds in obtaining a higher standard of results from school boys than is implied in the ordinary M.A. degree of the Scotch universities, or the ordinary B.A. degree of Oxford or Cambridge.

Of the *Real-schulen*, or scientific schools, I have not much to say. Started originally more than a hundred years ago, it is only within the last fifty years that they have had a considerable development. Of the 90,000 pupils attendant on secondary schools in Prussia, about 30,000 appear to go to the *Real-schulen* or their preparatories. These schools do not prepare for the universities, but for business, certain departments of the public service (such as architecture or mining), and for the Polytechnic College.

The time-table for *Prima* in a *Real-schule* consists of thirty-two hours, made up as follows:—Religion, 2; German, 3; Latin, 3; French, 4; English, 3; Geography and History, 3; Natural Sciences, 6; Mathematics, 5; Drawing, 3. Latin, however, is not insisted on, and a liberty is left to the school delegacy of adjusting the subjects in some degree to the necessities of the immediate neighbourhood, with reference either to particular languages or particular industries, that may exist. A suitable leaving examination is prescribed, qualifying the holders of certificates for military exemption and for the public service.

An eminent authority, Dr Jäger, told Dr Matthew Arnold that the *Real-schulen* were not considered successful institutions. He said that the boys in corresponding classes of the classical schools beat the *Real-schule* boys in subjects which both do alike, such as history, geography, German, and even French, on which the *Real-schule* boys spend much more time. Dr Jäger assigned as the cause for this result that classical training strengthens a boy's mind more than modern or scientific teaching. I confess, however, that I think the comparison, as stated, not quite complete,

as in matters not connected with language and history the *Realschule* boys might be found to have faculties of observation and deduction to which the classical boys would be strangers. I merely state what has been said.

Turning now to the universities of Prussia, we find ourselves in the region of pure unfettered science. The *abiturienten-examen* of the classical schools gives the universities such a starting ground in the thorough previous education of all the students who matriculate, that they are able to commence the treatment of all subjects on a high scientific level, in confidence that such a mode of treatment will be followed and understood.

The appointments of professors are invariably made, so far as I can learn, on the grounds of greatest scientific eminence. The appointments are all in the hands of the Crown—that is, of the minister of instruction. When a vacancy occurs, the faculty to which the chair belongs sends up a short leet of names to be recommended to the minister, and from these he generally makes the appointment. But I believe that the name chosen is always that of the man whom previous public performances and general opinion in the scientific world have designated for the place. I believe that anything like political or theological bias in the appointment of professors is unheard of. Other personal considerations (which might be more plausibly entertained) are also omitted, such as power of clear exposition and capacity for managing a class. Hence it may happen that the professor, when appointed, is obscure in style and unattractive as a lecturer; but the students have, at all events, the feeling that in him they have the greatest authority that could be found on the particular subject. And there is in German universities a general consciousness that it is better to have the last and most reliable results in science than to have a popular exposition of what is old and perhaps exploded. The professor has a fixed salary from Government, frequently amounting to L.350 or L.400 a year, in addition to a share of examination fees and the fees of his class. But he is bound to lecture free of charge twice a week. The fees in theology or philosophy are about 17s. for the six months. In the medical classes they go as high as L.1, 14s. 5d. for the course. Several professors have altogether an income of from L.1000 to L.1500 a year, which, in proportion to

ordinary rates of expenditure in Germany, is something considerable. We all know that the headmasters of Eton and Rugby realise L.4000 or L.5000 per annum, which is probably superior to most university emoluments within the United Kingdom. But nothing of the kind occurs in Prussia; the highest schoolmasterships are below, both in rank and emolument, the ordinary run of professorships. The best school appointment in Prussia appears to be the rectorship of the *Schul-Pforta*, an endowed gymnasium in Prussian Saxony; to this L.300 per annum and a house are attached. The professors, being fairly endowed by Government, are far from being sheltered from competition by any kind of monopoly. The State can always appoint any eminent man as full professor, even in a faculty which has already its full complement. Then, secondly, the State at its pleasure appoints extraordinary or assistant professors, who have a small salary, their chief reliance being on fees. Thirdly, the Faculties appoint as *Privat-docenten* persons who can prove their fitness. The *Privat-docenten* appear not to fulfil the functions of what we should call tutors, but rather to be analogous to our extra-academical lecturers in the Medical Faculty. The *Privat-docenten* and the extraordinary professors form a reserve of men, establishing their reputations, from whom the future full professors will be chosen. Before the beginning of the session a harmonious arrangement is made between the professors, extraordinary professors, and *Privat-docenten*, in a Faculty, as to the subjects on which each is to lecture, so as to cover the whole field of instruction proper to the Faculty. The dean then publishes the programme, and the only restriction is that the fees must be uniform.

There is, in short, absolute liberty of teaching to those who can prove their competent knowledge of any subject; and there is equal liberty of learning, for no student is obliged to attend any particular courses, or number of lectures, with a view to his degree. All that general culture which we endeavour to ensure by our Arts curriculum is provided in Prussia beforehand by the *abiturienten-examen*, and the student is considered fit to choose absolutely for himself his own University curriculum. In the professional Faculties he, of course, cannot dispense with instruction in all the separate branches; but in the Faculty of Philosophy, which answers to our

Faculty of Arts, and embraces the humanities and the mathematical and natural sciences, the student is allowed to choose any two subjects he likes for his final examination; and if he passes in these, he gets his degree as Doctor of Philosophy. To pass, however, in any subject is supposed to imply, not a schoolboy preparation, but a manly mastery of the whole subject. For instance, in order to pass in Greek and Latin philology a student would be called on to revise the readings in some Greek or Latin book, with scholarly reasons for all his opinions on each point, and, in addition, to show, *viva voce*, a complete knowledge of classical literature, philology, and antiquities. The liberty allowed to students is doubtless often abused. In a recent life of the Count von Bismarck it is mentioned that, while attending the University of Berlin, he fought innumerable duels, and only attended one lecture. That lecture was by the eminent Professor Savigny; but Bismarck, thinking that he did not gain within the hour as much information as would suit his purposes, abandoned the course, and applied himself to a *repetitor* or crammer, by whose assistance he succeeded in passing the examination of the Law Faculty.

On the whole, there is probably not so much industry among the students of a German as of a Scotch University; but there is far more than at Oxford or Cambridge. And whenever industry exists, being based on more complete previous preparation, and being in relation to really scientific lectures, it is probably of a higher and more fruitful kind than can be found among the students of Great Britain.

Still, complaints are made against the Prussian university system. One of these is, that the students are too exclusively engaged in taking notes of lectures, and that they have too little practice of their creative faculties. The prejudicial effects of this may, perhaps, be traced in the want of the graces of style which characterises to so great an extent most German books.

Another complaint is, that the students, though systematically prepared up to entrance into the university, are afterwards left without sufficient guidance as to the order in which they should take up successive subjects.

It is quite possible that Prussia, which honestly and thoroughly desires the best in education, may descend a little from the clouds

in its university system, and deign to adopt something like the Little-go or Moderations examination of the English universities, though such an examination in Prussia would be, of course, on a distinctly higher level. Prussia might, perhaps, with advantage curtail a little the liberty of her universities, and increase a little the liberty of her primary schools, in respect both of studies and management. She might allow a more easy and natural connection than appears to exist between the primary school and higher education. She would like also to see a gradual relaxing of the leading strings of Government, and a greater development of cultivated local energies. It would be a great misfortune for the newborn German empire if military successes should be found to have intensified the centralising forces in all the affairs of national life. The Liberals appear sanguine that this will not be the case. But a struggle on questions of internal policy may very likely succeed the conflicts of the war. In the meanwhile, on the educational question Germany and England hold positions the very opposite of each other. In Germany there is the idea of what is wanted, and a universal carrying out of that idea. But too much comes from the central power. There is a deficiency of communal life and independent individual action. The question with Germany is how to shift, without losing, the motive power. In England there is abundant local action and vitality, but a deficiency in cultivated guidance for that action. There is with us an immense leeway to make up, both in overtaking, with primary instruction the masses of the people, and also quite as much in regulating and defining the aims and the method of secondary and university education. The great question for England in this matter seems to be, first, how to get over religious difficulties in the way of primary instruction; and, secondly, how to obtain a sufficiently enlightened guidance for our higher education, without adopting, which all ought to deprecate, anything like a bureaucratic system.

On the Physiology of Wings: being an Analysis of the Movements by which Flight is produced in the Insect, Bat, and Bird. By James Bell Pettigrew, M.D., F.R.S. Communicated by Professor Turner.

(*Abstract.*)

(Received 2d August 1870.)

In the present memoir the author enters very fully into *the figure-of-8 wave movements*, described by the wing in space, to which he first directed attention in March 1867.* He has adduced the experiments with *natural* and *artificial wings*, on which his description was originally based, and has shown, by the aid of original models and a large number of diagrams and drawings, that *artificial wings* can be made to approach indefinitely near to *natural ones*, not only in their structure, but also in their movements. He further points out that the fins and tail of the fish—the flippers and caudal extremity of the whale, dugong, manatee, and porpoise, and the flippers of the seal, sea bear, walrus, and turtle—bear a close analogy to wings, and ought to be studied in connection with them. As further proof that the wing describes a figure-of-8 wave-track in flight, the author cites the results announced in February 1869 by Professor J. E. Marey, of Paris.†

* *Vide* "The Various Modes of Flight in Relation to Aëronautics;" by the Author in the "Proceedings of the Royal Institution of Great Britain for March 22, 1867;" also his memoir "On the Mechanical Appliances by which Flight is attained in the Animal Kingdom," read to the Linnean Society of London on the 6th and 20th of June 1867, and published *in extenso* in the 26th volume of their Transactions, a large number of woodcuts and engravings being specially devoted to the elucidation of the figure-of-8 wave track made by the wing as observed in the flight of the insect, bat, and bird.

† "Revue des Cours Scientifiques de la France et de l'Etranger." Professor Marey, in a letter addressed to the French Academy, under date May 16, 1870, fully acknowledges the author's claim to priority (as regards himself) in the discovery of *the figure-of-8 wave movements made by the wing in flying*. M. Marey, in the letter referred to, states ("Comptes Rendus," page 1093, May 16, 1870), "J'ai constaté qu' effectivement M. Pettigrew a vu avant moi, et représenté dans son Mémoire, la forme en 8 du parcours de l'aile de l'insecte: que la méthode optique à laquelle j'avais recours est à peu près identique à la sienne . . . . je m' empresse de satisfaire à cette demande légitime, et je laisse entièrement la priorité sur moi, à M. Pettigrew relativement à la question ainsi restreinte."

Professor Marey, by employing a sphygmograph similar to that used for ascertaining the state of the pulse, succeeded in causing the wings of insects and birds to register their own movements. He says:—"But if the frequency of the movements of "the wing vary, the *form* does not vary. It is invariably the same; "it is always *a double loop, a figure of* 8. Whether this figure be "more or less apparent, whether its branches be more or less equal, "matters little; it exists, and an attentive examination will not fail "to reveal it." *

The subjoined are a few of the results obtained by the author in the course of his numerous observations and experiments:—

The wing is of a generally triangular form. It is finely graduated, and tapers from the root towards the tip, and from the anterior margin towards the posterior margin. It is likewise slightly twisted upon itself, and this remark holds true also of the primary or rowing feathers of the wing of the bird. The wing is convex above and concave below, this shape, and the fact that in flight the wing is carried obliquely forward like a kite, enabling it to penetrate the air with its dorsal surface during the up stroke, and to seize it with its ventral one alike during the down and up strokes. The same remark applies to the *remiges* or rowing feathers of the wing of the bird.

The wing is moveable in all its parts; it is also elastic. Its power of changing form enables it to be wielded intelligently, even to its extremity; its elasticity prevents shock, and contributes to its continued play. The wing of the insect is usually in one piece,† that of the bat and bird always in several. The curtain of the wing is continuous in the bat, because of a delicate elastic membrane which extends between the fingers of the hand and along the arm; that of the bird is non-continuous, owing to the presence of feathers, which open and close like so many valves during the up and down strokes.

The posterior margin of the wing of the insect, bat, and bird, is rotated *downwards and forwards* during extension, and *upwards*

* Revue des Cours Scientifiques de la France et de l'Etranger, p. 252. 20th March 1869.

† The wings of the beetles are jointed, so that they can be folded up beneath the elytra or wing cases.

and backwards during flexion. The wing during its vibration descends farther below the body than it rises above it. This is necessary for elevating purposes.

The distal portion of the posterior margin of the wing of the insect is twisted in a downward and *forward direction* at the end of the down stroke, whereas, at the end of the up stroke it is twisted downwards and *backwards*. The proximal portion of the posterior margin always assumes a reverse position to that occupied by the distal portion, so that the posterior and anterior margins of the wing are not in the same plane, and in certain situations the two margins appear to cross each other. What is here said of the insect's wing applies equally to the wings of the bat and bird.

The wing during its vibrations *twists* and *untwists*, so that it acts as a reversing reciprocating screw. The wing is consequently a screw *structurally* and *functionally*.

The blur or impression produced on the eye by the rapidly oscillating wing *is twisted upon itself*, and resembles the blade of an ordinary screw propeller.

The twisted configuration of the wing and its screwing action are due to the presence of *figure-of-*8 *looped curves* on its anterior and posterior margins; these curves, when the wing is vibrating, reversing and reciprocating in such a manner as to make the wing change form in all its parts. The curves in question are produced to a great extent by vital movements, independently alike of the elasticity of the wing and the reaction of the air. They can, however, be produced by the latter agencies likewise. The change and reversal of the curves occurring on the anterior and posterior margins cause the different portions of the wing to strike at various angles during the down and up strokes.

The angles which the different parts of the wing make with the horizon are greatest towards the root, and least towards the tip of the wing. The angles are, in fact, adjusted to the speed at which the different portions of the wing travel—a large angle with a low speed giving the same amount of buoying and propelling power as a small angle with a high speed.

The speed attained by the tip of the wing is always very much higher than that attained by those portions nearer the root—the

root corresponding to the *short* axis of rotation. (The *long* axis of rotation runs along the anterior margin of the wing.)

The angles which the wing makes with the horizon are increased during the down stroke, and decreased during the up stroke, the posterior margin of the wing being screwed down upon the air during the down stroke to increase the elevating and propelling power of the wing, and unscrewed or withdrawn from the air during the up stroke to afford support, and assist in propulsion.

The wing, in virtue of the variations of inclination of different parts of its surface, acts as a true kite during both the down and up strokes, *i.e.*, it flies down and up alternately in such a manner as to keep its ventral concave or biting surface always closely applied to the air. The wing is, therefore, effective during both the *down* and *up strokes*, so that it is a mistake to regard the down stroke as alone contributing to flight. In reality the down and up strokes are parts of one movement, the wing describing first a looped and then a wave track.

The tip of the wing in especial acts as a kite during the up stroke, the kite being inclined upwards, forwards, and outwards.

The kite formed by the wing differs from the boy's kite in being capable of change of form in all its parts. The change of form of the wing is rendered necessary by the fact, that the wing is articulated or hinged at its root (short axis), its different parts, as a consequence, travelling at various degrees of speed in proportion as they are removed from the axis of rotation. It is also practically hinged along its anterior margin (long axis), so that the tip travels at a higher speed than the root, and the posterior margin than the anterior. The compound rotation and varying degree of speed attained by the different parts of the wing has the effect of twisting the wing upon its long axis, and producing a variety of kite-like surfaces calculated to operate effectually upon the air, whatever the position of the wing may be.

The wing, when the flying animal is fixed or hovering steadily before an object, describes a figure-of-8 wave track in space,—the figure-of-8, when the animal flies in a horizontal direction, being opened out or unravelled to form first a looped and then a waved track.

In horizontal flight the wing describes a series of large waves or

curves, the body describing a series of smaller and opposite curves, the wing always rising when the body falls, and *vice versa.* The descent of the wing in this manner necessitates the elevation of the body, and the descent of the body contributes to the elevation of the wing.

The wing elevates the body when it descends, and the body, when elevated, falls forwards in a curve, and so contributes to the elevation of the wing. This arrangement draws the wing forward upon the air during the up stroke, and opposes the direct downward action of gravity by presenting the concave or biting surface obliquely to the air in the direction of the travel of the body. The under surface of the wing is thus made to act as a true kite during the up stroke.

The wing is urged at different velocities, the power applied being much greater during the down stroke than during the up one. The power is also greater at the beginning of the down and up strokes than towards the termination of those acts. The variation in the intensity of the driving power is necessary to slow the wing towards the termination of the down stroke, to prepare it for the up stroke, and to afford the air an opportunity of reacting on the under surface of the wing, to the elevation of which it contributes. The wing is elevated more slowly than it is depressed, and allows the body time to fall downwards, the fall of the body assisting in elevating the wing relatively to the bird. The wing, the air, and the weight of the body, are consequently active and passive by turns.

The wing is depressed by voluntary muscular efforts. It is elevated by vital, and mechanical acts, viz., by the contraction of the elevator muscles and elastic ligaments, by the reaction of the air called into play by the fall and forward travel of the body.

If the wing is in one piece, it is made to vibrate figure-of-8 fashion in a more or less *horizontal direction.* It thus attacks the air by a series of zig-zag movements, very similar to those performed by an overloaded dray-horse when ascending a hill. If the wing is in more than one piece, it is made to oscillate in a more or less *vertical direction;* the wing, under these circumstances, being usually closed during the up stroke and opened out during the down stroke. The wing is closed and its area diminished during the up stroke, expressly to avoid the resistance of the air.

The wing of the insect is, in some cases (the wasp, for instance), folded upon itself during the back stroke to avoid the resistance of the air; in other cases, when two pairs of wings are present (the butterfly, for example), the first pair of wings is made to overlap the second pair for a similar purpose.

When the wing is in one piece, and made to vibrate in a more or less horizontal direction, it is followed in its passage from right to left by a current which the wing meets in its passage from left to right. When the wing passes from left to right it is followed by a current which the wing meets in its passage from right to left, and so on. The wing has therefore the power of creating the current on which it rises.

When the wing is in several pieces, and made to vibrate more or less vertically, one portion of the pinion (during the acts of extension and flexion) makes a current which another portion utilises. Thus the tip and root of the wing (hand and arm) make a current during extension on which the middle part of the wing (fore-arm) acts during flexion, and the reverse. This arrangement begets a cross pulsation, and extends in the bird even to the primary and secondary feathers. The wing may thus be said to rise upon a whirlwind of its own forming.

The wing has the power of producing artificial currents, and of utilising and avoiding natural currents, so that it is equally adapted for flying in a calm and in a storm. As the wing (or parts of the wing) strikes in opposite directions, it in this manner reciprocates, the down stroke running into and contributing indirectly to the efficacy of the up stroke, and the reverse. The down and up strokes consequently form one continuous act, and neither is complete without the other. The down stroke produces the current on which the wing operates during the up stroke, and *vice versa*.

The reciprocation of the wing is most perfect when the animal is fixed in one spot, and least perfect when it is flying at a high horizontal speed. It is, however, a matter of indifference whether the wing attacks the air or the air attacks the wing, so long as a sufficient quantity of air is worked up under the wing in any given time.

The wing of the bat and bird are drawn towards the body and flexed at the termination of the down stroke to destroy the

momentum acquired by the pinion during its descent, and to prepare it for making the up stroke. It is elevated as *a short lever* to avoid the resistance of the air, and pushed away from the body or extended towards the end of the up stroke to prepare it for making the down stroke. It is depressed with great energy as *a long lever*, and hence the greater elevating and propelling power of the down as compared with the up stroke.

When the bat and bird are stationary, the tip of the wing, from its alternately darting out and in, and forwards and backwards, during extension and flexion, and during the down and up strokes, describes an ellipse, the axis of which is inclined obliquely *upwards* and forwards. When the bat and bird are progressing at a high speed, the axis of the ellipse is inclined obliquely *downwards* and forwards, the ellipse itself being converted into a spiral and then a wave line. The outward and forward (extension) and inward and backward (flexion) play of the pinion contributes to the balancing power of the bat and bird, as it augments the horizontal area of support.

The wing of the insect is recovered or drawn towards the body, and that of the bat and bird recovered, flexed, and slightly elevated by the action of elastic ligaments. Those ligaments, by their contraction, conserve and interrupt muscular efforts without destroying continuity of motion.

The elastic ligaments are in many cases furnished with muscular fibres, and are most highly differentiated in those animals whose wings vibrate the quickest.

The primary, secondary, and tertiary feathers of the wing of the bird are geared to each other by fibrous structures in such a manner that the feathers are made to rotate in one direction during flexion, and in another and opposite direction during extension. The double rotation of the feathers in question confers a distinctly valvular action on the wing of the bird.

The under surface of the wing of the bat and bird is thrown into a beautiful arch during extension and the down stroke, the arch being so formed that its tension increases according to the pressure applied.

The wing is inserted into the upper part of the thorax, and balances the body by playing alternately above, beneath, and on a

level with it. When above the body, the latter is suspended from the wings as from a parachute. When beneath the body, it is suspended from the top of a cone formed by the wings, and when on a level with the body, the latter is placed in the centre of a circle described by the rapidly oscillating wings. The body is suspended from the wings very much as a compass set upon gimbals is suspended.

The wing balances the body in consequence of its travelling at such a speed as enables it to convert the area mapped out by its vibrations into what is practically a solid basis of support.

The wing, whether in one piece or in many, rotates upon two centres, the one centre corresponding to the root of the wing (short axis), the other to the anterior margin (long axis). The rowing feathers have a similar compound motion. This mode of action of the wing is intimately associated with the power it enjoys of alternately seizing and evading the air, of producing artificial currents, and of utilising artificial and natural currents.

The wing is cranked slightly forwards, a small degree of rotation of the anterior margin being followed by a very considerable sweep of the posterior margin.

The wing area is greatly in excess of what is absolutely necessary, and as much as four-sixths may be removed in certain insects (the common blow-fly, *e.g.*), without destroying the power of flight. The wing area may also be considerably reduced in birds without in any way impairing flight. This shows that elaborate calculations of wing area, in relation to weight of trunk, must prove futile, unless the rapidity with which the wing vibrates and the state of the air are also taken into account.

Weight is necessary to the flight of the insect, bat, and bird, as at present constructed. If flying creatures were lighter than the air, the wing would require to be twisted completely round as in the auks and penguins, so that the under ventral or concave surface would strike from below upwards instead of from above downwards.

In aërial flight the under or concave surface of the wing is applied *from above*, whereas in subaquatic flight it is applied *from below*. The scull, like the subaquatic wing, is applied from below, so that the analogy between the aërial wing and the oar as employed in sculling is more apparent than real.

A diving bird which flies under the water *is lighter than the water*, and flies *downwards*. A bird which flies in the air *is heavier than the air*, and flies *upwards*. Relative levity and weight are therefore necessary to the diving and flying bird as at present constituted.

Weight, when associated with or operating upon wings, contributes to horizontal flight. A flying animal, when it drops from a height with expanded motionless wings, does not fall vertically downwards, but downwards *and forwards*, the wings converting what would otherwise be a vertical fall of the body partly into *forward travel*. The weight of the body thus to a certain extent relieves the muscular system from excessive exertion. If a sufficient breeze be blowing, the weight of the trunk and the breeze upon the wings operating conjointly are sufficient to keep the body of the animal in the air for protracted periods. This is well seen in the case of the albatross, which can sail about for an hour at a time when there is wind without once flapping its wings.

The wing, as a rule, is more flattened in the insect than in the bat and bird. It is, moreover, driven at a higher speed, those animals which fly the quickest having for the most part the flattest wings. The dragon fly furnishes a good example.

The greater the concavity of the wing, the greater the elevating power; the flatter the wing, the greater the propelling power.

The wings in living animals are thoroughly under control both during the down and up strokes; the wing, consequently, is not simply an elastic apparatus, which derives the movements of its separate parts from the air; on the contrary, it directs and controls the air in such a manner as to extract the maximum of support and propulsion from it.

The wings of bats and birds are moved by direct muscular action in combination with certain *elastic* ligaments, and the same holds true of the dragon fly and some other insects. The elasticity of the wing and the resiliency and reaction of the air, however, assist the muscles and ligaments.

The great speed attained by the tip and body of the wing is due to the fact that the wing is articulated or jointed at its root, any movement communicated at the root being quickened in proportion to the distance from the root. In other words, a compara-

tively slow movement communicated to the root of the wing is at once converted into a very rapid one at the tip.

If an artificial wing be constructed in strict accordance with any of the natural wings (insect, bat, or bird), and applied by a sculling figure-of-8 movement to the air, it will be found to supply a steady buoying and propelling power, similar in all respects to that supplied by the living wing.

In order to secure this result, the artificial wing should be concavo-convex, and slightly twisted upon itself, *i.e.*, it should be finely arched in every direction. It should be mobile as well as elastic,* and be applied to the air at different angles and at different degrees of speed, in such a manner that the wing and air may be active and passive by turns.

The *artificial wing*, like the natural one, must be more or less triangular in shape. It must taper from the root towards the tip, and from the anterior margin in the direction of the posterior margin. It should be capable of change of form, and elastic throughout, the flexibility being greatest at the tip and posterior margin of the wing, and least at the root and along the anterior margin. It must move in all its parts at different periods of time, as in this way the air is alternately seized and dismissed, dead points avoided, and a continuous reciprocating movement secured. In producing a continuous vibration of the artificial wing, much assistance is obtained by employing a ball-and-socket joint at its root, with a system of elastic springs of different strengths. The principal springs should be arranged at right angles to each other, the superior and posterior springs being stronger than the inferior and anterior ones. Oblique springs may be added, and the whole, because of their different strengths and their peculiar directions and insertions, can be made to give the wing any amount of torsion in the direction of its length during every portion of either the up or down stroke. The muscles and elastic ligaments of insects, bats, and birds, perform a similar function. A ball-and-socket joint, or what is equivalent thereto, is necessary at the root of the wing,

* Borelli (1668), Durkheim, and Marey state that an artificial wing should be composed of a *rigid rod* in front and a flexible sail behind, but experiment has convinced the author that no part of the wing should be absolutely rigid.

because the pinion should be free to move in an upward, downward, forward, and backward direction. It should also be able to rotate around its anterior margin to the extent of nearly a quarter of a turn. All the movements referred to are derived in the author's models from a *direct piston action*, from the reaction of the air, the elasticity of the wings and springs, and the weight of the machine bearing the wings. They are restrained and directed by the gearing apparatus extending between the piston and the wings, but more especially by the different lengths, strengths, and directions of the elastic springs themselves. The piston is made to descend with a very violent hammer-like motion at the beginning of the down stroke, the movement being gradually slowed as the wing descends to a certain point, at which the movement is reversed and the piston ascends more slowly, its ascent being occasioned for the most part by the reaction of the air, the elasticity of the wing and of the springs at its root, and by the descent of the engine propelling the wings. The driving power, the weight of the apparatus, the recoil of the air, and the elasticity of the wings and springs are thus made to act in concert, the different forces being active and passive at intervals, and no two forces acting together at precisely the same instant of time.

If a longitudinal section of a bamboo cane, 10 feet in length and half-an-inch in breadth, be taken by the extremity and made to vibrate, it will be found that a wavy serpentine motion is produced in it, the waves being greatest when the vibration is slow, and least when it is rapid. It will further be found that, at the extremity of the section where the impulse is communicated, there is a steady reciprocating movement devoid of dead points. The continuous movement in question is no doubt due to the fact that the different portions of the reed reverse at different periods, the undulations induced in the reed being to an interrupted or vibratory movement very much what the continued play of a fly-wheel is to a rotatory motion.

If a similar reed has added to it at its outer or distal half tapering rods of whalebone radiating in an outward and backward direction to the extent of a foot or so, and the whalebone and the reed be covered with a thin sheet of india-rubber, an artificial wing resembling the natural one in all its essential properties is at once

produced.* Thus if the wing be made to vibrate at its root, *a double wave is produced,* the one wave running in the direction of the length of the wing, the other in the direction of its breadth. The wing further *twists* and *untwists* figure-of-8 fashion during the down and up strokes. There is, moreover, a continuous play of the wing, the down stroke gliding into the up one, and *vice versa,* by a system of continuous and opposite curves, which clearly shows that the down and up strokes are parts of one whole, and that neither is perfect without the other. This form of wing is endowed with the very remarkable property that it will fly in any direction, demonstrating more or less conclusively that flight is essentially a *progressive wave movement.* Thus if the anterior or thick margin of the wing be directed upwards, and the angle which the under surface of the wing makes with the horizon be something less than 45 degrees, the wing will, when made to vibrate, fly with an undulatory motion *in an upward direction,* like a pigeon to its dove-cot. If the under surface of the wing make no angle, or a very small angle with the horizon, it will dart forward in a series of curves *in a horizontal direction,* like a crow in rapid horizontal flight. If the angle made by the under surface of the wing be reversed, so that the anterior or thick margin of the wing be directed downwards, the wing will describe a wave track and fly *downwards,* as a sparrow from the top of a house or tree. In all those movements *progression* is a necessity; the movements are continuous gliding *forward movements;* there is no halt or pause between the strokes, and if the angle which the wing makes with the horizon be sufficiently great, the amount of steady, *tractile,* and *buoying power* developed is truly astonishing. This form of wing elevates and propels both during the down and up strokes, and its working is accompanied with little or no slip. Its movements may be regarded as the literal realisation of the figure-of-8 hypothesis of flight.

* The author has made a great variety of artificial wings. Of these some are in one piece, with a continuous covering; others in a single piece, with the cover broken up into a large number of small valves; others in several pieces, with a continuous covering, and others jointed, with the cover broken up into a number of valvular segments. In all cases the frames of the wings are composed of *elastic material,* such as steel tubes, bamboo and other canes, osier twigs, whalebone, gutta percha, &c., &c.; the covers of the wings are made of india-rubber cloth, tracing cloth, argentine, linen, silk, &c., &c.; the springs of the wings of steel, caoutchouc, &c., &c.

If the artificial wing be in one piece, it ought to be made to vibrate in a more or less horizontal direction; if in several pieces, it should be worked in a more or less vertical direction, as the wing in this case acts alternately as a short and long lever, in virtue of its closing and opening during the up and down strokes, the acting area of the wing being greatly reduced during the up stroke, and greatly increased during the down one.

If a properly constructed artificial wing be made to vibrate in a vertical direction, it invariably darts *downwards and forwards in a curve* during the down stroke, and *upwards and forwards* in a *similar* but *opposite curve* during the up stroke, the two curves running into each other to form a progressive, continuous, *wave track*.

If the wing be made to vibrate from side to side in a more or less horizontal direction, it rises zig-zag fashion by a series of looped curve movements, each pass of the wing being on a higher level than that which preceded it. Whether the wing be moved vertically or horizontally, it invariably twists and untwists during its action. In twisting and untwisting, it developes figure-of-8 curves, not only along its anterior and posterior margins, but throughout its entire length and breadth.

The figure-of-8 vertical movement may be converted into the figure-of-8 horizontal movement by a slight rotation of the wing on its long axis, or by a tilt of the body or frame bearing the wing. It is in this way that the wing may act either as an elevator and propeller, or merely as an elevator. Thus it is not uncommon to see an insect elevate itself by a horizontal screwing figure-of-8 movement, and then, suddenly changing the direction of the stroke of the wing and of the body, dart forward in a nearly horizontal direction.

The artificial wing, like the true one, attacks the air at a great variety of angles during the down and up strokes. Thus during the down stroke the angles which the wing makes with the horizon are increased, whereas during the up stroke they are diminished.

The angles made by the different portions of the artificial wing vary as in the living wing, the angles made by the parts nearest the root being greater than those nearer the tip. This is occasioned by the manner in which the artificial wing *twists* and *untwists* during its action, the torsion in question being due to the

elastic properties of the wing and the resistance which it experiences from the air, as well as to the fact that the tip and posterior part of the wing travel at a much higher speed than the root and anterior part. The small angle made by the tip, as compared with the root of the wing, equalises its action, a large angle urged at a low speed giving the same amount of buoyancy and propelling power as a smaller angle urged at a higher speed.

The artificial wing, because of its elasticity and by the aid of certain springs, can be made to slow and reverse of its own accord at the end of the down and up strokes in precisely the same manner as the natural wing. It can likewise be made to change its course without halt or dead point, so as to give continuity of motion and continued buoyancy.

If the artificial wing be moved figure-of-8 fashion in a more or less horizontal direction, it can be made to create and utilise its own currents, the stroke from right to left producing the currents on which the wing rises in its passage from left to right, and the reverse. It can also be made to utilise and evade natural currents.

If the tip of a properly constructed artificial aërial wing be turned downwards, and the wing be made to move from side to side figure-of-8 fashion like the tail of a fish, it forms a very excellent aërial propeller.

The artificial wing, to be effective, must rotate about two separate axes, the one corresponding to its root (short axis), the other to its anterior margin (long axis).

If two artificial wings, similar to those described, be placed end to end, inclined at a certain upward angle, and made to revolve, they form a most powerful aërial screw. This form of screw is propelled with comparatively little force, and its working is attended with quite a nominal amount of slip.

The aërial screw here recommended *is elastic* and *capable of change of form* in all its parts, and so constructed that its angles vary to adapt themselves to the speed attained by the different portions of the blades at any given time. Thus the angles made by the blades are greatest when the speed at which the screw is driven is least, and *vice versa;* the angles made by those portions of the blades which are nearest the axis of rotation being always greater than those made by the portions nearer the tips of the blades. This form of

aërial screw differs widely from the aerial screws at present in use, and from the screw propeller employed in navigation, inasmuch as it is moveable in all its parts, and adjusts itself to its work in such a manner as to secure the maximum of elevating and propelling power, with a minimum of slip. The screw propeller and aerial screws as at present employed are, on the contrary, *rigid and unyielding*, and possess no accommodating power. As a consequence, much propulsive power is sacrificed in slip.

If the blades of the aërial screw referred to be greatly diminished in size, and formed of carefully tapered, finely graduated steel plate, it operates with remarkable efficiency in water, the elasticity of the screw diminishing the slip, while it greatly augments the propelling power.

The following Gentlemen were admitted Fellows of the Society:—

Rev. Thomas M. Lindsay, M.A.
William Robertson Smith, M.A.
Stair Agnew, Esq.

Monday, 30th January 1871.

Professor KELLAND, Vice-President, in the Chair.

At the request of the Council, Dr J. Collingwood Bruce delivered an Address on "The Results of the More Recent Excavations on the Line of the Roman Wall in the North of England."

Nearly a century after Julius Cæsar had landed in this island the conquest of Britain was begun in earnest.

In the year 79 Agricola planted the Eagles of Rome on the banks of the Tyne, and during the next campaign carried his conquests as far as the Tay. Before he gave up his command, he had raised the Roman standard in the Orkney Islands.

When Rome planted her foot she usually planted it firmly, and thus she retained in her grasp all the best portions of the island for more than 300 years. Some of the *legions* which landed in the

time of Claudius remained in the island until the close of the Roman domination.

In the year 410, when Alaric and his Goths entered Rome, Honorius renounced all claim upon the allegiance of Britain.

As to the origin of the wall, when Agricola advanced against the Caledonians, he thought it necessary to use precautions against a rising amongst the conquered tribes whom he left behind him. He made good roads contemporaneously with his advance. As he moved along he drew the road with him. By this means his retreat was always secure and his supplies comparatively certain. It is believed that we owe to him the northern Watling Street and the Maiden Way, which run northwards parallel to each other at about twenty-five miles apart. For miles together both of these roads remain to this hour as the Romans left them. Another precaution adopted by Agricola was the planting of garrisons in well-selected situations. There were two parts of the island where these garrisons could be best placed, namely, where the influx of the sea brings the eastern and western coasts into near contiguity—between the Firths of Clyde and Forth, and between the Tyne and Solway. Here walls were afterwards built. The southern wall was not a mere fence. It was a line of military operation. In erecting it the Romans did not give up the country to the north of it, but by its means made it more thoroughly their own. A transverse road along it was a necessary adjunct. At the Northumberland Isthmus Watling Street and the Maiden Way went north and south; another road, which has been called the Stanegate, went from east to west.

Dr Bruce then enumerated some of the principal stations in the wall as amplified and finally completed by Hadrian, who made use of such of the pre-existing stations of Agricola as served his purpose.

The stationary camps on the Roman wall usually have four gateways, one in each end, and one in each side rampart. Each gateway consists of two portals divided by strong piers of masonry, with its own arch overhead. There is uniformly a guard chamber on each side of the gateway.

The wall, as erected by Hadrian, exists to this day in wonderful completeness. Except in places where towns have sprung up on

its site, there is scarcely a yard of its course from Wallsend to Bowness where traces of it are not to be found. Where the stone-works have disappeared the fosse or earthen ramparts generally show themselves.

The wall is really an important fortification, consisting of several parts. There is first the stone wall, with a deep and broad fosse on its northern margin; next, the vallum or earth wall, which at varying distances keeps to the south of the stone wall. Then between these was a well-made road. Lastly, there was a series of stationary camps, castles, and turrets, for the accommodation of the soldiery who garrisoned the structure.

The length of the great wall is said to be seventy-three and a-half miles. It is usually about eight feet thick, and in two places it now stands nine and a-half feet high. Its original elevation was much greater.

The stations were military cities, mostly attached to the wall. The largest of them contain an area of six acres, some of them only three. The stations are distant from one another at an average of about four miles. Their form is that of a parallelogram with the corners rounded. The first thing which the builders of the wall did was to build the station, when they felt that they could safely undertake the other parts of the fortification, running the wall right and left. The masonry of the gateways is peculiarly massive and strong. In some of them the joints are as close as ever, and the courses as true as they were 1700 years ago. As far as can be ascertained, every station had a double gateway opening northwards, as well as in other directions. The north gate of Borcovicus station (House-steads) must have been much used, for its threshold is deeply worn by the feet of passengers.

That the Romans did not give up to the enemy the country on the north side of the wall is shown by a circumstance that the garrison at the station of Borcovicus had an amphitheatre provided for their amusement on the north side of the wall, where the ground outside the wall was best suited for its formation. It was not unusual with the Romans to provide amusements for the soldiery even upon a campaign.

In crossing from sea to sea, the wall, about the centre of its course, comes near an upheaved mass of basalt. For about ten

miles it takes advantage of this circumstance, and swerving out of its direct course, seizes hill after hill, so as to present to the enemy not only the obstacle of its own height, but that of the ridge of which it is built. A similar and more striking one of the natural ground is seen at Peel Crag.

When the wall runs over precipitous ledges like this, the fosse on the north side of it is of course discontinued, but the moment it again descends into the valley it is renewed.

Dr Bruce's paper contained several other particulars illustrating the present condition of the wall, and showing the powerful and systematic organisation displayed in its construction as a means of commanding and keeping in subjection the adjacent country. It also contained references to the monuments and inscriptions found in the line of the wall, indicating in particular the prevalent religious feelings of the period, and in particular showing an infusion of Eastern ideas into the native mythology of the Romans.

The following Gentlemen were admitted Fellows of the Society :—

Charles Hayes Higgins, M.D.
Angus Macdonald, M.D., F.R.C.P.

Monday, 6th February 1871,

Dr CHRISTISON, President, in the Chair.

The following Communications were read :—

1. Note on two Species of Foraminifera, and on some Objects from the Nicobar Islands of great Ethnological interest. By T. C. Archer, Esq. Specimens were exhibited.

Mr Archer exhibited two interesting Foraminifers, one being *Saccammina Carteri*, which forms a large proportion of the Carboniferous limestone at Elfhills, Northumberland; the other, a gigantic species of the Arenaceous group brought from Persia by the late Mr Loftus, and named after him, *Loftusia persica*. The latter specimen was that to which Mr Archer especially called the attention of the

Society, as it was similar to a class of fossils which had previously been found in the Upper Greensand formation in England, and believed to be sponges. However, the whole history of these monsters of their Order has been so well worked out in the admirable monograph of Dr Carpenter and Mr H. B. Brady, that their proper character is now thoroughly known.

Mr Archer also exhibited some objects of great Ethnological interest from the Nicobar Islands.

The following is the Memorandum accompanying the Wooden Figures obtained by Captain Edge, R.N., commander of H. M. S. "Satellite," from the Nicobars, in July 1867.

Reports having reached the authorities at Singapore that several vessels had, from time to time, been attacked by the savages upon these islands, and their crews barbarously murdered, it was determined to despatch an expedition to that spot; and accordingly, in July 1867, H. M. ship "Wasp," Captain Bedingfield, R.N., and H. M. ship "Satellite," Captain Edge, R.N., proceeded thence. The savages fled on the approach of the vessels of war, and upon landing at Enounga, one of the largest of the villages, Captain Edge discovered these figures in their huts, and upon his return to Singapore he gave them to Major M'Nair of the Royal Artillery for presentation to a museum.

The photographs are those of three of the savages who were captured, and of a little girl of seven years of age, who was rescued from their hands and brought to Singapore.

List of Wooden Figures from the Nicobar Islands, procured by Captain Edge, R.N., and presented to the Edinburgh Museum of Science and Art, by James M'Kenzie, master of the ship "Shree Singapora."

1. Large figure of a woman.
2. Male idol.
3. Figure of a native male in European style.
4. Do. do. (smaller size).
5. Figurehead of a native female.
6 & 7. Two small figures.
8. Figure of an animal.

These specimens were exhibited to the Ethnological Society in London at the beginning of last year.

After all that has been read of the complete absence of any kind of Art amonst the savages of these islands and the neighbouring Andamans, one is irresistibly led to think that these objects are not the works of the natives, but have been produced by some debased European or other captive.

2. Certain Phenomena applied in Solution of Difficulties connected with the Theory of Vision. By R. S. Wyld, Esq.

The theory of vision has been the subject of much more scientific study than that of any of our other senses, but notwithstanding this, the subject is still encumbered with some difficulties and contradictions, the solution of which is essential to our having a true and complete theory. Such are the questions,—first,—regarding single and double vision, as depending on the excitement of corresponding, or, as they are generally called, identical points of the retinæ; second,—the question whether perception is in the retinæ or in the brain; and lastly, the question regarding the decussation and ultimate course of the fibres of the optic nerves.

Regarding the subject of *single vision* with two eyes, there has frequently been exhibited a great amount of misunderstanding; since the discovery of the stereoscope, however, the nature of what has commonly, though not with strict propriety, been called single vision, has become much better understood. The truth is, there is no such thing as single vision when two eyes are in use, and a very little attention will make it clear how the case stands. Take two shillings of like appearance, and place them correctly and with the same sides up, in the different compartments of the stereoscope, but so far apart that they do not appear to coalesce. In this position they are distinctly seen by each eye as two separate objects. Cause the coins next gradually to approach till they seem to coalesce or unite into one—we say seem, for there is no true visual union. Even when they seem to unite, there are still two impressions made—one on each retina—and a corresponding impulse is from each of these membranes sent to the brain and to the mind, though from the close resemblance of the two impressions it may be impossible to distinguish the one from the other.

To prove that there are two mental impressions, let us reverse one of the coins. When this is done, we have no longer the impression of one coin, but of two coins occupying the same place. Both are visible, and they appear as if the one were visible through the other. While we steadily regard this anomalous presentation, the eye and the judgment become alike puzzled by it, and an effort is made to reduce the phenomenon to a normal and intelligible object of vision; a succession of transformations is the result of the joint action of the mind, and of the disturbed nervous centres which ensues; at one moment we see one coin, and then, suddenly, it disappears, and the other takes its place; then we see both coins at once, or a part of each perhaps becomes alone visible. In ordinary vision, then, we must conclude that objects make an equal impression on the identical points of each retina, though we are not intellectually conscious of the fact of duality; and the question thus arises, If there are two retinal impressions, how do we account for the two appearing as if superimposed the one on the top of the other? The eyes are set apart in the head, and the supposed sensory ganglia at the base of the brain, the *corpora geniculata*, the *corpora quadrigemina*, and the optic thalami, are all in duplicate: and the cerebral hemispheres divide the head in two equal sections. How, then, are we to account for the two visual images being united? It has been very generally assumed that the mind combines the two impressions, as it were, into one. This is the opinion of Professor Wheatstone and Dr Carpenter, and it was for many years my opinion; but the phenomena about to be alluded to convinced me that I was wrong, and that there exists a physical cause for the union of the two images; and to prove this is the main purpose of the paper.

When we take two strips of white card-board about an inch broad, and insert one at each side of the stereoscope, we find that each strip is distinctly seen by each eye; but when we cause them gradually to approach till the two ends appear to overlap say an inch or more, the effect is singular. Where the strips seem overlapping, the brightness is observed instantly to become very much increased: so much so, indeed, that when we fix the attention on the quadrangular part formed by the overlapping ends, all the rest of the strips become invisible, and the overlapping parts alone

remain distinct objects of vision. It may however be mentioned, by the way, that either of the cards may be recalled to sight by the simple act of moving it two or three times backwards and forwards, and thus exciting the nerve and arousing the attention; but this in no degree impairs the superior brightness of the overlapping parts.

Such are the facts, but what is the cause of the increased brightness where the cards appear to overlap, and what is the cause of the apparent overlapping where corresponding points of the retinæ are excited by objects in reality apart? I am not aware of any writer having distinctly laid before us a specific physical cause accounting for these several phenomena. It appears to me that they clearly point to an anatomical cause.

A great many writers have attributed single vision to habit. Dr Smith in his optics attributes single vision to this cause. Dr Carpenter also seems to take this view. He says ("Physiology," p. 705), "A condition of single vision seems to be that the two images of the object should fall on parts of the retinæ accustomed to act in concert, and habit appears to be the chief means by which this conformity is produced." Dr Reid, in his "Inquiry into the Human Mind," states that he has devoted thirty years to the study of the subject, and he accepts it as a mystery which cannot be explained. Sir Wm. Hamilton attempts no explanation. Neither does Sir D. Brewster in his famous controversy with Professor Wheatstone attempt any explanation. Buffon thinks we first see objects double and inverted, and that we correct this judgment by experience. Blanville, Gassendus, Porta, Tacquet, and Gall, maintain that we see with only one eye at a time.

Perhaps the majority of writers have looked no deeper than the surface of the retina, and have been content to state the phenomena as depending on an inscrutable property of that sensitive membrane, or simply as a law of our being: even as they, with quite as little ingenuity, and with less excuse, attribute our sense of visual direction to an inscrutable property of the retina. Some anatomists have, however, supposed that the decussation of the optic nerves might explain the phenomena. Dr Wollaston, from a peculiar occasional disorder in his vision, suggested that there was a crossing of the fibres from the *inner parts* of either retina to the

ganglion on the opposite side of the head, while the fibres on the *outer* side of each eye went to the ganglion on their own side of the head. This explanation evidently implies that the retinæ are optically divided in two halves, and that the images of objects falling on the centres of the retinæ are similarly divided, one half of every object being represented on the right side of the head, and the other half on the left; and that objects whose images fall on the one side of the retinæ are represented only on the lobe on that side of the head. This is surely extremely improbable.

Newton, in his optics, throws out a query (query 15th at the end of Second Book), suggesting that the species or picture of the objects seen with both eyes may be united in the commissure of the optic nerves, the fibres of the right side of both nerves uniting there, and, after union, going thence into the brain on the right side of the head, and the fibres on the left side of both nerves, after union in the commissure, going into the brain on the left side of the head, and the two meeting in the brain in such a way that the fibres make but one entire species or picture. The writer had not seen Newton's query till after his paper was submitted to the Council, but he considers that Newton's is the most advanced position which has up to the present times been taken on the subject. It is evident, however, that Newton had never very carefully reduced his idea to form, nor had he then the means which we now possess of testing its correctness; and it was doubtless owing to this circumstance that the idea, instead of being followed up and corrected in its details, was allowed to fall out of sight, and failed to gain the attention of optical writers.

Whether there is or is not a crossing of the true visual or optic nerves in man and the higher mammalia seems yet to be an unsettled point, though the opinion is gaining ground that there is a crossing of the inner fibres. It is always asked if there is no crossing of fibres, why are the optic nerves brought into connection? The question, as an argument in favour of the crossing, is, however, robbed of half its force, when we consider that the apparent union of the commissure may not be for a transfer of the true nerves of vision, but for effecting a union of the nerves essential for the nutrition of the retinæ, and of those nerves whose function it is to secure equality and unity of action in the reflex opera-

tions which regulate the expansion and contraction of the iris of the eyes.

I do not believe in any partial crossing of the true visual nerve-fibres. The fact, however, of an entire crossing, or of no crossing at all, in no ways affects my theory, which I shall now, after a few necessary words of explanation regarding the functions of the retina, proceed to explain.

The central point of the retina, the *fovea centralis*, is distinguished from the rest of the retina by its peculiar anatomical structure. It is also distinguished by its superior discriminating powers. It is the only part of the retina which takes minute cognisance of the forms of objects. We may satisfy ourselves of this by fixing the eyes on any word in a printed book held at the usual reading distance. While the eyes remain fixed on the middle of any word of, say six or seven letters, most persons will find that they are quite unable to perceive a single letter of the adjoining word. This proves how limited is the area of distinct vision on the retinæ.

When we fix the eyes on any distinct object in an extended landscape we turn the axis of each eye to the object especially examined, and the images of it fall on the *foveæ centrales*, and appear single. All the other objects in the landscape are mapped at the same time around these central points, on corresponding parts of each retina, *i.e.*, on parts which are correspondent in distance and direction, from the *foveæ centrales;* and these objects also, so far as we can see them, appear single. The remarkable circumstance, however, is, that the slightest shift or displacement of the axis of one of the eyes, and of the image on it, disorders correct vision, and produces the perception of a duplicate impression of the landscape. This circumstance has led authors very generally to the conclusion, as I have said, that either habit, or some inscrutable law of the retinæ, causes *single vision* when corresponding parts of that organ are impressed, and *double vision* when non-corresponding parts of the two retinæ are acted on. The writer maintains that these phenomena, and also the phenomenon of increased brightness obtained by the use of both eyes, can only be explained on the assumption or theory, that the retinal impulses of both eyes are united in a central cerebral sensorium. He, therefore, suggests

that the true optic or visual nerve-fibres from the retinæ cross at the optic commissure, that they are continued through the optic tracts, and sweep inwards to the *corpus quadrigeminum;* that those from the left eye enter that cerebral lobe at the right side, and spread across and forward in it in the form of an inverted cone; while the nerve-fibres of the right eye enter the same lobe at the left side, and spread in a like manner across it from left to right. The fibres from each eye thus cross each other in this lobe, which, from being an important central ganglion, and most intimately connected with the fibres from the optic nerves, the writer suggests as the probable sensorium in vision. The effect of this simple arrangement is, that the *corresponding nerve-fibres* from each retina are brought into juxtaposition, fibre to fibre; and in natural vision the sensorium thus becomes the organ in which the nervous impulses which come from the two eyes are united and grouped in the form they occupy on the retinæ.

When, then, in the experiment before-mentioned we advance the two strips of card-board but a short way at each side of the stereoscope, their images are found on the inner parts of each retina, and the ends of the strips are seen as two separate objects, because their images are thrown on non-identical portions of the retinæ, and different parts of the sensorium are accordingly impressed. When, again, the strips are advanced a little further, till the images begin to cross the centre of the retina of each eye, the spectator immediately sees the ends to overlap, and at the same time to acquire additional brightness. This evidently arises from the corresponding parts of each retina being impressed, and the two similar impulses being transmitted to that portion of the sensorium with which these parts of the retinæ are in connection,—each nerve-fibre from the one eye bringing its impulse into juxtaposition with the corresponding impulse from the other eye. And thus we account at once, for the increased brightness, and the apparent superposition of the images of external objects. A diagram at a glance shows how these are the necessary results of the arrangement of the nerve-fibres which we have suggested.

That the nerve-fibres coming from each eye are not *united* or *fused* in the sensorium, but merely brought into juxtaposition, is a fact also proved by the following experiment with coloured strips.

When we introduce a blue strip at the one side of the stereoscope, and a red or yellow one at the other side, till they appear to overlap or unite into one object, the result is increased brightness where they overlap; but there is no blending of the colours so as to produce purple or green. The one coloured strip, as in the experiment with the coins, shines through the other; or at one time the colours are alternately visible, at another time one-half of each coloured end only is visible, and occasionally spots of the one are seen to shine through the ground colour of the other, thus establishing the important fact or law, that though the combination of different colours, external to the living organism, produces the effect of an intermediate colour, yet the impulse of different colours on separate retinæ can not be so combined by the mind, but the impulse peculiar to each colour is conveyed by the nerve receiving it to the sensorium unchanged, and excites in the mind its own characteristic sensation. The increased intensity where the adjoining nerve-fibres in the sensorium are all in action I attribute to the well-known law of irradiation, or *lateral* expansion of nervous action, which exists among neighbouring nerve-fibres when powerfully excited

The arrangement of the fibres above suggested explains—

1*st*. The nature and cause of the peculiar action of the identical retinal points.

2*d*. The physical cause of single and double vision.

3*d*. The reason why we have increased brightness by the use of both eyes, whether in ordinary vision or when using the stereoscope.

4*th*. The several phenomena force us to the conclusion that visual sensation is not in the retinæ, but in a common cerebral sensorium.

3. Additional Note on the Motion of a Heavy Body along the Circumference of a Circle. By E. Sang, Esq.

Abstract.

In the course of physical inquiries we meet with many problems having the appearance of great simplicity, and yet presenting to the analyst difficulties of the highest order. The law of the motion of a heavy body along the circumference of a circle is one of these.

One particular case of this motion, viz., the case of the swinging of a clock-pendulum, is of paramount importance, and has been investigated with very great care. In this case our attention is directed principally to the computation of the time of an entire oscillation, since it is this which determines the beating of the clock. In the paper to which this note is an addition (Vol. xxiv. Trans.), a very rapid method of computing this total time is given. My object is now to supply the deficiency in that paper, and to show how the time of describing any given portion of the whole arc may be computed.

The general question may be stated thus:—A heavy body is projected with a known velocity along the circumference of a circle, and we are required to compute the time in which it will reach any indicated position, as also its place at any prescribed time.

No practicable solution of either of these problems has hitherto been given, with the exception of the case already mentioned. This note contains a simple and complete solution of both problems.

If a heavy body be projected from the lowest point of a circle along the circumference with a velocity less than that due to a fall from the highest point, its motion becomes slower as it ascends, and its speed is entirely exhausted at some point in the semi-circumference; from that point it returns to the bottom of the curve, passes to the other side, and so oscillates. But, if the initial velocity be greater than what is due to a fall along the diameter, the body passes the zenith point, and circulates round and round the circumference with an unequable motion. And if the velocity be just sufficient to carry the body to the zenith point, it rests there, and the motion ceases. Now, while the investigation of the oscillatory and of the continuous motion is difficult, that of the limit between the two is easy.

If the body move away from N with a velocity due to a fall through the distance ZN, it will have, when it reaches the point A, a velocity due to a fall through ZG. But the distance through which a weight falls freely is proportional to the square of its acquired velocity, and ZG is proportional to the square of ZA; wherefore the velocity at the point A must be proportional to the chord ZA; that is to say, the rate of increase of the angle NZA is

proportional to its own cosine; or, writing A for this angle, we have

$$dA \propto \cos A \,.\, dt\,, \quad dt \propto \sec A \,.\, dA$$

and, therefore, the time occupied in passing over some fixed minute portion of the arc at A is proportional to the secant of the angle NZA.

In Mercator's Projection of the Sphere, the differences of the meridional parts are proportional to the secants of the latitudes, wherefore the time of describing the arc NA must be proportional to the meridional part corresponding to the angle NZA, that is, must be proportional to the logarithmic tangent of $45° + \frac{1}{2}A$. Measure off then some distance ZE horizontally to represent the linear unit, and bisect the angle AZE by the line ZT meeting the plumb-line from E in T, the time of passing along NA is proportional to the logarithm of ET, or rather to the logarithm of the ratio of ET to EZ. Hence, when the angle is given we can readily compute the time, or when the time is given we can as readily compute the angle; and thus for this particular case the problem is completely resolved.

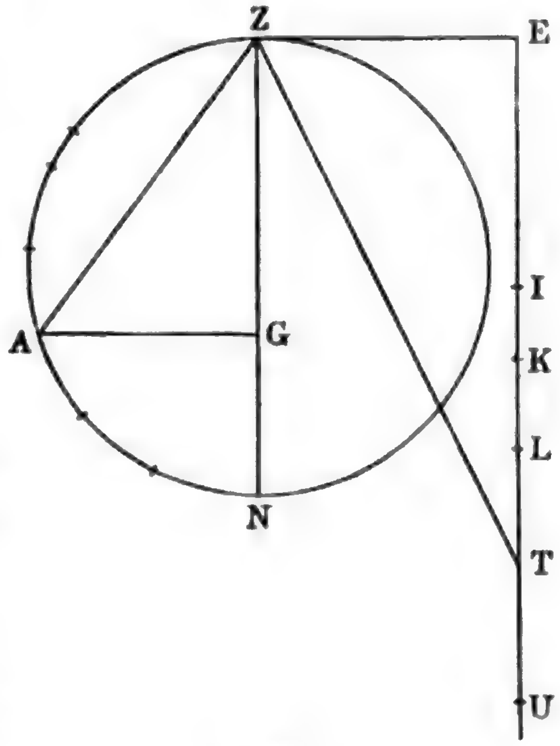


Fig. 1.

Making EI equal to EZ, if we make a series of continued proportionals EI, EK, EL, ET, EU, &c., and, joining Z with the several points, make angles doubles of EIK, EIL, &c., we shall obtain the positions of the moving body after equal intervals of time. The time of its reaching Z is thus infinite.

The relation of the continuous to the reciprocating motion may be exhibited by a simple contrivance. Let two straight rods AC, CB be jointed at the point C, and let the two ends A, B be connected by a straight line, say an elastic thread.

If the rods be turned so as to lessen the angle ACB, the angles

at A and B will increase. If the motion be sufficiently continued, the greater angle A will become a right angle, and then B will have reached its maximum. Should the motion be still further continued, A becomes obtuse and B decreases; till, when the rods have entirely closed, A becomes 180° and B becomes zero. Continuing the angular motion, A becomes a reverse angle, and B appears on the opposite side of AB. Thus the alternate increase and decrease of the smaller angle B resembles the changes of the angle NZA (fig. 1), when the motion is oscillatory. And at the same time the continual development of the angle at B

Fig. 2.

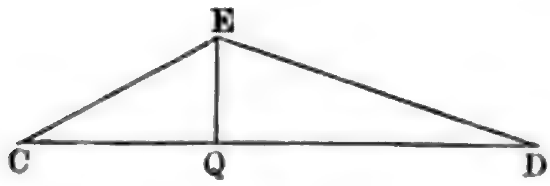


Fig. 3.

resembles the change of NZA when the heavy body over-passes the zenith point. The resemblance is a close one, for if we suppose CAB to increase with a velocity proportional to the distance PB, intercepted by the perpendicular CP, its variations are then exactly analogous to those of the angle NZA, when a heavy body revolving in a circle whose diameter is proportional to AC, has its velocity at the lower point equal to that obtained by falling through a distance proportional to CB. And similarly the variations of the smaller angle B are analogous to the oscillations of a heavy body in another circle, the greatest height being to the whole diameter in the ratio of AC to CB.

When AC is very small in comparison with CB, the maximum angle B is also small; that is to say, the arrangement represents an oscillation in a small arc; but when the two rods are nearly of equal lengths, as in the case of CE, ED (fig. 3), the maximum value of D approaches to a right angle, and the arrangement represents an oscillation extending to nearly the whole circum-

ference. If the trigon were isosceles, the representation would be that of the motion which we have already investigated.

If the angle A vary with a velocity proportional to PB, and B with a velocity proportional to AP, the exterior angle at C must have the rate of its variation proportional to AB. Now, if we make DCE (fig. 3), equal to half the sum of CAB and ABC, CE a mean proportional between AC and CB, and then inflect ED equal to half the sum of the same lines, the perpendicular EQ intercepts QD just half of AB. Thus QD is proportional to the rate of increase of ECD, and consequently CQ to the rate of change of CDE Thus the synchronous variations of the trigons ACB and CED would represent four connected cases, two of oscillation and two of revolution in a circle.

Now, the ratio of CE to ED is much nearer to one of equality than is the ratio of AC to CB; and if we were to proceed again in the same way, we should obtain a trigon still more nearly isosceles; and, after a very few operations of this kind, we shall obtain a trigon sensibly isosceles. That is to say, we shall have referred the oscillation in a given arc to the motion in just the whole circumference. So, seeing that the motion in this last case has been completely investigated, we have a complete solution of the general problem; the necessary calculations being of remarkable simplicity.

4. On the Capture of a Sperm Whale on the Coast of Argyleshire, with a Notice of other Specimens caught on the Coast of Scotland. By Professor Turner.

In the autumn of last year, whilst spending a few days in the neighbourhood of Oban, I visited Dunstaffnage, and in the court-yard of the Castle saw the two halves of the lower jaw-bone of a sperm-whale. On inquiry, I learned that they were the relics of a whale captured some years ago in the neighbouring sea. From some of the older inhabitants of Oban I gleaned some particulars respecting this animal; and as no record of its capture has as yet found a place in zoological literature, I am induced, as the sperm-whale so very seldom visits our shores, to communicate a brief notice to the Society.

In the month of May 1829 a large whale was seen spouting in

the Sound between Lismore, Mull, and the mainland. The fishermen were at first afraid to approach it, but as, after a few days, the animal became less active in its movements, they sallied forth in boats, and inflicted severe wounds with harpoons and other weapons. The animal was then secured, and towed ashore in Dunstaffnage Bay, close to the ruins of the Castle. It was said to have been about 60 feet long, and possessed a very bulky head, with a square snout. It was at once seen to be very different in its form and appearance from the large whales which usually visit our shores; but it was not until an oily fluid, which flowed out of a wound near the snout, and congealed on the surface of the water, was recognised to be spermaceti, that the character and value of the animal was determined. A considerable quantity of spermaceti was obtained from the great cavity in the head, and the blubber yielded a large amount of oil. I could learn nothing definite as to the sex.

The lower jaw was preserved as a relic in Dunstaffnage Castle, and, in the garden of one of the hotels in Oban, I met with a caudal vertebra, which was said to have belonged to this animal.

When I saw the jaw it was much injured. Not only were all the teeth lost, but the symphysial ends of both halves were broken off, and the expanded articular portion of the right half sawn off and removed. It is to be feared, if some care be not taken to preserve the fragments which remain, that in a few years all trace of this rare and interesting specimen will have disappeared.

From the left mandible some measurements were obtained which may give an approximation to the dimensions of the bone. The length was 149 inches; but as the anterior end was absent—as, indeed, only the sockets of sixteen teeth remained—this measurement falls several inches short of the original length of the bone. The articular end was expanded, and possessed a vertical diameter of 22 inches. On its inner face was the very large opening of the dental canal. Close to the junction of the articular and dentary parts of the mandible was a well-marked constriction, where the bone measured only 8 inches in breadth. The breadth of the alveolar edge of the jaw, about its middle, was 4½ inches. In its general form the mandible was broad and thin at its articular part, then constricted, beyond which it dilated, and then gradually tapered away to the anterior extremity.

The first instance on record of the stranding of a sperm-whale on the Scottish coasts is the specimen described in the "Phalainologia Nova," by Sir R. Sibbald, which came ashore at Lime Kilns, on the north side of the Forth, in February 1689. It was a male, 52 feet long, and had 42 teeth in the lower jaw. Several portions of this animal were preserved by Sibbald in his museum, and formed a part of the collection which was presented by him* to the University of Edinburgh.

In the copy of the "Phalainologia Nova," in the library of the Royal College of Physicians of this city, a manuscript letter has been inserted, in which an account is given of the stranding of another sperm whale in the Forth. The manuscript is entitled "Part of a Letter from Mr James Paterson, Keeper of the Balfourean Museum at Edinburgh, to Mr Edward Lhwyd, Keeper of the Ashmolean Museum at Oxford. Edinburgh, July 22, 1701." Penes E. W.†

"There was lately a pretty big whale came in at Crawmond. It had no whalebone, and teeth only in the lower jaw, which, according to Sir R. Sibbald, is the characteristick of yt kind which has ye sperma cete. You have ys figured in Jonston, tab. 42 of his Fishes.‡ Diverse of our physicians were present at ye opening

* Auctarium Musæi Balfouriani e Musæo Sibbaldiano: sive Enumeratio et Descriptio Rerum Rariorum, tàm Naturalium, quam Artificialium, tàm Domesticarum quam Exoticarum: quas Robertus Sibbaldus, M.D. Eques Auratus, Academiæ Edinburgenæ donavit. Edinburgi, impressum per Academiæ Typographum, Sumptibus Academiæ, 1697. In this catalogue, under the head "De Piscibus Viviparis Raribus," the following specimens obtained from this sperm whale are referred to:—A tooth, the crystalline humour of the eye, a fragment of the flesh and skin, and a specimen of spermaceti from the head. "The Sperma Ceti was lodged most of it within the skull of it, which was of a prodigious bigness."

† Mr Small, the Librarian to the University and to the College of Physicians, informs me that the initials "E. W." are in all probability those of Dr Edward Wright of Kersie, who became a Fellow of the College in 1753. His valuable library of works on natural history, of which the copy of the "Phalainologia Nova," above referred to, formed a part, was presented, in 1761, to the College by Alexander Gibson Wright, Esq. of Cliftonhall.

‡ The "Historia Naturalis," by Joannes Jonstonus, M.D., was published at Amsterdam in 1657. Book v. De piscibus et cetis, contains a folio plate, tab. 42, on which is represented a great whale, 60 feet long, lying on its right side, and presenting its abdomen, with a large pendulous penis, to the observer. From the form of the head and the shape of the lower jaw it is

of ye head, where they got 2 barrels of sperma cete: This filled up the whole cranium; they could find no other thing they could call ye brain, if it were not a friable cineritious-like substance, which seemed very improbable. They found ys sperma, not only in ye head and spina dorsi, but (which perhaps has not been hitherto observed) dispersed through ye whole body; in ye glands, whence they prest it out in considerable quantities. The chyrurgions spoke of buying the skeleton; but I don't know how it came, ye owners disposed of all another way, so yt neither they nor we got anything of it. Dr Sibbald got a tooth. He has made a description of it, and says he has materials for a 2nd part of his 'Phalainologia.' Our whale was a male: the penis appeared near 7 feet without ye body. The whole length of the creature was near 52 feet, and ye circumference of ye biggest part of it about 30. The nether jaw was only 3 foot ½ about, and had 48 teeth in it. The upper jaw had sockets lined with cartilages to receive 'em."

Dr Wright has also inserted into the same copy of the "Phalainologia Nova" a plate containing six figures, which are marked as follows:—Fig. 1. Balæna fœmina, pinnis et cauda sinuatis; fig. 2. Balæna Macrocephala in faciem obversa, ut dorsum appareat; fig. 3. Eadem in latus decumbens; fig. 4. Delphinus; fig. 5. Phocœna; fig. 6. Pediculus Ceti Bocconi.

In explanation of this plate, Dr Wright states—"This plate I found in a book of original drawings of Sir Robert Sibbald's, which I met with accidentally some years ago. All the explanation I could make out is as follows:—Fig. 1. The original drawing is marked in Sir Robert Sibbald's own hand, 'A Whale cast in at Resyth Castle.' Figs. 2, 3, marked in Sir Robert's hand, 'A Sperma Ceti Whale;' and in another hand, 'Whaile at Monyfeith, Feb. 23, 1703—(fig. 2) backe, to represent the taill; (fig. 3) side; but it did lay halfe upon its side that one Ey & a litle of the bellie was

obviously a sperm whale. The drawing has clearly been made from the animal as it lay on the beach, as the coast line, and numerous figures of persons, either gazing at the whale or on their way to see it, are carefully given. The whole plate has an air of truth and nature which contrasts favourably with the imaginary figures of dragons, mermaids, basilisks, griffins, and unicorns represented in other parts of the work.

sanded. 57 foots long and 56 round, tooth under, & all the skin blackish blew, werie smooth, and as thick as a bull's, & all white fat within & nixt the skin.'"

Figures 2 and 3 are very fair representations of the back and left side of a male sperm whale, and the plate was in all probability prepared for the second part of his "Phalainologia," which does not seem, however, to have been published.

In the year 1756 a sperm whale, 63 feet long, is said to have been stranded on the west coast of Ross-shire.*

In the year 1769 a third specimen was seen in the Forth. It ran ashore on Cramond Island, on December 22, and was there killed. It was described and figured by Mr James Robertson, of Edinburgh, in the "Philosophical Transactions."† This animal was a male, and measured 54 feet in length, the greatest circumference being 30 feet.

In the Statistical Account of Scotland, vol. v., 1793, it is stated in the account of Unst, in Shetland, that "the spermaceti whale sometimes wanders to this coast, and is here entangled and taken." The Rev. George Low, in his "Fauna Orcadensis," 1813, says that the sperm whale "is often drove ashore about the Orkneys, and sometimes caught. One, about 50 feet long, was caught in Hoy Sound, some years ago, from which was extracted a vast quantity of spermaceti; as also another, which drove ashore in Hoy."

The most recent specimen, also a male, of this animal was washed ashore, in a much decomposed state, in July 1863, near Thurso. The skeleton was presented to the British Museum, and formed a part of the material from which Professor Flower has drawn up his admirable account of the osteology of the sperm whale.

This whale, in the tropical or semi-tropical seas, which more especially are its proper habitat, moves about, as a general rule, in large herds or "schools." The eight well-authenticated specimens which have now been captured on the Scottish coasts have been solitary animals, which have wandered northwards, perhaps, in the track of the Gulf Stream. Of these eight specimens the sex

* Jardine's "Naturalist's Library, Mammalia," vol. vi. Cetacea. Edinburgh, 1837.

† March 10, 1770.

of three was either not recognised or has not been stated. Five, however, are known to have been males—a circumstance of considerable interest, as it serves to corroborate the statement made by Mr Thomas Beale, in his work on the natural history of the sperm whale, that "the large and fully-grown males always go singly in search of food."

5. On the Efficient Powers of Parturition. By Dr J. Matthews Duncan.

There can be no doubt that, among the numerous matters at present occupying the attention of obstetricians, none is more important than the subject of this paper. So evident is the correctness of this statement that one cannot but wonder why attempts to arrive at the truth have been, so far as we know, delayed till the present day. It is long since excellent researches of an analogous kind in regard to the force of the circulation of the blood, the power of the ventricles of the heart, were published; yet such researches do not seem naturally so attractive, nor do they give promise of so valuable practical results as those into the power of labour.

It is well known that the first and, I believe, the best results in this inquiry have been obtained by careful deduction from experiments on the tensile strength of the amniotic membrane. The researches referred to were made quite independently, and published soon after one another by Poppel, of Munich, and by Tait and myself conjointly. Studying this subject, I thought of some other modes of reaching conclusions, such as by observations on the caput succedaneum. Means might be taken to find the force required to raise a caput succedaneum, and the variations of force required to raise this swelling in different degrees of thickness. Such an investigation would, no doubt, lead to similar valuable results, but the plan has never been employed. Again, observations might be made to ascertain the force required to rupture the fourchette or the perineum, and thus a fact might be got which would be of service in this inquiry. It is well known to accoucheurs how these parts sometimes offer a successful resistance to all the powers of labour. This resistance, if its force be ascer-

tained, is of course a measure of the power employed; at least, it would afford a valuable result as to the limits of the power. Like statements might be made regarding the laceration of the margin of the cervix uteri, as a test of the power exerted at the completion of the first stage of labour. Many methods were available, but none were till very recently worked out.

It is probable that many intelligent and thoughtful accoucheurs had some rough ideas as to the amount of power exerted in parturition. They could not fail, in attending on ordinary labours, to observe the strength of hand and arm required to keep back the head too rapidly advancing over a delicate perineum. This power is, under certain conditions, a measure of the force of the labour, but I am not aware that any one has hitherto made the simple and proper dynamometrical experiments to decide the amount of force so exerted by the accoucheur. The problem may be more exactly stated as follows:—If in an unobstructed and powerful labour, the accoucheur, by the directly opposing pressure of his hand on the fœtal head, arrests its progress for one or several pains, he has in the pressure of his hand a force which, added to the small amount required to effect parturition, exceeds all the combined powers of labour in this case. He may then estimate by dynamometrical experiment what was the force he used, or what force he is capable of applying in the way in which he actually applied it to arrest the progress of labour. This experiment may be varied in different ways, of which I may mention one. Let us suppose a case of rigid vulva, the perineal resistance being overcome, and the head retroceding during the interval between powerful bearing down pains. Now, it is well known that in such a case a little manual pressure from above may be enough to push the head down again on the perineum, or to resist retrocession, or that the first and painless part of the next pain will make the head that has retroceded, again bulge out the perineum, before it is forced by the powerful acme of the pain against the resisting vulva. If, then, the practitioner opposes the advance of the head even so far as to bulge out the perineum, he must have a nearly exact measure of the force which the labour could bring to bear against the vulvar obstacle.

In such experiments or practice, what force does the accoucheur

exert? I have a hand well accustomed to such work, and I find by actual trial with an accurate dynamometer, 50 lbs. to be about the highest power I can use, situated as I am at the bedside in attendance on a case. I have ample reason, then, in such experience to believe that very few of the most powerful labours exert a force of 50 lbs.; that an ordinary strong labour is easily arrested by a much smaller force than 50 lbs.; that the great majority of labours is accomplished by repeated efforts whose highest power never exceeds 25 lbs. I may add that, in the great mass of short forceps deliveries, the force required from the accoucheur, even when he delivers the head, unaided by the natural efforts, seldom reaches 50 lbs. These statements are, to a great extent, arbitrary or dependent on my skill as an observer, yet I feel very confident of their accuracy.

Again, the intelligent practitioner who has observed a case of difficult labour finished either by the long forceps or by podalic extraction, could not but form some rough idea of the force he used, and compare it with the force which the labour exerted in its nugatory struggles. The force which the accoucheur thus exerted would not be certainly the equivalent of what the labour must have put forth in order to produce a spontaneous termination. It would, no doubt, in most cases surpass the force which the mother must have exerted to produce the spontaneous birth. But it would be, nevertheless, a valuable measurement indicating a force which in such a case the labour failed to produce. Joulin and I have made dynamometrical experiments to make use of such measurements in estimating the highest power of labour.

Another method of advancing our knowledge of this subject has been followed by the Rev. Professor Haughton. This gentleman does not, as his predecessors, examine the effects produced by the powers of labour, and thus get results having a very distinct positive value. He follows a plan which may be justifiable, yet which is difficult and dangerous. He takes an almost opposite method to that used by me. He measures the bulk and the extent of the involuntary and voluntary muscles employed in the function, and from these data he arrives at conclusions which he in one particular corroborates by a simple experiment. The results arrived at are statements of the powers of the parts, which are true if his methods

are true. Even if his methods are correct, the results are not actual values, but possible values, or statements of what may be, not of what has been.

These results are very different from those of Poppel, Tait, and myself, and it is one of the objects of this paper to inquire into their value. In doing this, I shall not discuss the method, but merely examine the results, by the aid of any obstetrical light which I can throw upon them.

Before proceeding to this inquiry, it is to be remarked that Haughton arrives by his method at new results which the methods of previous observers did not afford the means of reaching. There are, as is universally known, two great forces employed in labour—the uterine contractions and the involuntary and voluntary bearing down. The former of these forces is peculiar to the parturient female. The latter, as Haughton truly observes, is not peculiar to parturition, but is "available to expel feces, urine, or a fœtus." Haughton's plan is, to examine the uterus, measure it, and through this, arrive at a conclusion as to its power; then to examine the muscles which co-operate to produce bearing down, measure them, and through this arrive at a conclusion as to their power. The addition of the two results will, of course, give the power of labour. As I have already said, this is a dangerous and difficult plan to follow, and this is because there is room for error at every step.

The conclusions which Poppel and Tait and myself enunciated regarding the power of natural parturition stand on a completely different and, it appears to me, far more secure footing. There can, indeed, be scarcely any important difficulty raised regarding them. The strength of the fœtal membranes is ascertained by experiment. Certain facts are well known regarding the rupture of the membranes generally, and regarding their rupture in the labours in which the membranes experimented on were produced. These two sets of data, when put together, lead by a process of reasoning, which it would be tedious here to recapitulate, to conclusions regarding the lower limit of the power of natural labour, and regarding the power of labour generally, which cannot, so far as I see, be cavilled at. It is evident that this method tests only the whole or the combined powers of labour. It can afford no hint

as to the comparative value of the two forces which combine to produce the power which is to be measured.

The results given in Professor Haughton's paper which appear to me to be both new and important are three. I shall first state them, and then proceed to their examination one by one :—

1. The first conclusion is, that "the uterine muscles are capable of rupturing the membranes in every case, and possess in general nearly three times the amount of force requisite for this purpose." . . . . "It would be a waste of power (adds Haughton) to endow the uterus with more force than I have shown it to possess, for it is not necessary that the uterus should complete the second stage of labour, as the abdominal muscles are available for this purpose; so that by using them, and not giving the uterus more force than is absolutely necessary for the first stage of labour, an admirable economy of muscular power is effected." . . . "The extreme force of uterine contraction produces a pressure of 3·402 lbs. per square inch, which is equivalent to a pressure of 54·106 lbs. acting upon a circle of four and a-half inches in diameter, which is assumed as the average area of the pelvic canal."

2. The second of Professor Haughton's new and important conclusions is, that the action of the voluntary abdominal muscles "constitutes the chief part of the force employed in difficult labours." . . . "The amount of available additional force given out by the abdominal muscles admits of calculation, and will be found much greater than the force produced by the involuntary contractions of the womb itself."

3. The third conclusion is, "that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner." . . . "Adding together the combined forces of the voluntary and involuntary muscles, we find—

Involuntary muscles . .	=	54·10 lbs.
Voluntary muscles . .	=	523·65 lbs.
Total . .		577·75 lbs. av."

I. The first of Professor Haughton's conclusions on which I comment is, to the effect that the unaided uterine muscle can

exert a force in labour of 54 lbs., that this force is employed in dilating the cervix and rupturing the membranes, and that it can or does effect little more.

Now, it appears to me that Haughton limits far too much the use of the power of the uterus. I have no doubt that the uterine efforts not only dilate the cervix and rupture the membranes in most cases, but also do, in most cases, perform the chief part of the work required to bring forth the child. Although I do not coincide with Haughton in his reflections on the economy of muscular power, I shall not discuss the point therein raised. Yet I cannot avoid saying that, in the present instance, his own statements invalidate his reflections, for he asserts that the uterine muscle has three times the amount of muscular power required to do the work demanded of it. In endowing the uterus with this great power, Haughton, in my opinion, furnishes conclusive evidence against his own view as to the use of the contractions of the uterus. For I am sure that the great mass of births, even in difficult labours, including only the most difficult, is effected by a force less than what Haughton ascribes to the uterine muscle alone. I am satisfied that the whole combined powers of labour seldom reach above 50 lbs., while Haughton gives the uterus alone a power of 54.

I do not say Haughton is wrong in supposing that the uterus can exert a force of 54 lbs. On the contrary, I have no reason to doubt it. But I am sure that while easy labours require for their whole work a force scarcely exceeding the weight of the child, only a few difficult labours require for their whole work a force exceeding 50 lbs.

Every accoucheur knows to some degree of exactness the force which is required to restrain the forward movement of the child when there is no special resistance to its advance. This power I have measured approximatively by dynamometrical experiments, and I find it to be at the most 50 lbs.,—a power less than what is ascribed by Haughton to the unaided uterus. In other words, the uterus and voluntary muscles combined, stimulated to violent effort by insuperable temporary resistance, exert a force greater than is required to complete the labour; yet this force is generally much less than 50 lbs., and possibly never exceeds it.

It is well-known to accoucheurs that the great resistance to the progress of the child in the second stage of labour is what is called in obstetrics the perineum. The power of this part I do not know, and guessing is a bad proceeding in a scientific paper. Yet I may venture to say that no perineum would long resist a force of 50 lbs. repeatedly applied, a force less than Haughton ascribes to the uterine muscle.

II. Haughton's second conclusion is that the chief force in parturition is furnished by the voluntary muscles. The available power of these is (he says) 523 lbs., while that of the uterus is 54. The whole amount of expulsive force of the voluntary muscles is, he says, not usually employed to assist the uterus in completing the second stage of labour; but this does not contradict the conclusion we have ascribed to him. The conclusion is indeed, for Professor Haughton, inevitable, for every accoucheur knows that the bearing down efforts, whatever may be their actual measured power, are very strong, perhaps as strong as possible, quite frequently in ordinary labours. Besides, Haughton himself expounds his meaning in the following words:—"It is plainly necessary that the first stage in the expulsion of the fœtus should not be intrusted to a voluntary muscle, and hence an involuntary muscle is gradually provided, which takes the initiative and commences the process of parturition, the completion of which is then accomplished by the aid of voluntary muscles, to the employment of which, at this stage, no moral objection can be raised. It is also necessary (if the Contriver be allwise, or if the principle of least action in nature be true), that the involuntary muscle so produced, should not possess more or less force than is requisite for its purpose. The uterine muscle does not grow to meet a growing resistance (as happens frequently in other cases), and its precise degree of strength cannot be produced by a tentative process; for in healthy gestation the uterine muscle never tries its force against the membranes it is called upon to rupture until the actual period of parturition has arrived."

The view expounded in these words has great authority on its side beside that of the quoted writer, for the point therein raised as to the relative powers and uses of the uterine and auxiliary

forces of parturition is one that has been much discussed and for a long time. The great Haller, indeed, held opinions which are in accordance with Haughton's view. This renowned physiologist discarded the opinion common in his day, and now almost universally entertained, that the uterus is the main source of the power exerted in every stage of parturition.

Haughton gives us no reason for discrediting the general opinion of obstetricians, relying apparently on his conclusions alone regarding the comparative power of the two forces, that of the uterine muscle and that of the assistant voluntary muscles. No doubt he makes some observations intended to be corroborative as to the economy of force and other so-called laws of nature; but such reflections cannot be regarded otherwise than as premature by those who, like myself, do not adopt this writer's conclusions upon whose verity their justice depends.

In the course of his concise view of this question in his work on Physiology, Haller twice takes care to express his doubts as to the truth of his own opinions; and he ends by appealing to anatomists for light upon the subject. This appeal is, at least, ingenuous, for his argument against the ordinary opinion rests greatly upon the uterine fibres, their direction, and the direction of the force evolved by them; and, as Haller's notions on this anatomical point were very imperfect, and his mechanical ideas equally so, we need attach no weight to this part of his argument. Besides this, however, he has really nothing deserving the name of good evidence on his side. He thinks the effects produced by expulsive pains greater than the power of the uterus; but this is evidently mere begging the question. So also is his dependence, for aid in his judgment, on a picture of the great struggles of the voluntary muscles.

Authors generally do, as I have said, entertain an opinion opposed to that of Haller and Haughton. They are too numerous to name, and no one merits special mention; for, so far as I know, no one has distinguished himself by the novelty or elaborateness of his arguments in support of the ordinary view that the uterus is the chief agent in the whole process of parturition, and that the voluntary muscles, whether stimulated by volition or by reflex excitement, are, in a secondary position, aiding the uterus indeed, but not supplying the chief force. There is no positive value in an

argument of appeal to authority, yet it is evident that the amount of authority against him made Haller hesitate to enunciate his own views; and, when we consider the number, the intelligence, and the acute attention of the obstetricians who form a majority, scarcely differing from the whole body, in favour of our view, we cannot but be weightily impressed in its favour.

I must admit that some of the arguments made by obstetric authors to do regular service in defence of their view are very weak or quite vain. I may cite examples. Cases of parturition completed when the uterus is prolapsed, and is said to derive no assistance from bearing down efforts, are cited. But such cases prove almost nothing, even supposing they are correctly described; for there is in such cases absence of the ordinary difficulties of labour which consist in the propulsion of the child through the pelvis. Cases of expulsion of the child after death of the mother are quoted. But so far as I have perused them, they are given with a deficiency of circumstantial data such as to invalidate them altogether. Indeed, it is, in some of them, not even shown that the uterus acted at all; while in all there is the assumption that the difficulty of birth after death is as great as before it. The like objections may be made to examples of labour in asphyxia, narcotism, and syncope. It has been asserted also that narcotism by chloroform affords evidence that the uterus is the chief agent in parturition. But I must assert the incorrectness of this argument, and I cannot understand why Haughton should call attention to the influence of this agent, for any argument from it is valid, so far as it goes, only against his own views. I have, in a large experience, never seen chloroform inhalation destroy the action of the voluntary muscles. I believe it generally weakens their action, and it is well known that, at the worst, it only weakens the powers of labour. It is not known whether it weakens the uterine action or the action of the voluntary muscles in the greatest degree. If it does, as is alleged, when given profusely, destroy the action of the voluntary muscles, it certainly seldom completely arrests the progress of labour. Lastly, cases of labour in paraplegic women are cited in favour of the ordinary opinion. But I fear they do not even appear to favour it; and, with a view to the present question, they cannot be held as settling anything, seeing we do not know

what influence paraplegia may exert on the uterus itself. Besides, the cases are insufficient in every way.

The arguments on which I place chief reliance are the following:—

1. The great power of the uterus felt by the hand of the accoucheur, as in the operation of turning, long after the rupture of the membranes.

2. The great and sufficient power of the uterus observed in cases where the action of the voluntary muscles is weak or restrained.

3. The regulating influence of purely uterine pains in the progress of the second stage of labour.

4. The supremely important demand for and presence of powerful uterine action after the expulsion of the child.

5. The arrest of the progress of labour by inertia of the uterus. This argument appears to me unanswerable, for the condition often occurs when there is certainly only the slightest possible resistance to the progress of the child, when the mother ardently desires the completion of labour, and bears down violently with this object in view.

6. In cases of uterine inertia, such as are above described, the practitioner may find, by pulling with the forceps from below or pushing with the hands from above, in the absence of all parturient effort, whether of the uterus or of the voluntary muscles, that a very small force, say not exceeding the weight of the child, is sufficient to finish a labour upon whose progress violent bearing down efforts have had no effect.

7. The circumstance that, were the voluntary muscles the chief agents, expulsion of the child would be in great part a voluntary act, which it certainly is not.

8. The asserted completeness of the function of parturition in animals in which the assistant bearing down efforts are annihilated by opening the abdomen; the process being effected by their uterine and vaginal muscles, which are weak when compared with that of women.

Baudelocque and Velpeau* relate cases which appear to show that woman has very rarely voluntary power over the progress of parturition for a time Such cases offer no difficulty when regarded

* Traité complèt de l'art des Accouch. Ed. Bruxelles, p. 227.

with a view to the present question. They are explicable in more ways than one, and an illustrative statement is, for my present purpose, quite sufficient. Every experienced accoucheur has seen cases where voluntary increase of bearing down has sufficed to expedite labours, which, if the women had been left in a sleepy, lethargic condition, might have been protracted for an indefinite length of time.

There can be no doubt that the uterus is a very powerful agent in expelling the fœtus from its cavity into the world—that it is not the sole agent, and that it is assisted by the action of the voluntary muscles. Though I have not proved absolutely that the uterus is the chief agent in the performance of this function, yet I have no doubt that it is so; and I think that the arguments I have adduced give this belief of the profession the highest degree of probability. This belief does not imply that the aid afforded by the voluntary muscles is inconsiderable or unimportant. It only renders it quite incredible that while the power of the uterus is 54 lbs. that of the voluntary muscles can be 523.

III. Haughton's conclusion, on which I wish last of all to comment, is, "that, on an emergency, somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

In my work entitled "Researches in Obstetrics," to which Professor Haughton refers, I have discussed carefully, but briefly, this point, and announce the conclusion that the comparatively small figure of 80 lbs. gives the highest power of labour; and I quote Joulin, who estimates it at somewhat above 100 lbs. I do not deny that in exceptional circumstances a few pounds above 80 may be reached, but I feel pretty sure that seldom in the history of woman has the figure 80 been attained, whether on an emergency or not. This conclusion is arrived at by experiment and observation—experiments on the force required to pull a child through a contracted brim of pelvis, observations of the force used to complete a difficult labour, which nature, in its most violent throes, has failed to accomplish.

Every accoucheur will, I suppose, readily admit that, in a case

of delivery by podalic extraction, the surgeon can exert a great deal more force to bring the child into the world than the most energetic labour can. Now, in these circumstances the surgeon can use no force nearly reaching to a quarter of a ton. A very much smaller power would rend the luckless body of the child in pieces.

Such a power as a quarter of a ton does, in my opinion, represent a force to which the maternal machinery could not be subjected without instantaneous and utter destruction. To speak of a rigid perineum resisting such a power, or the fourth part of it, would be ridiculous. The possession and use even of a considerable portion of such a power would render the forceps and the cephalotribe weak and useless instruments. The mother could bray the child as in a mortar, and squeeze it through a pelvis which would, under other circumstances, necessitate Cæsarean section. Such a power would, if appropriately applied, not only expel the child, but also lift up the mother, the accoucheur, and the monthly nurse all at once. It would be dangerous not only to the mother and the child; it would imperil also the accoucheur. It has been calculated for me, that if this force were applied just as the chief resistance to delivery was overcome, the child would be shot out of the vagina at the rate of thirty-six feet per second!* The blow would be equal to the shock produced by the fall of the child from a height of twenty-one feet.

In an early part of this paper I have said that the method of inquiring into the subject which Haughton adopts is both difficult and dangerous, and I think I have said enough to show that danger has not been avoided. There must be error in Professor Haughton's calculation of the power produced by the action of the voluntary muscles, or there must be error in judging of the application of this power to the accomplishment of the function, or there must be error in both. I shall not attempt to show where the error lies, but its occurrence does not astonish me; for any one

* In making this calculation the child is taken as 7 lbs., the pressure as 580 lbs., and it is supposed to be exerted through a space of three inches—measurements which are fair statements of the case. It is farther supposed that the friction is negligible when compared with the forward pressure. This is certainly the case if the forward pressure be nearly as much as is stated by Professor Haughton as possible.

who has studied the difficult subject of the retentive power of the abdomen will recognise the difficulty of reaching conclusions as to the power of labour by Haughton's method. It is highly probable that the power of the voluntary muscle is dissipated, perhaps in compressing intestinal gases, perhaps in consequence of being misdirected.

Whatever may be the real source of error as to this matter, it is highly desirable to find it out, in order that we may, by more accurate proceedings, arrive at the true results which Haughton hoped to reach.

The following Gentlemen were admitted Fellows of the Society :—

Rev. William Scott Moncrieff, of Fossaway, M.A. (Camb.)
Professor A. R. Simpson.
Dr R. J. Blair Cunynghame.
Dr Cosmo Gordon Logie, Surgeon-Major, Royal Horse Guards.

Monday, 20th February 1871.

W. F. SKENE, LL.D., Vice-President, in the Chair.

The following Communications were read :—

1. On the Pentatonic and other Scales employed in Scottish Music. By the Hon. Lord Neaves.

Lord Neaves adverted to the peculiarity which had been observed in many Scotch airs, that they are composed on a pentatonic scale, and do not make use of the fourth or seventh of the gamut. It has been said that these airs can be played on the black notes of the pianoforte, which means that they can be played on the key of F♯ major, of which the fourth and seventh are represented by white notes, but are not needed. He also observed that this class of airs could be played on the white notes of the piano, both in the key of F and in that of G. They could be played on F, because, as they do not use the fourth, they do not need B♭; and they could be played on G, because, as they do not use the seventh, they do not need F♯. They could also, of course, be played on the key of C.

Many minor airs can be played on the pentatonic scale of the relative major; that is, airs on D♯ minor can be played on the black notes, and airs in A minor can be played on the white notes on the pentatonic of C; airs in D minor on the pentatonic of F; and airs in E minor on the pentatonic of G.

Specimens of major pentatonic airs are these—"Roy's Wife," "Auld Langsyne," "Ye Banks and Braes," "The Gypsies came," "Whistle o'er the lave o't."

Specimens of minor pentatonic airs—"The Mucking o' Geordie's byre," "My tocher's the jewel," "Auld Robin Gray" (old set), "Wandering Willie," "Ca' the yowes to the knowes."

Some minor airs are composed on the pentatonic of the tone below.

Specimens—"Adieu, Dundee" (in Skene MS.), "Blythe, Blythe."

In several old pentatonic airs grace notes or transitional notes have been added in modern singing or playing, but the original pentatonic character can still be traced.

Another large class of Scotch airs are composed on the full diatonic scale, and can be played entirely on the *white* notes without any apparent modulation.

When these airs are on the key of C major, there is nothing very peculiar in them, and there are many of this class. But when they are composed on other keys, certain peculiarities appear.

Several Scotch airs are composed in the key of G, but played on the full diatonic scale of C, so as frequently to introduce F natural, sometimes with a pathetic, sometimes with a comic effect. The old set of the "Flowers of the Forest" (Skene MS.) is an example of the one, and the tune of "Pease Strae" of the other.

Other specimens are—"Bessie Bell," "Tullochgorum," "Lochaber no more."

Minors in the diatonic scale are often singular, as, for instance, the air of "My boy, Tammie," played on the white notes. It runs into three keys—D minor, C major, and F major.

The pentatonic scale is not peculiar to Scotch music, but it may partly be accounted for by the fact that rude wind instruments are apt to be defective in the fourth and fifth. The simple diatonic scale, without other semitones, may in like manner have been used

from the adoption of early harps or other stringed instruments of a limited construction.

Scotch airs were often imitated by introducing a particular accentuation, called the Scottish "snap," as in the Vauxhall air, "'Twas within a mile of Edinburgh Town."

He expressed an opinion that many airs were common to Scotland and the North of England, and he denied that Scotch airs were always sombre, as had sometimes been alleged.

Airs illustrating the views above stated were played by Mr Bridgman in a manner of which it may be allowable to say that it gave great satisfaction to the audience.

2. On the Motion of Free Solids through a Liquid. By Sir William Thomson.

This paper commences with the following extract from the author's private journal, of date January 6, 1858:—

" Let $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$, $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ be rectangular components of an impul-
" sive force and an impulsive couple applied to a solid of invariable
" shape, with or without inertia of its own, in a perfect liquid,
" and let u, v, w, ϖ, ρ, σ, be the components of linear and angular
" velocity generated. Then, if the vis viva* (twice the mechanical
" value) of the whole motion be, as it cannot but be, given by the
" expression

" $Q=[u,u]u^2+[v,v]v^2+\ldots.+2[v,u]vu+2[w,u]wu+2[\varpi,u]\varpi u+\ldots.$

" where $=[u,u]$, $[v,v]$, &c., denote 21 constant co-efficients determin-
" able by transcendental analysis from the form of the surface of
" the solid, probably involving only elliptic transcendentals when
" the surface is ellipsoidal: involving, of course, the moments of
" inertia of the solid itself: we must have

$$[u,u]u+[v,u]v+[w,u]w+[\varpi,u]\varpi+[\rho,u]\rho+[\sigma,u]\sigma=\mathfrak{X},\ \&c.$$
$$[u,\varpi]u+[v,\varpi]v+[w,\varpi]w+[\varpi,\varpi]\varpi+[\rho,\varpi]\rho+[\sigma,\varpi]\sigma=\mathfrak{L},\ \&c.$$

" If now a continuous force X,Y,Z, and a continuous couple
" L,M,N, referred to axes fixed in the body, is applied, and if
" $\mathfrak{X}$ . . . . . &c., denote the impulsive force and couple capable
" of generating from rest the motion u, v, w, ϖ, ρ, σ, which exists

* Henceforth T, instead of $\frac{1}{2}$Q, is used to denote the "mechanical value," or, as it is now called, the "kinetic energy" of the motion.

" in reality at any time t; or merely mathematically, if $\mathfrak{X}$, &c., " denote for brevity the preceding linear functions of the com- " ponents of motion, the equations of motion are as follow:—

$$\left.\begin{array}{l} \dfrac{d\mathfrak{X}}{dt} - \mathfrak{Y}\sigma + \mathfrak{Z}\rho = \mathrm{X}\,, \dfrac{d\mathfrak{Y}}{dt} = \&c., \&c. \\ \dfrac{d\mathfrak{L}}{dt} - \mathfrak{Y}w + \mathfrak{Z}v - \mathfrak{M}\sigma + \mathfrak{N}\rho = \mathrm{L} \\ \dfrac{d\mathfrak{M}}{dt} - \mathfrak{Z}u + \mathfrak{X}w - \mathfrak{N}\varpi + \mathfrak{L}\sigma = \mathrm{M} \\ \dfrac{d\mathfrak{N}}{dt} - \mathfrak{X}v + \mathfrak{Y}u - \mathfrak{L}\rho + \mathfrak{M}\varpi = \mathrm{N} \end{array}\right\} \quad . \quad . \quad . \quad (1)$$

" Three first integrals, when

$$\mathrm{X} = 0,\ \mathrm{Y} = 0,\ \mathrm{Z} = 0,\ \mathrm{L} = 0,\ \mathrm{M} = 0,\ \mathrm{N} = 0,$$

" must of course be, and obviously are,

$$(2)\ \mathfrak{X}^2 + \mathfrak{Y}^2 + \mathfrak{Z}^2 = \text{const.}$$

" resultant momentum constant;

$$(3)\ \mathfrak{L}\mathfrak{X} + \mathfrak{M}\mathfrak{Y} + \mathfrak{N}\mathfrak{Z} = \text{const.}$$

" resultant of moment of momentum constant; and

$$(4)\ u\mathfrak{X} + v\mathfrak{Y} + w\mathfrak{Z} + \varpi\mathfrak{L} + \rho\mathfrak{M} + \sigma\mathfrak{N} = \mathrm{Q}\ ."$$

These equations were communicated in a letter to Professor Stokes, of date (probably January) 1858, and they were referred to by Professor Rankine, in his first paper on Stream Lines, communicated to the Royal Society of London,* July 1863.

They are now communicated to the Royal Society of Edinburgh, and the following proof is added:—

Let P be any point fixed relatively to the body, and at time t, let its co-ordinates relatively to axes OX,OY,OZ fixed in space, be

* These equations will be very conveniently called the Eulerian equations of the motion. They correspond precisely to Euler's equations for the rotation of a rigid body, and include them as a particular case. As Euler seems to have been the first to give equations of motion in terms of co-ordinate components of velocity and force referred to lines fixed relatively to the moving body, it will be not only convenient, but just, to designate as "Eulerian equations" any equations of motion in which the lines of reference, whether for position, or velocity, or moment of momentum, or force, or couple, move with the body, or the bodies whose motion is the subject.

x, y, z. Let PA, PB, PC be three rectangular axes fixed relatively to the body, and (A,X), (A,Y), . . . the cosines of the nine inclinations of these axes to the fixed axes OX, OY, OZ.

Let the components of the "impulse"* or generalized momentum, parallel to the fixed axes be ξ, η, ζ, and its moments round the same axes λ, μ, ν, so that if X, Y, Z be components of force acting on the solid, in line through P, and L, M, N components of couple, we have

$$\left.\begin{array}{l}\dfrac{d\xi}{dt} = \mathrm{X}\,, \quad \dfrac{d\eta}{dt} = \mathrm{Y}\,, \quad \dfrac{d\zeta}{dt} = \mathrm{Z} \\[2ex] \dfrac{d\lambda}{dt} = \mathrm{L} + \mathrm{Z}y - \mathrm{Y}z\,, \quad \dfrac{d\mu}{dt} = \mathrm{M} + \mathrm{X}z - \mathrm{Z}x\,, \quad \dfrac{d\nu}{dt} = \mathrm{N} + \mathrm{Y}x - \mathrm{X}y \end{array}\right\} \quad (6).$$

Let $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$ and $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ be the components and moments of the impulse relatively to the axes PA, PB, PC moving with the body. We have

$$\left.\begin{array}{l}\xi = \mathfrak{X}(\mathrm{A}, \mathrm{X}) + \mathfrak{Y}(\mathrm{B}, \mathrm{X}) + \mathfrak{Z}(\mathrm{C}, \mathrm{X}) \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \lambda = \mathfrak{L}(\mathrm{A}, \mathrm{X}) + \mathfrak{M}(\mathrm{B}, \mathrm{X}) + \mathfrak{N}(\mathrm{C}, \mathrm{X}) + \mathfrak{Z}y - \mathfrak{Y}z \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad (7).$$

Now let the fixed axes OX, OY, OZ be chosen coincident with the position at time t of the moving axes, PA, PB, PC, we shall consequently have

$$\left.\begin{array}{l} x = 0\,, \quad y = 0\,, \quad z = 0\,, \\[1ex] \dfrac{dx}{dt} = u\,, \quad \dfrac{dy}{dt} = v\,, \quad \dfrac{dz}{dt} = w\,, \end{array}\right\} \quad (8).$$

$$\left.\begin{array}{lll} (\mathrm{A}, \mathrm{X}) = (\mathrm{B}, \mathrm{Y}) = (\mathrm{C}, \mathrm{Z}) = 1 & & \\ (\mathrm{A}, \mathrm{Y}) = (\mathrm{A}, \mathrm{Z}) = (\mathrm{B}, \mathrm{X}) = (\mathrm{B}, \mathrm{Z}) = (\mathrm{C}, \mathrm{X}) = (\mathrm{C}, \mathrm{Y}) = 0 & & \\ \dfrac{d(\mathrm{A}, \mathrm{Y})}{dt} = \sigma\,, & \dfrac{d(\mathrm{B}, \mathrm{X})}{dt} = -\sigma\,, & \dfrac{d(\mathrm{C}, \mathrm{X})}{dt} = \rho \\[2ex] \dfrac{d(\mathrm{A}, \mathrm{Z})}{dt} = -\rho\,, & \dfrac{d(\mathrm{B}, \mathrm{Z})}{dt} = \varpi\,, & \dfrac{d(\mathrm{C}, \mathrm{Y})}{dt} = -\varpi\,, \end{array}\right\} \quad (9).$$

Using (7), (8), and (9) in (6) we find (1).

* See "Vortex Motion," § 6, Trans. Roy. Soc. Edin. (1868).

One chief object of this investigation was to illustrate dynamical effects of heliçoidal property (that is right or left-handed asymmetry). The case of complete isotropy, with heliçoidal quality, is that in which the coefficients in the quadratic expression for T fulfil the following conditions.

$$\left.\begin{array}{lll} [u,u] = [v,v] = [w,w] & \text{(let } m \text{ be their common value)} \\ [\varpi,\varpi] = [\rho,\rho] = [\sigma,\sigma] & \quad ,, \; n \quad ,, \quad ,, \quad ,, \\ [u,\varpi] = [v,\rho] = [w,\sigma] & \quad ,, \; h \quad ,, \quad ,, \quad ,, \\ [v,w] = [w,u] = [u,v] = 0\,; & [\rho,\sigma] = [\sigma,\varpi] = [\varpi,\rho] = 0 \\ \text{and } [u,\rho] = [u,\sigma] = [v,\sigma] = [v,\varpi] = [w,\varpi] = [w,\rho] = 0 & \end{array}\right\} \quad (10).$$

so that the formula for T is

$$T = \tfrac{1}{2}\{m(u^2+v^2+w^2) + n(\varpi^2+\rho^2+\sigma^2) + 2h(u\varpi+v\rho+w\sigma)\} \quad . \quad (11).$$

For this case therefore the Eulerian equations (1) become

$$\left.\begin{array}{l} \dfrac{d(mu+h\varpi)}{dt} - m(v\sigma - w\rho) = X, \text{ \&c.} \\ \text{and } \dfrac{d(n\varpi+hu)}{dt} = L, \text{ \&c.} \end{array}\right\} (11).$$

[Memorandum:—Lines of reference fixed relatively to the body].

But inasmuch as (11) remains unchanged when the lines of reference are altered to any other three lines at right angles to one another through P, it is easily shown directly from (6) and (9), that; if, altering the notation, we take u, v, w to denote the components of the velocity of P parallel to three fixed rectangular lines, and ϖ, ρ, σ the components of the body's angular velocity round these lines, we have

$$\left.\begin{array}{l} \dfrac{d(mu+h\varpi)}{dt} = X, \text{ \&c.} \\ \text{and } \dfrac{d(n\varpi+hu)}{dt} - h(\sigma v - \rho w) = L, \text{ \&c.} \end{array}\right\} (12).$$

[Memorandum:—Lines of reference fixed in space], which are more convenient than the Eulerian equations.

The integration of these equations, when neither force nor couple acts on the body (X = 0, &c.; L = 0, &c.), presents no difficulty, but its result is readily seen from § 21 ("Vortex Motion") to be that, when the impulse is both translatory and rotational, the point P, round which the body is isotropic, moves

uniformly in a circle or spiral so as to keep at a constant distance from the "axis of the impulse," and that the components of angular velocity round the three fixed rectangular axes are constant.

An isotropic heliçoid may be made by attaching projecting vanes to the surface of a globe, in proper positions; for instance, cutting at 45° each at the middles of the twelve quadrants of any three great circles, dividing the globe into eight quadrantal triangles. By making the globe and the vanes of light paper, a body is obtained rigid enough and light enough to illustrate by its motions through air the motions of an isotropic heliçoid through an incompressible liquid. But curious phenomena, not deducible from the present investigation, will no doubt, on account of viscosity, be observed.

Part II.

Still considering only one movable rigid body, infinitely remote from disturbance of other rigid bodies, fixed or movable; let there be an aperture or apertures through it, and let there be irrotational circulation or circulations (§ 60 "Vortex Motion") through them. Let ξ, η, ζ, be the components of the "impulse" at time t, parallel to three fixed axes, and λ, μ, ν its moments round these axes, as above, with all notation the same, we still have (26 "Vortex Motion")

$$\left.\begin{aligned} \frac{d\xi}{dt} &= \text{X, \&c.} \\ \frac{d\lambda}{dt} &= \text{L} + \text{Z}y - \text{Y}z, \text{ \&c.} \end{aligned}\right\} \quad \ldots \text{ (6) (repeated).}$$

But, instead of for T a quadratic function of the components of velocity as before, we now have

$$\text{T} = \text{E} + \tfrac{1}{2}\{[u, u]\,u^2 + \ldots + 2[u, v]\,uv + \ldots\} \quad \ldots \text{ (13).}$$

where E is the kinetic energy of the fluid motion when the solid is at rest, and $\frac{1}{2}\{[u, u]u^2 + \ldots\}$ is the same quadratic as before. The coefficients $[u, u]$, $[u, v]$, &c., are determinable by a transcendental analysis, of which the character is not at all influenced by the circumstance of there being apertures in the solid. And instead of $\xi = \dfrac{d\text{T}}{du}$, &c., as above, we now have

$$\left.\begin{array}{l}\xi = \dfrac{dT}{du} + Il,\ \eta = \dfrac{dT}{dv} + Im,\ \zeta = \dfrac{dT}{dw} + In \\ \lambda = \dfrac{dT}{d\varpi} + I(ny - mz) + Gl,\ \mu = \&c.,\quad \nu = \&c.\end{array}\right\} \ldots (14),$$

where I denotes the resultant "impulse" of the cyclic motion when the solid is at rest; l, m, n its direction cosines; G its "rotational moment," (§ 6, "Vortex Motion"); and x, y. z the co-ordinates of any point in its "resultant axis." These (14) with (13) used in (6) give the equations of the solid's motion, referred to fixed rectangular axes. They have the inconvenience of the coefficients $[u, u]$, $[u, v]$, &c, being functions of the angular co-ordinates of the solid. The Eulerian equations (free from this inconvenience) are readily found on precisely the same plan as that adopted above for the old case of no cyclic motion in the fluid.

The formulæ for the case in which the ring is circular, has no rotation round its axis, and is not acted on by applied forces, though of course easily deduced from the general equations (14), 13), (6), are more readily got by direct application of first principles. Let P be such a point in the axis of the ring, and $\mathfrak{C}$, A, B, such constants that $\frac{1}{2}(\mathfrak{C}\omega^2 + Au^2 + Bv^2)$ is the kinetic energy due to rotational velocity ω round D, any diameter through P, and translational velocities u along the axis and v perpendicular to it. The impulse of this motion, together with the supposed cyclic motion, is therefore compounded of

momentum in lines through P $\begin{cases} Au + I \text{ along the axis} \\ Bv \text{ perpendicular to " " ,} \end{cases}$

and moment of momentum $\mathfrak{C}\omega$ round the diameter D.

Hence if OX be the axis of resultant momentum; (x, y) the co-ordinates of P relatively to fixed axes OX, OY; θ the inclination of the axis of the ring to O; and ξ the constant value of the resultant momentum: we have

$$\left.\begin{array}{l}\xi \cos\theta = Au + I;\ -\xi\sin\theta = Bv, \\ \qquad\qquad \xi y = \mathfrak{C}\omega; \\ \text{and} \\ \dot{x} = u\cos\theta - v\sin\theta;\ \dot{y} = u\sin\theta + v\cos\theta;\ \dot{\theta} = \omega.\end{array}\right\} \quad (15.)$$

Hence, for θ, we have the differential equation,

$$A\mathfrak{C}\frac{d^2\theta}{dt^2} + \xi\left[I\sin\theta + \frac{A-B}{2B}\xi\sin 2\theta\right] = 0 \quad . \quad (16.)$$

which shows that the ring oscillates rotationally according to the law of a horizontal magnetic needle carrying a bar of soft iron rigidly attached to it parallel to its magnetic axis.

When θ is and remains infinitely small, $\dot{\theta}$, y, and $\dot{y}$ are each infinitely small, $\dot{x}$ remains infinitely nearly constant, and the ring experiences an oscillatory motion in period

$$2\pi\sqrt{\frac{B\mathfrak{C}}{[I + (A - B)\dot{x}](1 + A\dot{x})}} ,$$

compounded of translation along OY and rotation round the diameter D. This result is curiously comparable with the well-known gyroscopic vibrations.

3. Laboratory Notes. By Professor Tait.

1. On Thermo-electricity.

Messrs J. Murray and J. C. Young have been carrying out experimentally the idea mentioned in my former note on this subject. (*Proc.* Dec. 1870.) Their first sets of observations, of the results of which I subjoin a specimen, were made with an iron-silver and an iron-platinum, circuit working opposite ways on a differential galvanometer. The resistances (including the galvanometer coils) were in this particular experiment 53·1 and 25·9 B.A. units respectively, so that but very slight percentage changes could be produced in them by the elevation of temperature of the junctions. As one of a number of closely agreeing preliminary trials the result is extremely satisfactory, though the exact adjustment has not yet been arrived at. To show the parabolas due to the separate circuits, and thus exhibit the advantage of the method, I have requested the experimenters to break the circuits alternately after taking each reading of the complex arrangement, and take a rough reading. The last four columns of the table give the results; but, as the temperatures were probably slightly different from those in the first columns, no very direct comparison can be instituted. A glance at the 4th, 6th, and 8th columns, however, shows how nearly a linear relation between temperature-difference of junctions and galvanometer deflection has been arrived at in the

composite arrangement, while the separate circuits give marked parabolas.

Low Temp.	High Temp.	Pt. Fe., Ag. Fe.	Deflection for increment of 10° C.	Pt. Fe.	Deflection for increment of 10° C.	Ag. Fe.	Deflection for increment of 10° C.
12·3° C	39·0° C	28·5	10·67	44	16·28	17	6·32
„	72	61·5	10·30	96·0	16·08	36	6·03
„	104	93·0	10·14	143·5	15·55	51·5	5·61
„	146·5	136·5	10·17	202·5	15·08	68·0	5·06
12·6	185	172·5	10·0	250·0	14·50	77·0	4·46
„	202·5	190·5	10·03	268·5	14·13	79·5	4·18
12·4	229·5	219·5	10·11	298·5	13·74	81·5	3·74
„	251·5	239·0	10·0	318·0	13·30	81·0	3·38
12·5	263·0	250·5	10·0	330·0	13·16	80·0	3·19
„	272·0	260·0	10·0	337·0	12·98	80·0	3·19

I find great difficulty in obtaining wires of the more infusible metals:—and I am therefore endeavouring to make a complex arrangement for very high temperatures with palladium and two very different kinds of platinum. Wires of nickel, cobalt, molybdenum, rhodium, or iridium, or of any one of these, would be of immense use to me, and I should be happy to hear from any one whether there is a possibility of procuring them.

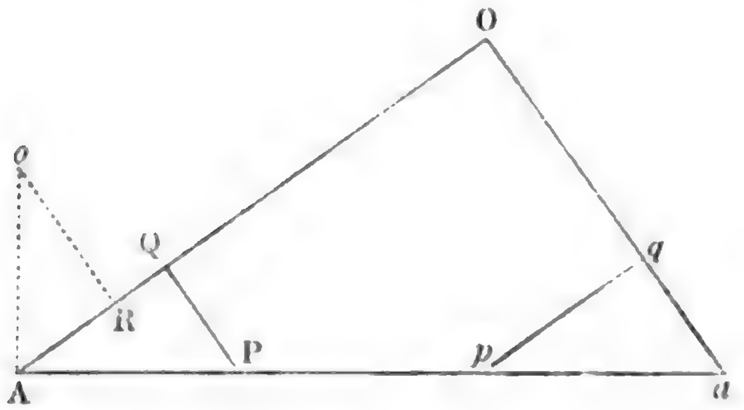
2. On Phyllotaxis.

I was recently led to consider this subject by Professor A. Dickson, who showed me some of his beautifully-mounted specimens, and explained to me the method he employs for the determination of the divergence, and of the successive leaves of the fundamental spiral or spirals. He referred me to two terribly elaborate papers by Bravais,* and I have since met with another of a similar character by Naumann.† These papers certainly cannot be supposed to present the subject from the simplest point of view. I do not doubt that the results I have here arrived at are to be found in some form or other in their pages, which are announced as completely elucidating the question; but I have not sought for them, my sole object having been to put what seem to me the elements of the matter as simply and intelligibly as I could.

* Annales des Sciences Naturelles, 1839.

† Poggendorff's Annalen, 1842.

Let A, a, represent the same leaf in a plane development of a branch or fir-cone (regarded as cylindrical); O, a leaf which can be reached from A by m steps in a right-handed spiral, developed into the straight line AO, and by n steps from a in a left-handed spiral aO. These spirals may in general be chosen so that m and n are not large numbers (3, 5, 8, 13, &c., being very common values); but they *must* (and can always) be so taken that m spirals parallel to aO, and n parallel to AO, shall separately include all the leaves on the stem or cone.



If m and n have a common factor λ, there will be $\lambda - 1$ leaves (besides A) which are situated *exactly* on the line Aa, and therefore the arrangement is composite, or has λ distinct fundamental spirals. If m' and n' be the quotients of m and n by λ, they are to be treated as m and n are treated below; and this case thus merges into the simpler one, so that we need not allude to it again.

It is obvious that, in seeking the fundamental spiral, we must choose the leaf *nearest* to Aa on the side towards O, as that succeeding A or a. The fundamental spiral will thus be right-handed if P, which is nearer to A than to a, be this leaf—left-handed if it be p. Of course, we may have a left-handed fundamental spiral in the former case, and a right-handed one in the latter; but the divergence in either will be greater than two right angles, and this the majority of botanists seem to avoid.

Draw PQ and pq respectively parallel to aO and AO, then the requisite condition is that

$$\frac{n}{m}\text{AQ} - \text{PQ}, \quad \text{or} \ \frac{m}{n}aq - pq,$$

shall be as small as possible.

Hence, if $\frac{\mu}{\nu}$ be the last convergent to $\frac{m}{n}$, and if $\frac{\mu}{\nu} > \frac{m}{n}$, it is

obvious that to get at P we must count μ leaves along AQ, and ν along QP. If, however, $\frac{\mu}{\nu} < \frac{m}{n}$, count ν leaves along aq, and μ along qp. P, or p, thus found is the next leaf of the fundamental spiral to A or a; the next is derived from it by a second application of the same process, and so on.

There is no necessity for restricting the development, as given above, to *once* round the cone. Suppose we go several times round and that A, a, α, &c., are successive positions of the same leaf. The processes given above may be employed, and the results will be of the same nature. But this extension enables us to obtain (more and more approximately, sometimes accurately) a *right* angle aAo, where o is a leaf reached after several turns of the fundamental spiral. This indicates that the leaves may be grouped (approximately or accurately) in lines parallel to the axis of the stem or cone. When this can be done accurately, it is easy to see that (since one of $\frac{n}{\nu}$, $\frac{m}{\mu}$, is greater, and the other less, than the number of leaves in one turn of the fundamental spiral) the difference of azimuth of two successive leaves of that spiral must be expressible in the form

$$2\pi\frac{r\mu + s\nu}{rm + sn};$$

where s and r are necessarily very small positive integers in all the ordinary cases of phyllotaxis, since they are the numbers of leaves in AR, Ro, respectively, which are portions of the spirals on which or parallel to which, m and n were measured.

The fraction

$$\frac{r\mu + s\nu}{rm + sn}$$

has been called the *divergence* of the fundamental spiral. Of its constituents the numbers m, n, r, s are at once given by inspection of any cone or stem, and (from m and n) μ and ν are easily calculated.

To extend this investigation to the cases in which the divergence is altered by torsion of the cone, it is merely necessary to notice that such a process alters only r and s. It produces, in fact, a simple shear in the developed figure.

Added, March 20th, 1871, *in consequence of some remarks made by Professor Dickson at the Meeting of that date.*

It is obvious that if the same leaf, O, be reached from A by m steps of a right-handed, and n of a left-handed, spiral (such that n of the former and m of the latter contain, severally, all the leaves), another common leaf can be reached by $m-n$ steps of the right-handed spiral, and n steps of a new left-handed one (these spirals possessing the same property of severally containing, in groups of n and $m-n$ respectively, all the leaves). This process may be carried on, when m and n are prime to one another, until we have steps represented by 1 and 1, in which case we obviously arrive at the leaf of the fundamental spiral next to A. It is better, however, to carry the process only the length of steps 1 and t, where t is determined by the condition that 1 and $t+1$ would give spirals both right-handed or both left-handed.

Now, in the majority of cases of fir-cones, it seems that we have t, found in this way, $=2$, i.e., *there are less than three leaves in a single turn of the fundamental spiral.* It is of course obvious that there can never be less than two, and the case of exactly two corresponds to the simplest of all possible arrangements, that in which the leaves are placed alternately on opposite sides of the stem. Fir-cones, therefore, give in general the arrangement next to this in order of simplicity. Hence, for such cones, and for all other leaf arrangements which are based on the same elementary condition, the values of m and n for the most conspicuous spirals must be of the forms

$$\begin{array}{ccccc} 2, & 3, & 5, & 8, & \&c., \\ 1, & 2, & 3, & 5, & \&c. \end{array}$$

These simple considerations explain completely the so-called mysterious appearance of terms of the recurring series 1, 2, 3, 5, 8, 13, &c., &c. The other natural series, usually but misleadingly represented by convergents to an infinitely extended continued fraction, are easily explained as above by taking $t=3$, 4, &c., &c. As a purely mathematical question it is interesting to verify the consistency of the statements just made, where the change in t is introduced, with those above made as to the effects of torsion in altering r and s. But this may easily be supplied by any reader who possesses a small knowledge of algebra.

Monday, 6th March 1871.

Dr CHRISTISON, President, in the Chair.

The following Communications were read:—

1. Account of the Extension of the Seven-Place Logarithmic Tables, from 100,000 to 200,000. By Edward Sang, Esq.

Abstract.

In this paper the details were given of the computations made for extending the Table of Seven-Place Logarithms to 200,000 and of the precautions taken to ensure accuracy in the printed work.

The calculations were originally intended for a Nine-Place Table to One Million; and the manuscript shows the logarithms to fifteen places, with their first and second differences for all numbers from 100,000 to 200,000.

2. On the Place and Power of Accent in Language. By Professor Blackie.

Professor Blackie then read a paper on "The Place and Power of Accent in Language." On the subject of accent and quantity, he remarked, especially in relation to the learned languages, the greatest confusion had prevailed, and the existing practice was altogether unreasonable and anomalous. In articulate sound four things had to be distinguished—volume or bulk, force or emphasis, elevation and depression, and prolongation or duration. English scholars had shown an unhappy incapacity of not being able to distinguish between stress and prolongation, and thus had been led to introduce the general practice of pronouncing Greek with Latin accents. In laying down the principles by which syllabic accentuation is guided, four points are to be attended to—significance, euphony, variety, and convenience. Fashion, of course, and custom have wide sway in this domain; but in the original structure

of language we have to look to significance and euphony rather than arbitrary usage, as the main causes which determined the place of the accent. In compound words it was natural that the qualifying or contrasting element should be emphasised, as in the proper Scotch pronunciation of *Balfour* (Coldtown), where the accent lies on that element of the word which distinguishes it from other *Bals* or towns. As to euphony, those languages are least euphonious which, like English and Gaelic, have a preference for the ante-penultimate accent, while those are most euphonious which, like Latin, Greek, and Italian, abound in penultimate or ultimate accented syllables. In respect of euphony, as well as variety, the Greek language was superior to the Latin, in that it allowed the accent on any of the three last places, while Latin allowed it only on the penult and ante-penult. The attempt to make out a special and exceptional case for Greek accents were vain. It is perfectly clear from the statements of the ancient Greek grammarians, that the Greek acute accent consisted not only in the raising of the voice on the syllable, as Professor Munro imagines, but in a greater emphasis or stress. The prejudice which has so long existed against the use of Greek accents arose partly from mere carelessness, partly from a notion that the observance of the accent would interfere with the proper quantity of the vowels, and destroy the beauty of classical verse. But this notion is altogether unfounded, as classical verse, originally an inseparable part of musical science, was not governed in any respect by the spoken accent, but guided entirely by the rhythmical ictus or time-beat. Practically, there was no difficulty in reading Greek prose by the accent, and Greek poetry by the quantity. In the μέλος, or purely musical part of the drama, the spoken accent naturally fell away. In recitation a sort of compromise probably took place, which is perfectly easy of execution. The paper included a history or review of the doctrines of learned men and great scholars on the subject of Greek accentuation, from Erasmus down to Chandler, Munro, Clark, and Geldart. It was astonishing that such confusion and beating the air about imaginary difficulties should have so long prevailed on a matter comparatively so simple; but there was not the slightest doubt that the moment our classical teachers should recur to living nature, instead of being governed by dead tradition in this

matter, the present monstrous, pernicious, and perplexing practice of reading Greek with Latin accentuation must cease. Independent of its absurdity, the loss of time occasioned by teaching one accent to the ear, and another to the understanding, should be motive enough for all teachers to deliver our classical schools from a yoke which, originally imposed by sheer laziness, is now supported only by ignorance, prejudice, and the tyranny of custom.

Monday, 20th March 1871.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. Notice of Exhibition of Vegetable Spirals. By Professor Alexander Dickson.

Dr Dickson exhibited a number of specimens, chiefly Fir Cones and Cacti, illustrating the principal series of vegetable spirals. Almost all the cacti and many of the cones were from the Edinburgh Botanic Garden and the Museum of Economic Botany there. As the nomenclature of the cacti in the Edinburgh garden, as in many other botanic gardens, is in a state of considerable confusion, the specific names will not be referred to, and the generic ones, even, must in some cases be held as only approximately correct. This, however, is of the less consequence as the phyllotaxis of such plants is eminently variable even in the same species. Ten different series or systems of spirals were illustrated by specimens, of which the following may be noted.

I. Ordinary series, $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}, \frac{5}{13}$, &c.

Cones of *Abies Douglasii* $\left(\frac{5}{13}\right)$: *A. excelsa* $\left(\frac{8}{21}\right)$: *Pinus Coulteri* $\left(\frac{13}{34}\right)$: *Araucaria excelsa* $\left(\frac{21}{55}\right)$: *Araucaria imbricata* $\left(\frac{34}{89}\right)$: Bijugates of the same series in cone of *Abies Douglasii* $\left(\frac{2}{5\times 2}\right)$, the solitary abnormality out of

200 cones examined; in an Echinocactus $\left(\frac{3}{8\times 2}\right)$; and in *Abies excelsa* and *Pinus Pinaster* $\left(\frac{8}{21\times 2}\right)$. Trijugates in an Echinocactus $\left(\frac{2}{5\times 3}\right)$; and in cones of *Abies excelsa* and *Pinus Pinaster* $\left(\frac{5}{13\times 3}\right)$.

II. Series, $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}$, &c.

Cones of *Pinus Pinaster*, *P. Lambertiana*, and *Abies excelsa* $\left(\frac{5}{18}\right)$: Mammillaria $\left(\frac{13}{47}\right)$: cone of *Pinus Jeffreyi* $\left(\frac{21}{76}\right)$. Bijugates of same series in an Echinocactus $\left(\frac{2}{7\times 2}\right)$; and one shoot of another Echinocactus $\left(\frac{3}{11\times 2}\right)$.

III. Series, $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}$, &c.

Echinocactus $\left(\frac{2}{9}\right)$; cone of *Pinus Pinaster* $\left(\frac{5}{23}\right.$ or possibly $\left.\frac{8}{37}\right)$. Bijugate of same series in an Echinocactus $\left(\frac{2}{9\times 2}\right)$.

IV. Series, $\frac{1}{5}, \frac{1}{6}, \frac{2}{11}, \frac{3}{17}$, &c.

Two Echinocacti $\left(\frac{2}{11}\right)$.

V. Series, $\frac{1}{6}, \frac{1}{7}, \frac{2}{13}, \frac{3}{20}$, &c.

A Cereus? and Mammillaria? $\left(\frac{2}{13}\right)$.

VI. Series, $\frac{1}{7}, \frac{1}{8}, \frac{2}{15}, \frac{3}{23}$, &c.

Melocactus and Echinocactus $\left(\frac{2}{15}\right)$.

VII. Series, $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c.

Echinocactus? $\left(\frac{5}{12}\right)$. Bijugate of same series in the middle region of a cone of *Pinus Lambertiana* in the Museum, Edinburgh Botanic Garden $\left(\frac{5}{12\times 2}\right)$; the two parallel spirals,

here, ran to the right hand, while the single spiral at top and bottom of the cone $\left(\frac{5}{23}\right)$ was left-handed.

VIII. Series, $\frac{1}{2}, \frac{3}{7}, \frac{4}{9}, \frac{7}{16}$, &c.

Echinocactus $\left(\frac{7}{16}\right)$.

IX. Series, $\frac{1}{3}, \frac{2}{7}, \frac{3}{10}, \frac{5}{17}$, &c.

Echinocactus $\left(\frac{5}{17}\right)$.

X. Series, $\frac{1}{4}, \frac{2}{9}, \frac{3}{13}, \frac{5}{22}$, &c.

Cone of *Pinus Pinaster*, in Museum of Edinburgh Botanic Garden, $\left(\frac{5}{22}\right)$.

Dr Dickson drew special attention to five flower spikes of *Banksia occidentalis,* which he had examined from the Edinburgh Botanic Garden. These he found to exhibit four distinct arrangements. One had fourteen vertical rows of bracts, from alternate whorls of seven; two presented thirteen verticals, from a $\frac{2}{13}$ arrangement; one had also thirteen verticals, but from a $\frac{5}{13}$ arrangement; the fifth had twelve verticals, from a $\frac{5}{12}$ arrangement.

2. On the Old River Terraces of the Spey, viewed in connection with certain proofs of the Antiquity of Man. By the Rev. Thomas Brown, F.R.S.E.

Abstract.

The author referred to the paper which he had read on the terraces of the Earn and Teith,* and then described similar deposits which he had observed last autumn on the Spey, giving examples with drawings, from the neighbourhood of Kingussie, Dalvey, and Ballindalloch. The arguments formerly adduced† were equally con-

* Trans. Roy. Soc. Ed. xxvi. 149. † Ibid. 154–163.

clusive in the Spey to show that these terraces were not old sea beaches nor lake margins, but the fluviatile deposits of some former epoch when the floods rose to a greater height. The problem then came to be, In what way are we to explain the action of the river in throwing up deposits 60, 80 feet, or even more above its bed? There are two ways, in one or other of which this may be accounted for,—either by supposing the river bed to have lain on its present level, and allowing rainfall sufficient to flood the channels up to the requisite height; or by supposing the bed of the stream to have been formerly at a higher level, and that, after forming the terraces, the current had excavated its bed down to where it now is. It is the second of these views which has found most favour among geologists, and various suggestions have been offered as to how the bed of the stream was formerly elevated.

One explanation is, that at the time of the highest terrace, the line of the valley, then comparatively shallow, was occupied by the original rock, still to a great extent *in situ*. In regard to our Scottish valleys this explanation is inadmissible. It was formerly shown, from the position of the boulder clay,* that the rocky structure of these river-courses had been hollowed out nearly as deep as now previously to the formation of the terraces; but apart from the Boulder clay the terraces themselves, as will be shown, prove the same thing, for example, the 70 feet terrace at Kingussie.

Another explanation is, that during the last submergence of Scotland the valleys had been filled by marine gravels, &c., and that the river bed had been thus lifted to the requisite height. This view, however, must also be set aside, because after that submergence, the valleys of Scotland were occupied by glaciers, which must to a great extent have cleared out these previous marine deposits.† Especially must this have taken place in Strathspey, lying so high above the sea, and connected with the central mountain-masses of the country. The glacier must have ploughed out the marine debris. It was after that the terraces were formed.

There is a third suggestion, that the river had raised itself on its own alluvium, formed the terraces, and then re-excavated its

* Trans. Roy. Soc. Ed., vol. xxvi., 171.

† Sir C. Lyell's Antiquity of Man, p. 206. Scenery of Scotland, by Mr Geikie, p. 347

bed. But here, again, the objections are equally decisive. *First*, the raising of a river bed in this way seems to take place only when the current has reached some comparatively level part of its course, as in the Po or Nile. The Spey is remarkable for the steep incline of its bed. The Ordnance Survey * shows that for nearly 30 miles below Grantown it goes down more than 600 feet,—fully 20 feet a mile. The current is strong, the old terraces are high. The idea is not for a moment to be thought of that it could have acted as the sluggish rivers which silt up their beds. But, *secondly*, how did the river, after silting up its bed, and raising itself, come to change its action, and cut its way down? Is any such case on record applicable to any river course as a whole? If such a revolution of river action be exceptional, or if it be unknown in nature, we should surely not be warranted in applying it to the rivers of Scotland generally at the period of the terraces.

Thus the idea that the river bed had formerly been elevated is encompassed by difficulties. In whatever form the explanation is put, objections at once suggest themselves which would appear to be fatal.

Turning to the other view, that the river had flowed on its present level, we find that the one great difficulty is the vast amount of water which would be needed to flood the channels up to the requisite height. Mr Prestwich, referring to the Somme and some English rivers, has calculated that it would require 500 times the present flow of the stream to form the 80 feet terrace.† When we look closely into the matter, however, this difficulty diminishes. The result of 500 : 1 is obtained by taking the present flow of the Somme at 800 square feet sectional area. That represents the river when *not* in flood. As the 80 feet terrace, however, is admittedly the work of the old river when *in* flood, we must take the present Somme also in flood, and that is not 800 but 3000 square feet (Prestwich).‡ The effect of this first correction is to bring the 500 : 1 down to 133 : 1. But, further, when Mr Prestwich comes to put all the facts together, he estimates the old Somme at a little more than five times the present—16,000§ against 3000 of

* As yet unpublished; but these results were obligingly communicated by Col. Sir H. James, F.R.S.

† Phil. Trans., vol. cliv., p. 265. ‡ Ibid., 292. § Ibid.

sectional area—and the result is, that if we compare his own view with that which he ascribes to his opponents, the 133 : 1 is further diminished to 25 : 1. But there is a still more important fact to be taken into account. In calculating the sectional area of the old river the whole valley is assumed as empty; but this it cannot have been, at least here in Scotland. If the rocky structure of the valleys was excavated, and the rock removed, how shall the floods be raised high enough to form the terraces? There only remain water and alluvium to fill the space. The only reasonable view is that the area of the valley was to a large extent occupied by masses of alluvium since removed. And this is borne out by what we actually find—fragments of old gravelly platforms left standing to tell of deposits which evidently were at one time far more extended. A third correction, not less important than the others, must be on this ground applied to Mr Prestwich's calculation. So far from the valley having been empty, it must to a great extent have been filled with alluvial deposit since denuded. The difficulty raised as to the volume of the old floods is thus to a great extent set aside.

At various points along the Spey—Kingussie, Coulnakyle, Cromdale—transverse sections of the valley were given, showing the height of the terraces. From the width of the valley in these cases (of which details were given) it appeared that a calculation like that of Mr Prestwich in the Somme would bring out results equally incredible as to the old floods, but owing to the above corrections this difficulty is removed, and the remarkable thing is that the 70 feet terrace at Kingussie has been laid open in an old river course, and the 80 feet terrace at Cromdale in a railway cutting so as to bring out similar results to those formerly shown from the valley of Monzie.* Explain the matter how we may, the river, with an open valley three-fourths of a mile wide, has begun at the bottom, on the level of its present bed, and piled up these deposits to the height of 70 or 80 feet. That they are the work of the river is proved by the way in which the platform-like surface of the terrace slopes down the stream.

The idea of ascribing these high-lying terraces simply to the greater flooding power of some former time was suggested by a comparison between the deposits of the Ruchil with those of the

* Trans. Roy. Soc. Ed., vol. xxvi. pp. 171, 172.

Upper Earn, and of the terraces of Loch Lubnaig with those of Loch Earn, as formerly explained.* It is confirmed by the terraces of the Spey, and more especially by the failure of all the other explanations.

Our knowledge of this whole series of deposits is as yet far too imperfect to allow of anything like a complete theory of their formation. If a suggestion might be offered, perhaps the course of events may have been something like this. When the glacial epoch ended, and the covering of ice and snow melted off Scotland, there would be no small amount of debris over the face of the country, and, unprotected by vegetable covering, it would be washed down into the valleys. Every one admits that the rivers of that age were larger than now—how much larger it is difficult to say. If the Spey had five times its present volume (as Mr Prestwich suggests in the case of the Somme) it would, judging from the present force of its current, assuredly keep its central channel open whatever the amount of debris which came down into the valley. River-like, it would form its banks, and spread out its haughs up to the height to which its floods could rise, *when confined to its comparatively narrow channel.* In the case supposed that height may have been great; and these old high terraces may be the fragments of alluvial platforms, which once spread out along the valley, where the old floods had raised them. Before the whole facts are fully explained, it seems probable that our ideas of the amount of water present in these old floods may have to be enlarged.

The bearing of these facts on certain arguments for the antiquity of man was considered, with special reference to the Spey deposits. There are gravel beds along the Somme in France, which, up to the height of 80 feet, contain flint weapons, which are held to be of human manufacture; and the argument is, that the river has excavated through the rock the valley in which it now flows—that this has been done since the deposition of the gravels, and to allow time for such excavation their age, and consequently the human period, must be carried back into some vast antiquity.

But here is an important fact, which the deposits of the Spey make still more clear in some respects than those of the Earn and

* Trans. Royal Soc. Edin., vol. xxvi, 163 166.

Teith. Along our Scottish rivers there are similar high gravels, 80 feet or more above the stream; and it is known that, previously to the time of their formation, the rocky structure of our valleys had already been hollowed out nearly as deep as now. This is shown at Kingussie, where the 70 feet terrace—and at Cromdale, where the 80 feet terrace—are seen resting on the rock nearly on a level with the river-bed. If, then, with the rocky bed down on its present level, the Scottish streams have managed *somehow* to form those high-lying deposits, why may not the French rivers have done the same? In that case, the Somme would require no time for the subsequent excavation of its valley, and the human period, so far as this argument is concerned, may not be so long after all.

The force of this does not depend on the correctness of the views stated above as to the formation of these terraces. *Whatever* was the way in which the Scottish rivers went to work, it was after the rock had been excavated, and the question would still be, why may not the French rivers have done the same?

One point seems clear, that the case of the French gravels must be shown to differ from those of Scotland before the advocates of extreme antiquity can prove their case from the Somme. After admitting the case in Scotland, if a distinction is to be made in regard to France, the burden of proof will lie with them. The probabilities would certainly seem to be against them. Rivers and valleys have the same laws in different countries. If the French rivers be alleged to have acted differently from the Scottish it may have been so, but the grounds of the difference would need to be adequate, and the proof clear. In the present case, the alleged distinction has reference altogether to the excavation of the rock. In France, they say it had to be done subsequently to the time of the terraces; in Scotland, it must be admitted to have been done before. Are there any grounds on which such a distinction can be made good? Was there such a difference in the formation of valleys between Scotland and France?

It will not be alleged that the soft texture of the chalk rock of the Somme, as contrasted with our harder rocks, can form the ground of distinction. In France itself the same valley-systems traverse many different kinds of rock.

Nor can it be said that the submergence of Scotland as contrasted with the area of the Somme, which was not submerged, can constitute the difference, for Mr Prestwich has shown* not only that the French system of valleys has crossed into the south of England, but that it prevails indifferently as much beyond as within the line of submergence traced by Sir C. Lyell. That submergence seems in this respect to make no difference.

It is equally in vain to allege that the large amount of alluvium in the Scottish valleys makes such a ground of distinction when contrasted with the lesser amount of such deposits on the Somme. The alluvium along our Scottish streams is a very variable quantity as between valley and valley, and as between different portions of the same valley. On the other hand, the amount of the Somme gravels at Amiens and above it, is great—so great, that both Mr Prestwich and Sir Charles Lyell argue in favour of their antiquity, from the length of time which must have been needed to accumulate such a volume of debris.† On the Oise also, and some neighbouring streams, the amount of alluvium is described as very great.

It is enough, however, to remark, that the burden of proof lies with the advocates of antiquity, and that its difficulties have not been surmounted. On the other hand, there is one thing which they may fairly be asked to do—if they maintain that the French and Scottish valleys have been formed on different principles—to show where the two systems meet. The French method, as we have seen, crosses into England. No one will maintain that the Scottish stops at the Tweed. Somewhere they must come in contact. It would be instructive if some one would try to show us two conterminous valleys wrought on the opposite plans. The attempt would probably evince the impossibility of drawing such a distinction. In all that is important, the French and Scottish valley systems go together.

The whole of these remarks are submitted as suggestions, showing the need of much more complete investigation. On this whole series of deposits we have much to learn,—far too much to admit of anything like confident conclusions being drawn as yet. The only safe course is to await the results of future research.

* Phil. Trans., vol. cliv. Pl. iv.

† Prestwich, *ut sup*, 286. Sir C. Lyell, "Antiq. of Man," p. 144.

If difficulty be still felt in regard to the amount of water required for those old floods, we might appeal to the kind of proof by which the existence of a former glacial epoch in Scotland is established. Who that looked to the present ice and snow of a Scottish winter, could think it likely that glaciers once filled the valleys of the Pentlands, and that masses of moving ice rose over the flanks of Arthur's Seat. We point to the rounded and striated rocks, and say, there are the foot-prints of the old glacier,—and the thing is proved, no matter how different may be the cold of our present winters. And why not reason thus in regard to the old floods? Who that looks on the present flow of our streams could realise floods able to raise those old 80 feet terraces? But why should we not point to these deposits where they lie, and say, these stratified gravels and bedded sands are the workmanship of the old currents, which once swept and eddied at that height down these valleys. If this kind of evidence makes you believe in the great old glacier all unlike our present ice, why should not similar proof make you believe in the great old floods of a former epoch, all unlike though they may be to our present streams?

And yet in Strathspey, with the traces of the Moray floods all around us, it is easier to believe these things than it would be almost anywhere else. It was at Coulnakyle, the scene of one of these drawings, that Captain M'Donald, R.N., a sailor of the old school, looked out and saw the Spey, about a mile wide, covered with waves, that put him in mind of Spithead in a fresh gale, and felt sure, as he told Sir T. D. Lauder, that he could have sailed a fifty-gun ship from Boat of Garten to Bellifurth, a distance of seven miles. The small burn of Drumlochan, which in its ordinary state "is hardly sufficient to keep the saw-mill going," rose till it swept away two bridges of twenty feet span, the column of water being estimated at 400 square feet sectional area. As the miller of Dalnabo expressed it, "the height the burns rose to that day was just a' thegither ridiculous." In looking back to the time of these old deposits, it is generally admitted that the volume of the rivers was decidedly greater than it is now. Mr Prestwich, as we have seen, assumes that the old Somme was five times the present. If we might suppose something like this in the Spey—if, further, there was along the valley an amount of alluvium sufficient to confine

the stream to its own channel—and if, from whatever cause, there came floods which would do in proportion for the enlarged Spey what the floods of 1829 did for the Drumlochan Burn, it does not appear as if the solution of the problem as to the formation of these high terraces should be difficult. It is in this direction that the solution is to be sought.

Monday, 3d April 1871.

Professor KELLAND in the Chair.

The following Communications were read :—

1. On the Gravid Uterus and the Arrangement of the Fœtal Membranes in the Cetacea. By Professor Turner.

(*Abstract.*)

In this memoir the author described the dissection of the gravid uterus of an Orca gladiator, for which he was indebted to Mr James Gatherer of Lerwick. The paper contained an account of the uterus and appendages, the fœtal membranes, the position and general form of the fœtus, and a comparison of the placentation with that of other mammals possessing the diffused form of placenta. The structure of the uterine mucous membrane, its subdivision into a gland layer and a crypt layer, the relations of the glands to the crypts, their structure, the arrangement of their blood-vessels, and the much greater vascularity of the crypts than of the glands, were especially described. The chorion, though with diffused villi, possessed not only a small non-villous part at each pole, but a third larger bare spot opposite the os uteri internum; the non-villous spots corresponded, therefore, to the three uterine orifices. The arrangement and structure of the villi, the relations of the vessels to them and to the chorion generally were described; the plexus of capillaries within the villi became continuous with a network, termed sub-chorionic, situated immediately beneath the intervillous part of the chorion, from this latter plexus the rootlets of the umbilical vein arose. The intra-villous capillary plexus lay in relation to the system of capillaries situated in the walls of the uterine

crypts, whilst the sub-chorionic lay in relation to the capillaries situated beneath the plane of the general uterine mucous surface. The amnion formed a continuous bag from one horn of the chorion to the other, but did not reach the poles of the latter. In the left horn, which contained the fœtus, it extended to 2 inches, in the right to 9 inches from the corresponding pole of the chorion, its free surface was studded with small pedunculated corpuscles. The allantois was not so extensive as the amnion. The urachus expanded into a large funnel-shaped sac, which bifurcated when it reached the chorion and formed a right and left cylindrical horn; the left reached to 7 inches from the left pole of the chorion, the right to 21 inches from the right pole.

2. Note on some Anomalous Spectra. By H. F. Talbot.

A recent number of Poggendorff's "Annalen" contains a short but interesting paper by Christiansen, of Copenhagen, in which he states that a hollow prism filled with the alcoholic solution of fuchsine produces a highly anomalous spectrum, which, instead of proceeding regularly from the red to the violet like the ordinary solar spectrum, stops at a certain point, returns backward, then stops again and resumes a direct course to the end. This paper by Christiansen, kindly pointed out to me by Professor Tait, recalls to my memory an experiment which I formerly made more than thirty years ago, and which, with the permission of the Society, I will briefly describe, premising, however, that I write from memory, and without access at present to the original paper which I believe I have still preserved. My account may therefore contain some inaccuracies, but the general nature of the experiment was as follows:—I prepared some square pieces of window gless, about an inch square. Taking one of these, I placed upon it a drop of a strong solution of some salt of chromium, which, if I remember rightly, was the double oxalate of chromium and potash, but it may have been that substance more or less modified. By placing a second square of glass on the first, the drop was spread out in a thin film, but it was prevented from becoming too thin by four pellets of wax placed at the corners of the square, which likewise served to hold the two pieces of glass together. The glasses were then laid aside for some hours

until crystals formed in the liquid. These were necessarily thin, since their thickness was limited by the interval between the glasses. Of course the central part of each crystal, except the smallest ones, was bounded by parallel planes, but the extremities were bevilled at various angles, forming so many little prisms, the smallest of them floating in the liquid. When a distant candle was viewed through these glasses, having the little prisms interposed, a great number of spectra became visible, caused by the inclined edges. Most of these were no doubt very imperfect, but by trying the glass at various points, some very distinct spectra were met with, and these could with some trouble be isolated by covering the glass with a card pierced with a pin-hole. It was then seen that each prism (or oblique edge of crystal) produced two spectra oppositely polarised and widely separated. One of these spectra was normal; there was nothing particular about it. The colours of the other were very anomalous, and, after many experiments, I came to the conclusion that they could only be explained by the supposition that the spectrum, after proceeding for a certain distance, stopped short and returned upon itself.

No accurate measurements, however, were made, because it always happened that, after the lapse of a minute or two, the crystals dissolved in the surrounding liquid, owing to the warmth of the hand or eye. The presence of the liquid, however, was necessary to give the crystals the requisite transparency, and, moreover, the liquid virtually diminishes the angle of the prism floating in it, which otherwise would be too great to give a good result. I never published this experiment, because I found it delicate and capricious, and I was reluctant to publish any facts that might be difficult for others to verify. But I have several times described it to Sir D. Brewster in conversation, and he always said that he thought it very important, at the same time suggesting that there might perhaps be some fallacy. This was because he doubted the possibility of a spectrum being partially inverted or returning on itself. But this doubt seems now to be wholly removed by Christiansen's experiment, in which there seem to be two inversions in the spectrum, and therefore I no longer hesitate to state the grounds on which I concluded long ago that this phenomenon was possible.

Writing entirely from memory, it is possible that I may have fallen into some inaccuracies in this brief account, which, if it should be the case, I trust the Society will, under the circumstance. kindly excuse.

P.S.—Since the above remarks were written, the first number of Poggendorff's "Annalen" for the present year has been received in Edinburgh. This contains a long article by Kundt on the subject of Christiansen's experiment.

He finds that anomalous spectra are given by all the aniline colours, and by permanganate of potash. Such spectra turn back upon themselves, generally having the green at one extremity, the blue being situated between the green and the red.

Hence this property is possessed by an extensive class of bodies, and must form a new and separate branch of optics. He says that the phenomenon only occurs when a very strong solution of the substance is employed in the form of a liquid prism of 25°. But only the thin extreme edge of the prism is available, the thickness of the rest rendering it opaque. He failed in the attempt to form a solid prism by mixing collodion with the alcoholic solution, but this might perhaps be achieved by other means. In the meantime a wide field of experiment is open.

3. Laboratory Notes. By Professor Tait.

1. On Anomalous Spectra, and on a simple Direct-vision Spectroscope.

When I first saw Le Roux's account of his very singular discovery of the abnormal refraction of iodine vapour, I was inclined to attribute the phenomenon to something similar to over-correction of an achromatic combination. In fact, if a hollow prism be filled with a mixture of two gases or vapours, one of which is more refractive than air, the other less refractive; while the second body is more dispersive than the first; it is easy to see that Le Roux's result might be obtained, although each of the substances employed is free from anomalous refractive properties. In a recent conversation with Mr Talbot, I happened to mention the subject, and I learned from him his remarkable observation just laid before the Society. I have since, when I had an opportunity,

made several trials with hollow prisms and prismatic vessels, using various substances, such as oils of cassia and turpentine, toluol, alcohol, saturated solutions of salts, &c., with the view of imitating, with nearly transparent substances, the singular results obtained by Talbot, Christiansen, and Kundt. The observations are certainly very easy in one sense, though very laborious in fact; but I have already produced a spectrum doubled on itself, and have no doubt that with patience I shall be able to produce one with two and even more inversions; though, of course, the more numerous are the inversions the smaller is the scale of the whole phenomenon. The easiest method seems to be to put into a hollow prism a mixture of two substances of very different refractive powers, and to immerse it in a prism or trough containing a substance of intermediate refractive power. When a trough is employed, an external glass prism may with advantage be used along with the combination. The sought phenomenon is, of course, obtained best near the point of adjustment for achromatism, and is in fact very closely connected with the investigations of Dr Blair in his attempts to improve the achromatic telescope by using fluid lenses.

One of my hastily set-up combinations (of two liquids only) gave me a direct-vision spectroscope complete, more powerful than one of Browning's excellent instruments with five glass prisms, and I have little doubt that in this way very good results may be obtained. But, if it be needful to examine only a small region of the spectrum at a time, practically unlimited dispersion may be obtained by using so very simple a combination as two approximately isosceles flint prisms of small angle with their edges together and their adjacent faces inclined at an angle approaching to 180°, so as to form a hollow prism to be filled with oil of cassia. In fact, the dispersion is in this case easily seen to be nearly proportional to the tangent of half the angle of the oil prism. If two kinds of glass, of very different dispersive powers, but of nearly equal mean refractive powers, could be obtained, a permanent combination might be easily formed on this plan, giving as much dispersion as a very long train of ordinary prisms, and losing scarcely any light. A slight inclination of the ends to one another will enable us to use ordinary flint and crown for the purpose, except in so far as total reflection may interfere. Such a combination, adjusted for the red

ray C, seems to promise to be of considerable use in observations of the sun's atmosphere. A somewhat similar result may be obtained by using a single large prism, one of whose faces, employed for total reflection, has a very slight cylindrical curvature.

2. On a Method of illustrating to a large Audience the Composition of simple Harmonic Motions under various conditions.

I have often felt the difficulty of illustrating, by means of Airy's Wave Machine, and various other complex instruments of a similar character, the composition of plane polarised rays into a single elliptically or circularly polarised one; the difficulty arising chiefly in showing separately, but in close succession, to the audience the two vibrations which are to be compounded, and their resultant. Lissajoux's apparatus would exactly answer the purpose if we had tuning-forks vibrating 10 or 15 times a second, its sole defect being the extreme rapidity with which differences of phase are run through; and, in fact, I have tried metronome pendulums with mirrors attached to them; but I have since found the following arrangement to be much more satisfactory. It consists simply in using plane mirrors rotating about axes very nearly perpendicular to their surfaces. A ray reflected almost normally from each of two such mirrors, equally inclined to their axes, and rotating in opposite directions with equal angular velocities, has communicated to it a simple harmonic vibration, whose line and phase can be adjusted at pleasure by a touch. Two such systems of pairs of mirrors, connected by elastic bands with an axle driven by hand, enable the operator to illustrate every combination of two simple-harmonic motions, as well as of circular and elliptic vibrations. By an obvious adjustment it is easy to use, instead of equal periods of vibration, periods bearing any desired relation to one another; and by crossing one or more of the bands we reverse the direction of rotation in the corresponding shafts. It is absolutely necessary to have adjusting screws by which to regulate the inclination of each mirror to its axis.

3. On a simple Mode of explaining the Optical Effects of Mirrors and Lenses.

It is very singular to notice how small a matter makes the difference between the intelligibility and unintelligibility of a demon-

stration to an audience as a whole not mathematical. In no part of Physics have I found this so marked as in the most elementary portions of geometrical optics. Such a formula as

$$\frac{1}{u} + \frac{1}{v} = \frac{2}{r},$$

when interpreted directly as signifying that "the sum of the reciprocals of the distances of the object and image from the surface of a concave spherical mirror, is equal to double the reciprocal of the radius of the mirror," if understood at all, is understood as a sort of *memoria technica* which enables the student to make calculations; but unless he have some knowledge of mathematics it suggests absolutely no higher meaning. If, however, we give to the various terms of the formula their meanings in terms of the *divergence* of the incident and reflected beams, and of the normals to the reflecting surface, even the non-mathematical student easily understands the relation signified. I am indebted to Mr Sang for a reference to Lloyd *On Light and Vision*, 1831, in which this mode of presenting the subject is introduced, but I think the term "vergency" there used is hardly so convenient as the more commonly employed word divergence. Our fundamental optical fact is that to produce the most distinct vision rays must diverge as if from a point about ten inches from the eye. No one has any difficulty in understanding this. As my object has been merely to mention to the Society what I have found to be a method (however trivial in itself, yet) of really considerable importance in teaching, I need do no more than give one simple example of its application, and that only to direct pencils of such small divergence that spherical aberration may be neglected. A perfectly obvious set of modifications is introduced when we treat of oblique pencils, and pencils of large divergence, but students capable of understanding these do not require the adoption of such elementary methods of explanation.

Take, then, the case of light refracted at a concave spherical surface, bounding a substance denser than air. If the incident and refracted rays make (small) angles α and β with the axis of the surface, and if γ be the angle between the normal at the point of

incidence and the axis, these angles being the respective divergences, we have *rigorously* by the law of refraction

$$\sin(\gamma - a) = \mu \sin(\gamma - \beta),$$

or, *approximately*,

$$\gamma - a = \mu(\gamma - \beta),$$

or

$$\mu\beta - a = (\mu - 1)\gamma \quad . \quad . \quad . \quad . \quad (1),$$

where μ is the refractive index. [This we may, if we choose, translate into

$$\left(\frac{\mu}{v} - \frac{1}{u}\right)y = \frac{\mu - 1}{r}y,$$

where y is the distance of the point of incidence from the axis, and the rest of the notation is as usual. In this form we see that, to our approximation, the result is independent of y.]

In (1) we have $\gamma = 0$ for a plane surface, and $\mu = -1$ when there is reflection instead of refraction.

Hence for a reflecting surface the meaning of (1) is—"the sum of the divergences of the incident and reflected rays is twice that of the normals to the surface." If the incident rays be parallel, the reflected rays diverge twice as much as do the normals.

At the second surface of a thin lens (1) becomes

$$\frac{1}{\mu}\beta' - \beta = \left(\frac{1}{\mu} - 1\right)\gamma',$$

which, compounded with (1), gives

$$\beta' - a = (\mu - 1)(\gamma - \gamma'),$$

which may be thus translated—"A lens produces *a definite change of divergence* on any direct pencil—and the change is $\mu - 1$ times the difference of the divergences of the normals to its surfaces."

Hence that a divergence may be changed into an equal *negative* divergence, it must be equal to half the change produced by the lens; *i.e.*, when the object and image are equidistant from the lens, their common distance from it is double the focal length of the lens.

4. On the Structure of the *Palæozoic Crinoids*. By Professor Wyville Thomson.

(*Abstract.*)

The best known living representatives of the Echinoderm Class Crinoidea are the genera *Antedon* and *Pentacrinus*—the former the feather stars, tolerably common in all seas; the latter the stalked sea lilies, whose only ascertained habitat, until lately, was the deeper portion of the sea of the Antilles, whence they were rarely recovered by being accidentally entangled on fishing lines. Within the last few years Mr Robert Damon, the well-known dealer in natural history objects in Weymouth, has procured a considerable number of specimens of the two West-indian *Pentacrini*, and Dr Carpenter and the author had an opportunity of making very detailed observations both on the hard and the soft parts. These observations will shortly be published.

The Genera *Antedon* and *Pentacrinus* resemble one another in all essential particulars of internal structure. The great distinction between them is, that while *Antedon* swims freely in the water, and anchors itself at will by means of a set of "dorsal cirri," *Pentacrinus* is attached to a jointed stem, which is either permanently fixed to some foreign body, or, as in the case of a fine species procured off the coast of Portugal during the cruise of the Porcupine in the summer of 1870, loosely rooted by a whorl of terminal cirri in soft mud. Setting aside the stalk, in *Antedon* and *Pentacrinus* the body consists of a rounded central disc and ten or more pinnated arms. A ciliated groove runs along the "oral" or "ventral" surface of the pinnules and arms, and these tributary brachial grooves gradually coalescing, terminate in five radial grooves, which end in an oral opening, usually subcentral, sometimes very excentric. The œsophagus, stomach, and intestine coil round a central axis, formed of dense connective tissue, apparently continuous with the stroma of the ovary, and of involutions of the perivisceral membrane; and the intestine ends in an anal tube, which opens excentrically in one of the interradial spaces, and usually projects considerably above the surface of the disc. The contents of the stomach are found uniformly to consist of a pulp

composed of particles of organic matter, the shields of diatoms, and the shells of minute foraminifera. The mode of nutrition may be readily observed in *Antedon*, which will live for months in a tank. The animal rests attached by its dorsal cirri, with its arms expanded like the petals of a full-blown flower. A current of sea-water, bearing organic particles, is carried by the cilia along the brachial grooves into the mouth, the water is exhausted in the alimentary canal of its assimilable matter, and is finally ejected at the anal orifice. The length and direction of the anal tube prevents the exhausted water and the fœcal matter from returning at once into the ciliated passages.

In the probably extinct family Cyathocrinidæ, and notably in the genus *Cyathocrinus*, which I take as the type of the Palæozoic group, the so-called CRINOIDEA TESSELLATA, the arrangement, up to a certain point, is much the same. There is a widely-expanded crown of branching arms, deeply grooved, which doubtless performed the same functions as the grooved arms of *Pentacrinus;* but the grooves stop short at the edge of the disc, and there is no central opening, the only visible apertures being a tube, sometimes of extreme length, rising from the surface of the disc in one of the interradial spaces, which is usually greatly enlarged for its accommodation by the intercalation of additional perisomatic plates, and a small tunnel-like opening through the perisom of the edge of the disc opposite the base of each of the arms, in continuation of the groove of the arm. The functions of these openings, and the mode of nutrition of the crinoid having this structure, has been the subject of much controversy.

The author had lately had an opportunity of examining some very remarkable specimens of *Cyathocrinus arthriticus*, procured by Mr Charles Ketley from the upper Silurians of Wenlock, and a number of wonderfully perfect examples of species of the genera *Actinocrinus*, *Platycrinus*, and others, for which he was indebted to the liberality of Mr Charles Wachsmuth of Burlington, Ohio, and Mr Sidney Lyon of Jeffersonville, Indiana; and he had also had the advantage of studying photographs of plates, showing the internal structure of fossil crinoids, about to be published by Messrs Meek and Worthen, State Geologists for Illinois. A careful examination of all these, taken in connection with the description

by Professor Lovén, of *Hyponome Sarsii*, a recent crinoid lately procured from Torres Strait, had led him to the following general conclusions.

In accordance with the views of Dr Schultze, Dr Lütken, and Messrs Meek and Worthen, he regarded the proboscis of the tesselated crinoids as the anal tube, corresponding in every respect with the anal tube in *Antedon* and *Pentacrinus*, and he maintained the opinion which he formerly published (Edin. New Phil. Jour., Jany. 1861), that the valvular "pyramid" of the Cystideans is also the anus. The true mouth in the tesselated crinoids is an internal opening vaulted over by the plates of the perisom, and situated in the axis of the radial system more or less in advance of the anal tube, in the position assigned by Mr Billings to his "ambulacral opening." Five, ten, or more openings round the edge of the disc lead into channels continuous with the grooves on the ventral surface of the arms, either covered over like the mouth by perisomatic plates, the inner surface of which they more or less impress, and supported beneath by chains of ossicles; or, in rare cases (*Amphoracrinus*), tunnelled in the substance of the greatly thickened walls of the vault. These internal passages, usually reduced in number to five by uniting with one another, pass into the internal mouth, into which they doubtless lead the current from the ciliated brachial grooves.

The connection of different species of *Platyceras* with various crinoids, over whose anal openings they fix themselves, moulding the edges of their shells to the form of shell of the crinoid, is a case of "commensalism," in which the mollusc takes advantage for nutrition and respiration of the current passing through the alimentary canal of the echinoderm. *Hyponome Sarsii* appears, from Professor Lovén's description, to be a true crinoid, closely allied to *Antedon*, and does not seem in any way to resemble the Cystideans. It has, however, precisely the same arrangement as to its internal radial vessels and mouth which we find in the older crinoids. It bears the same structural relation to *Antedon* which *Extracrinus* bears to *Pentacrinus*.

Some examples of different tesselated crinoids from the Burlington limestone, most of them procured by Mr Wachsmuth, and described by Messrs Meek and Worthen, show a very remarkable

convoluted plate, somewhat in form like the shell of a *Scaphander*, placed vertically in the centre of the cup, in the position occupied by the fibrous axis or columella in *Pentacrinus* and *Antedon*. Mr Billings, the distinguished palæontologist to the Survey of Canada, in a very valuable paper on the structure of the Crinoidea, Cystidea, and Blastoidea (Silliman's Journal, January 1870), advocates the view that the plate is connected with the apparatus of respiration, and that it is homologous with the pectinated rhombs of Cystideans, the tube apparatus of Pentremites, and the sand-canal of Asterids. Messrs Meek and Worthen and Dr Lütken, on the other hand, regard it as associated in some way with the alimentary canal and the function of nutrition.

The author strongly supported the latter opinion. The perivisceral membrane in *Antedon* and *Pentacrinus* already alluded to, which lines the whole calyx, and whose involutions, supporting the coils of the alimentary canal, contribute to the formation of the central columella, is crowded with miliary grains and small plates of carbonate of lime; and a very slight modification would convert the whole into a delicate fenestrated calcareous plate. Some of the specimens in Mr Wachsmuth's collection show the open reticulated tissue of the central coil continuous over the whole of the interior of the calyx, and rising on the walls of the vault, thus following almost exactly the course of the perivisceral membrane in the recent forms. In all likelihood, therefore, the internal calcareous network in the crinoids, whether rising into a convoluted plate or lining the cavity of the crinoid head, is simply a calcified condition of the perivisceral sac.

The author was inclined to agree with Mr Rofe and Mr Billings in attributing the functions of respiration to the pectinated rhombs of the Cystideans and the tube apparatus of the Blastoids. He did not see, however, that any equivalent arrangement was either necessary or probable in the crinoids with expanded arms, in which the provisions for respiration, in the form of tubular tentacles and respiratory films and lobes over the whole extent of the arms and pinnules, are so elaborate and complete.

5. On the Formation and Decomposition of some Chlorinated Acids. By J. Y. Buchanan.

1. *On the Rate of the Action of a Large Excess of Water on Monochloracetic Acid at* 100° C.—When monochloracetic acid is heated with water, double decomposition takes place, glycollic and hydrochloric acids being formed; and conversely, when glycollic acid is heated with hydrochloric acid, it is converted into monochloracetic acid and water. A similar reaction takes place with the two monochloropropionic and corresponding lactic acids, and probably with all their homologues.

The task which I have set myself is to study these reactions, in so far as they are dependent upon temperature, duration of reaction, and relative mass of reacting substances. In the present communication, I give the results of experimenting upon monochloracetic acid with a very large, practically infinite, excess of water at 100° C.

The monochloracetic acid was purchased from Dr Marquart, of Bonn, and rectified. What passed between 180° and 190° was used for the following experiments:—A watery solution of it was made which contained in a litre 32·4 grms., and showed a specific gravity = 1·0124, whence the chloracetic acid and the water were mixed in the proportion of one molecule of the former to 164 molecules of the latter.

As the increase of the acidity of the solution is the measure of the decomposition which takes place, it is easily determined by titration. For this purpose a solution of caustic soda was generally employed, although in the earliest experiments baryta water was made use of.* The saturating power of these reagents was

* Berthelot (Ann. de Chim. et de Phys. [3], LXV., 401) made use only of baryta, his objections to potash and soda being that they always contain carbonate, and that their salts with organic acids always have a more or less alkaline reaction. The first of these objections may be got rid of by keeping the solution, freed from CO_2 in the first instance by lime water, in a number of *small* bottles filled full up to their tightly fitting corks. The second I have found not to apply to the bodies here in question. There is no doubt, however, that baryta solution does present considerable advantages in the greater ease with which it can be procured in a state of absolute purity; and that any carbonic acid which it may absorb is at once eliminated, thereby, how-

ascertained by means of a very carefully prepared normal sulphuric acid, containing 49 grms. H_2SO_4 in a litre. 10 CC. of this acid saturated 42·7 CC. caustic soda, and 41·8 CC. baryta water, whence one litre caustic soda contains 9·3677 grms. NaHO, and one litre baryta water 20·450 grms. BaH_2O_2. 10 CC. of the above-mentioned chloracetic acid saturated 14·7 CC. caustic soda and 14·4 CC. baryta water.

In every experiment 10 CC. chloracetic acid solution were sealed up in a tube, and introduced directly into the boiling water bath. After the reaction was finished, it was transferred immediately to a vessel of cold water. By this means the time of heating up to 100° and of cooling down again to the surrounding temperature was reduced to a minimum.

The chloracetic acid solution was prepared in the middle of last November, and although it has now stood at the ordinary temperature of the laboratory for over four months, its saturating power has not changed to a sensible extent. It is true, however, that it gives a slight opalescence with solution of nitrate of silver. It appears then that the decomposition of monochloracetic acid by a large excess of water at the ordinary temperature is infinitely slow.

In the experiments at 100° C. the same quantity, namely, 10 CC. of the acid solution, was invariably employed. In the following table showing the results, the first column contains the duration of the experiment in hours; the second the number of CC. caustic soda or baryta water required to saturate the resulting acid, and the third gives the percentage chloracetic acid decomposed as calculated from column 2. No fraction smaller than 0·5 is given, this being the limit of possible errors of observation:—

ever, altering the strength of the solution. My principal objection to it was its great tendency to crystallise even in solutions a long way removed from saturation.

TABLE

TABLE I.—$C_2H_3ClO_2 + 164H_2O$ at 100° C.

Duration of Experiment in Hours.	Number of CC. required for neutralisation.		Percentage of $C_2H_3ClO_2$ Decomposed.
	Soda.	Baryta.	
0	14·70	14·40	0·0
2	15 55	...	6·0
4	16·35	...	11·0
6	16·85	...	14·5
11	18·10	...	23·0
14	18·80	...	28·0
16	19·30	...	31·5
18	19·85	...	35·0
21	20·30	...	38·0
24	20·95	...	42·5
27	21·35	...	45·0
30	22·15	...	51·5
33	22 55	...	53·5
37	22·95	...	56·0
43	23·90	...	62·5
48	24·45	...	66·0
72	...	25·40	76·5
96	...	26·20	82·0
120	27·57	...	87·5
144	28·00	...	90·5
192	28·40	...	93.0
332	28 95	...	97 0
430	29·05	...	97·5

The following Gentlemen were elected Fellows of the Society:—

JAMES GEIKIE, Esq.

THOMAS E. THORPE, Ph. D., Lecturer on Chemistry in the Andersonian Institution, Glasgow.

Monday, 17th April 1871.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. Notes on the Antechamber of the Great Pyramid. Based on the Measures contained in vol. ii. "Life and Work at the Great Pyramid," by C. Piazzi Smyth. By Captain Tracey, R.A. Communicated by St John Vincent Day, Esq., C.E., F.R.S.E.

In considering the authority for the division of the sacred cubit into 25 inches, we have, first, the architectural fact that the Queen's chamber, containing the visible expression of that cubit, stands in or upon the 25th course of masonry, comprising the whole Pyramid. And here, though not strictly bearing on the case, may be mentioned a connection between the lengths of the two passages (the first ascending, and the horizontal passages) leading to that chamber, remarkable when expressed in inches, of which 25 make a cubit.

Thus, the length of the first ascending passage from the axis of descending passage to north wall of Grand Gallery (see p. 54, v. ii., L. and W.)* = 1544·4 B. I., or 1542·9 inches, of which 25 make a sacred or Pyramid cubit, and which for the future we will term "Pyramid inches."

Now, this length of 1542·9 P. I.—25 = 1517·9 P. I.—is the exact length of the horizontal passage from north wall of the Grand Gallery to the north wall of the Queen's Chamber—

$$\left.\begin{array}{r}\textit{E.g.}\text{, length of horizontal gallery (see}\\ \text{p. 57, v. ii., L. and W., last line),}\end{array}\right\} = \begin{array}{r} 1519{\cdot}4 \text{ B. I.} \\ 1{\cdot}5 \\ \hline 1517{\cdot}9 \text{ P. I.} \\ \hline \end{array}$$

* In this paper the following abbreviations are used: "L. and W.," for "Life and Work at the Great Pyramid;" B. I. = "British Inches;" P. I. = "Pyramid Inches" Pyramid Inch = British Inch × 1·001.

But on entering the Antechamber, we find this particular measure or $\frac{\text{sacred cubit}}{25}$ (which we have termed the Pyramid inch, to avoid expressing that particular measure of length by the algebraical x) not only typified, but expressed, and most notably in the granite leaf, whose precise functions have never yet been explained.

For there—on a stone immediately in front of an unmistakable symbol of division into five—we find a raised boss, with a single straight edge exactly $\frac{1}{5}$ of a Pyramid or sacred cubit in length, and consequently representing 5 of these inches.

The thickness of this boss along the whole line of 5 inches is exactly $\frac{1}{5}$ of that line, $\frac{1}{25}$ of the same cubit, or precisely the inch we are in search of.

Further, the centre of this boss is exactly one inch from the middle of the Antechamber, its distance from either side being 19·5 and 21·5 inches from the west and east walls respectively, and, consequently, it is one inch to the west of centre (just as the niche in the Queen's Chamber, marking the whole 25 inch cubit by the breadth of its flat top, is also 25 inches removed from the central vertical line of the wall in which it is formed).

It may be argued that all these expressions of an inch in the Antechamber depend upon the shape and position of a stone that was not necessarily placed there by the architect of the Pyramid.

Let us, therefore, seek some connection with the grander features of the building, both for the stone itself and the particular measure of length, of which we are thus far led to consider it the standard.

The following calculation shows that a line drawn from the angle of the great step at an angle of 26° 18′, or parallel to the true axis of the Grand Gallery, passes about 1·13 inch below the centre of the bottom of the upper stone forming the granite leaf, or the one that bears the boss.

Vol. ii. L. & W. pp. 93, 96.	B. I.
North end of step to north side of leaf (omit boss)	= 134·3
,, south ,,	= 150·55
	2)284·85
Distance of centre of leaf from north end of step,	142·42

Height of bottom of leaf above floor, . .	43·7
,, lower stone of leaf, . . .	27·75
,, junction of the stones above the floor, =	71·45
Now, 142·4 × ·494, or nat. tan. of Grand Gallery angle, =	70·32
	1·13 B.I.

∴ A line ∥ to axis of Grand Gallery, drawn from ∠ of Great Step, passes 1·13 B. I. below centre of joint of leaf.

P. 96 L. & W. This and the next calculation.

Distance of south wall of Antechamber from ∠ of Step = 229·6 B.I.
229·6 × ·494 (nat. tan. Grand Gallery ∠) = 113·42 ,,

show that the same line produced, strikes the south wall of the Antechamber at a height of 113·42 B. I. from the floor. As the boss is to the west of the centre of the room, we turn to that side, and find that the height of the granite wainscot there, where it bears against the south wall, is 111·8 inches or 1·62 B. I. lower than the spot indicated. But, on examining the course of the axis* itself of the Grand Gallery when produced, the following calculation shows that it passes through the lower stone of the leaf at a distance of 0·8 inch below its centre on its northern side, and on being produced strikes the south wall of the Antechamber at a height above the floor of 104·02 B. I., or just an inch above the height of the wainscot on the east side, which reaches an altitude of 103·1 B. I.

Thus connecting the inch, the granite leaf, and the rest of the building in a manner that none but the original Designer could have introduced.

P. 96 L. & W.

North side of leaf (omit boss) from north side of step = 134·3 B. I.

Height of bottom of leaf above floor, . . .	43·7	(P. 99 L. & W.)
One-half height of lower stone,	13·9	,,
Height of centre of lower stone,	57·6	
But 134·3 × ·494	= 66·24	
and axis of ascending passage continued through Grand Gallery is 9·4 B. I. below ∠ of Step†	= 9·4	
	= 56·8	= Height at which axis of Grand Gallery strikes lower stone on north side, or (57·6 − 56·8) or 0 8 B. I. below centre of stone.

* That is, axis of 1st ascending passage continued through Grand Gallery.

† See next calculation.

P. 74 L. & W.

Vertical height of Great Step— East, 35·8 B. I. West, 36·2 — 36· mean.	}	B. I. Height of ‖ axis = 113·42 − 9·4 = 104·02 = Height of true axis of Grand Gallery above the floor.

Vertical height of northern entrance to Grand Gallery (p. 70 L.&W.) is $53{\cdot}2 - \frac{53{\cdot}2}{2} = 26{\cdot}6$ = height of axis which subtracted from 36· =

9·4 = vertical height of $\angle$ of Great Step above the point where the axis of first ascending passage passes into it.

But the axis of the Grand Gallery, the most important line in the whole building, having so signally pointed out the importance of the lower stone of the leaf, let us examine it also in terms of the inches we are led to connect so closely with it. Taking the mean of all the measures given, the calculation following shows that the cubical contents of that part of the stone not sunk in the grooves

$$= 15{\cdot}7 \times 41 \times 27{\cdot}7 = 17830{\cdot}5 \text{ British inches.}$$
$$\phantom{= 15{\cdot}7 \times 41 \times 27{\cdot}7 =} 17{\cdot}8$$
$$= 17812{\cdot}7 \text{ Pyramid inches.}$$

P. 99 L. & W.

Thickness—East end of leaf,		.	15·4
,, West ,,		.	16·
	Mean,	.	15·7 B. I.
Height,		.	27·5
,,		.	28·
	Mean,	.	27·7 B. I.

P. 100 L. & W.

Width, 41 B. I.—this measure being taken on the leaf itself, and on the same side as the boss.

Log. 15·7 = 1·1958997
,, 27·7 = 1·4424798
,, 41· = 1·6127839
= 4·2511634 = log. of 17830·5 British inches.

The Ark, or Laver by theory, and the Pyramid Coffer in practice, contain 71321·25 B. I. = 71,250 P. I., the quarter of which, or 17812·5 Pyramid inches (the volume of this particular stone), is the Chomer or Homer of sacred standard.

The remarkable result thus obtained induces a further examination of the position of this stone.

We remark that the base of this stone (lower stone) is in the same horizontal plane as three other well defined lines of the antechamber—viz., the division between the courses of the wainscot on the east wall, and the tops of the doors in the north and south walls.

It is to be noticed that the refined workmanship of the granite wainscoting has been most fully developed to the south of the leaf.

We will thus examine that portion first. The granite leaf itself and the granite walls mark off above the horizontal plane a certain space.

The dimensions of this part of the plane are—

In length varying from	(1.) 79·0 B.I.	to 79·1 B.I.	
In breadth	(2.) 41·2	to 41·45 B.I.	
While at the height of	(3.) 27·5	to 28	B.I. there

runs across it the joint line of the leaf.

(1.) P. 96 L. & W.—North end of step to south side of leaf, } E. 150·3; W. 150·8

Mean 150·55

North end of step to south end of antechamber, } E. 229·4
Do. do. W. 229·8

Mean 229·6

Length, East side,	229·4	
	150·3	
	79·1	
Do. West,	229·8	
	150·8	Mean 79·05
	79·0	

(2.) P. 93 L. & W.—41·45

$$\begin{array}{l} 41{\cdot}45 \\ 41{\cdot}2 \\ \hline 82{\cdot}65 \\ \hline 41{\cdot}325 \text{ Mean.} \end{array}$$

(3.) P. 99 L. & W.—27·5

$$\begin{array}{l} 27{\cdot}5 \\ 28{\cdot} \\ \hline 55{\cdot}5 \\ \hline 27{\cdot}75 \text{ Mean.} \end{array}$$

The already acquired facts give us good reason to look upon the 25th part of the sacred cubit as an unit of measure that may be safely used in at least the antechamber of the great Pyramid, and we only argue in conformity with other teaching of the Pyramid in assuming that the volume of the lower stone of the leaf *may* also be an *unit* of *volume* for antechamber cubical measures.

Thus if we take the lowest readings, a cubical space of 27·5 × 41·2 × 79·0 B.I., or (1.) 89507·0 B.I. is marked out; or (2.) 5·019 of our volume unit.

$$\begin{array}{llrcl} & & \text{B.I.} & & \\ (1.) & \text{Log. of} & 27{\cdot}5 & = & 1{\cdot}4393327 \\ & \ldots & 41{\cdot}2 & = & 1{\cdot}6148972 \\ & \ldots & 79{\cdot}0 & = & 1{\cdot}8976271 \\ & & \text{B.I.} & & \overline{\phantom{1{\cdot}0000000}} \\ & \ldots & 89507{\cdot}0 & = & 4{\cdot}9518570 \end{array}$$

and (2.) $\dfrac{89507{\cdot}0}{17830{\cdot}5} = 5{\cdot}019$

Practically 5 volumes of the lower stone of the leaf, and therefore $\frac{1}{10}$th of the lower course of the king's chamber.

For that has been shown (by Professor Piazzi Smyth) equal to 2000 baths, or 50 coffers, therefore the space in the antechamber

Equals . . .	50 baths
or	5 chomers
of which last our unit represents .	1

We have consequently the Hebrew chomer standing, as it were, at the end of a measure of 5 times its own capacity, as in the

king's chamber has been found the coffer in one 50 times its own content. The rest of the granite-lined chamber, of which the above formed part, may also be worthy of consideration. Its length and breadth are the same as that of the portion already considered, while its height is determined by that of the containing wainscots. But these, as we have already seen, are determined by the heights at which the south wall is touched, the one by the axis of the (first ascending passage produced through the) Grand Gallery prolonged into the antechamber, and the other by a line parallel thereto drawn from the angle of the great step. But as it would be evidently giving either undue weight to use it alone, let us take (as the following calculation shows) the average height of the two —viz., (1.) 108·72 B.I.

Taking the highest readings of the dimensions, we obtain—(2.), 108·72 × 79·1 × 41·45 B.I., or 356460·4 B.I. (3.), we find therein 19·99, &c. of the units we have seen reason to employ, or so close on 20 as to justify our acknowledging *intention* in the size.

(1.)—H. of ‖ axis,	. .	113·47	
„ grand gallery axis produced		104·07	
		2)217·54	
		108·72	mean.

(2.) Log. of	108·72	=	2·0363094
	79·1	=	1·8981765
	41·45	=	1·6165245
	356460·4	=	5·5520104
	Minus log. 17830·5	=	4·2511634
(3.)	19·99, &c.	=	1·3008470

Granting that, we have another noteworthy connection established between the antechamber and king's chamber, as there the volume of the lower course has been shown (by Professor Smyth) to equal 50 coffers, or 200 of our units, while here we have its tenth part, or 20 units equalling 5 coffers.

It will doubtless be objected that in one instance we have used the highest, and the other the lowest readings of the measures. Just proportion teaches that the product of the means should be of no less value than that of the extremes.

Let us then take the means of those two sets of numbers, whose extremes only we have been using heretofore, and employ them in

connection with other dimensions of that marked horizontal plane already alluded to.

Examination of it shows that it is broadly divided into two portions, by the leaf resting on it; and the linear measures of the two rectangles thus formed are respectively, the northern one—

(1.)		(2.)
41·7	P. 96, V. 2, L. & W.	P. 99, V. 2, L. & W.
41·45	P. 93, "	21·0
41·2	" "	
41·45 mean.		

$$\{(41{\cdot}45 \times 2) + (21{\cdot} \times 2)\} = 82{\cdot}9 + 42 = 124{\cdot}9$$

and the southern one—

(3.) See (1) page 426.* (4.) See (2) page 427.*

$$\{(79{\cdot}05 \times 2) + (41{\cdot}3 \times 2)\} = (158{\cdot}1 + 82{\cdot}6) = 240{\cdot}7$$

British inches, 365·6
·36

or in Pyramid inches, 365·24

roughly divided into ⅓ and ⅔rds of No. of days in a year.

The perimeter of the chamber at the ceiling (363 inches) had pointed out the probability of our finding some of the external proportions of the pyramid repeated here; and as there we find the "year" in terms of 4 cubits, or 100 inches, so here we have a "year" of inches; and as there the grander and external year is intimately connected with the height of the pyramid through π, so here we find, through the same medium, a connection with the length of the chamber, a mean of three measures of which gives 116·32 for its length in pyramid inches, for taking 365·24 as circumference, diameter = 116·26.

P. 95 L. & W.—Length of antechamber, 116·3
... ·8
... ·2

Mean 116·43 British inches.
·11

116·32 Pyramid inches.

$$\begin{aligned} \text{Log. of } 365{\cdot}24 &= 2{\cdot}5625783 \\ \pi &= {\cdot}4971499 \\ 116{\cdot}26 &= 2{\cdot}0654284 \end{aligned}$$

* These numbers refer to pages of this volume.

Or an approximation to π, as represented by a "year" of inches marvellously close both in the numbers representing the circumference and diameter, and reproducing here the grander proportions of the external form of the pyramid.

It is to be remembered that the "year" of inches was divided roughly into $\frac{1}{3}$ and $\frac{2}{3}$ds, and the three stones of the ceiling and the three cuts on the wainscot seem to point to some important division by 3.

We have seen π playing so important a part in deciding the height of the pyramid and the length of the Antechamber, that we may at any rate try what a division by 3 will do.

On the base of the pyramid the "year" which represents circumference (or, as regards the height of the pyramid π) was expressed in units of 100 inches. Have we any chance of finding not circumference, for we already have our "year" of inches, but diameter, or radius, as a purely mathematical expression as regards π, when expressed in say the same terms of 100 inches?

Taking π as represented by 314·159, &c. Pyramid inches, we find diameter + radius expressed very closely, as $\frac{2}{3}$ and $\frac{1}{3}$ of the height of the antechamber (*i.e.*, 149·2″).

But when we divide π itself (still expressed in terms of R = 100 Pyramid inches) by 3, we obtain the figures 104·72, which strike us as being an approximation to the height of the wainscot on the east wall (103·1); but when we refer to the grand gallery axis (to whose connection with the east wainscot our attention has already been drawn) we find a still closer approximation (viz., 104·06 P.I.) to the expression of $\frac{\pi}{3}$.

But $\frac{\pi}{3}$ is a curious expression, and not much used in calculations I am conversant with, except in one instance; but that instance bears on the case, as it is in the calculation of volume of spheres, cones, and also pyramids, the area of whose base is expressed in terms of π.

It may be advantageous to note here the connection between the volumes of pyramids and spheres. The content of a pyramid is mathematically expressed thus.

$$V = \frac{a.h.}{3},$$

where a = area of base,
and h = height of pyramid.

But in the purely mathematical form of pyramid we are led to consider

$$a = \pi R^2$$

$$h = R\left(= \frac{D}{2} = \frac{1}{2}\right), \text{ when V would equal } \frac{\pi R^3}{3} \text{ : but in a sphere,}$$

$$\text{volume} = 4\,\frac{\pi R^3}{3}.$$

So that in the case of the great hemispherical molten sea, whose content = 50 lavers, a pyramid of the same base and height would contain 25 lavers, 100 homers, or five of the largest marked-off space in the antechamber whose content has already been pointed out.

This may certainly lead us to infer, that as up to the antechamber our measures have been lineal and superficial; now, on the other hand, we must be prepared for cubical measures with, perhaps, also some concerning the content of spheres, cones, or pyramids.

Commencing our investigation at the horizontal marked plane previously referred to, we remember in its most highly finished portion that its smallest dimensions are 79·0 B. I. and 41·2 B. I., and here we may notice that their sum $\left(\begin{array}{r} 79{\cdot}0 \text{ B.I.} \\ 41{\cdot}2 \quad\;\; \\ 120{\cdot}2 \text{ B.I.} \end{array}\right)$, 120·2 B.I. or 120 1 P.I. is very close upon the radius of the hemisphere that the presence of $\frac{\pi}{3}$ has led us to refer to. The precise figures standing thus:—

Radius of $\frac{1}{2}$ sphere whose volume = 3,562,500 P.I. (= lower course of King's Chamber = "Molten Sea") is 119·371 P.I.

When vol ume of sphere = 3562500 × 2 cubic inches.

Required its radius:

$$\text{Now V. of sphere} = \frac{4\pi R^3}{3}$$

$$\therefore 7125000 = \frac{4\pi R^3}{3}$$

$$\therefore R^3 = \frac{7125000}{\frac{4\pi}{3}}$$

$$\therefore R = \sqrt[3]{\frac{7125000}{4\cdot1887902}} = 119\cdot371$$

$$\begin{array}{lr} \log. 7\cdot125\cdot000 = & 6\cdot8527849 \\ \log. \frac{4}{3}\pi = & 0\cdot6220886 \\ & 3)\overline{6\cdot2306963} \\ 119\,371 = & 2\cdot0768987 \end{array}$$

But we are getting on too fast. Now in spite of the presence of π are we to suppose the circle squared practically, as we have imagined, when suggesting that the area of the base of a square pyramid might be represented by πR^2?

To seek an answer to that question we must go back to that part of our investigation, where we had reason to believe that the connection between 116·3 and 365·24 was intentionally introduced as an exponent of the relation between diameter and circumference, and we may not unreasonably test the accuracy of our deductions by finding the area of the circle there expressed, trusting that if we are working in the right direction this step may lead to some further proof of its being so.

But in so doing we should use the figures only as a guide to the *intentions* of the Great Architect, and having as we believe learnt that the "year" of inches symbolises a circle of 365·256, &c., we may take as our starting-point the more accurate diameter represented by $\frac{356\cdot256}{\pi}$ or 116·264 pyramid inches.

To proceed.

The area of a circle whose diameter is 116·264 is 10,616·65.

This number in itself does not seem peculiarly suggestive, but when we recollect how remarkably both the east wainscot and granite floor* point to an accurately marked square of 103 Pyramid

* Viz. the east wainscot, a vertical line 103 inches high, and of the floor, a special portion constructed in granite showing a horizontal line 103 inches long.

inches whose area = 10,609, we think we have advanced in the right direction and shown that the builder here places for our instruction and guidance another practical illustration of the importance and use of π, its former application being lineal, and this superficial. And here we stay to point out how these curious proportions, coincidences, and symbols become legible when read by the units of length and volume supplied by the architect of the pyramid himself, and extant (let us hope) to this day in the very spot where their use first becomes imperative.

For though the proportions remain the same whether expressed in inches, feet, or metres, they only become vocal as it were when read by the units there prepared and hung up near them.

What should be the next step in the process of inductive argument?

The sides and perimeter of this square (of 103·0 P.I.) are so obviously connected with the length and breadth of the King's Chamber, as exactly $\frac{1}{4}$, and $\frac{1}{2}$ thereof, that a consideration of the area of *its* floor would perhaps be the next step, guided too by the admonition we fancy we have received on passing through the antechamber, that cubical and not simply linear or superficial measures should occupy us in the chamber ultimately attained.

With what results this has been done over the area of *that* floor, we already know, from Taylor, Smyth, Petrie, and Day, results too so overwhelmingly important, that though the tables of the Law, written by the hand of the Omniscient, have been lost to man, we have here inscribed by the great architect of the pyramid the very essence of all legislation, so exact and so scientific in all its branches, as far as we can penetrate, that it is indeed "ennobling to the mind of man to contemplate."

2. Experiments and Observations on Binocular Vision. By Edward Sang, Esq.

(*Abstract.*)

This communication was chiefly directed to the question whether the idea of distance be obtained from the adjustment of the eyes to distinct vision, or from the convergence of their axes. The case of the chameleon was cited as one in point, since that lizard

directs its eyes each to a separate object, but habitually, when about to strike its prey, brings both eyes to bear upon it. Several experiments, mostly suggested by Wheatsone's inquiries, were cited, and the conclusion was arrived at, that, although the adjustment for direct vision concur in the formation of the estimate of distance, the convergence of the eyes plays the principal part.

3. On the Fall of Rain at Carlisle and the neighbourhood. By Thomas Barnes, M.D.

In this communication, the author offers remarks on journals kept by Dr Carlyle, in the city of Carlisle, from 1757 to 1783 inclusive; by the Rev. Joseph Golding, at Aikbank, near Wigton, Cumberland, from 1792 to 1810 inclusive; and by himself at Bunkers Hill, two and a half miles west of Carlisle, which is situate 184 feet above the sea-level. The author gave tables showing the quantity of rain of each month and year included in these periods. From the averages, it appears that about twice as much rain falls in each of the latter months of the table as in the month of April; and about one-third less rain falls in the first six months of the year than in the last six months, and that April is the driest month of the year.

4. Mathematical Notes. By Professor Tait.

1. On a Quaternion Integration.

A problem proposed to me lately by my friend T. Stevenson, C.E., for constructing what he calls a *Differential Mirror*, when attacked directly led to the equation

$$S\,.\,d\rho\left(\frac{\beta + \alpha V\alpha\rho}{\rho}\right)^{\frac{1}{2}}\rho = 0,$$

where α is a *unit*-vector, perpendicular to β.

By another mode of solution it was easy to see that the integral must be of the form

$$T\rho - T(\beta + \alpha V\alpha\rho) = \text{constant}.$$

It may be instructive to consider this question somewhat closely, as the form of the unintegrated expression is certainly (to say the least) at first sight unpromising.

The problem was: to construct a reflecting surface from which rays, emitted from a point, shall after reflection diverge uniformly, but *horizontally*. Using the ordinary property of a reflecting surface, we easily obtain the first written equation. By Hamilton's grand "Theory of Systems of Rays," we at once write down the second.

The connection between them is easily shown thus. Let ϖ and τ be any two vectors whose tensors are equal, then

$$\left(\frac{\tau+\varpi}{\tau}\right)^2 = 1 + 2\varpi\tau^{-1} + (\varpi\tau^{-1})^2$$
$$= 2\varpi\tau^{-1}(1 + \mathrm{S}\varpi\tau^{-1}),$$

whence, to a scalar factor *près*, we have

$$\left(\frac{\varpi}{\tau}\right)^{\frac{1}{2}} = \frac{\tau+\varpi}{\tau}.$$

Hence, putting $\varpi = \mathrm{U}(\beta + a\mathrm{V}a\rho)$ and $\tau = \mathrm{U}\rho$, we have from the first equation above

$$\mathrm{S}\,.\,d\rho\,[\mathrm{U}\rho + \mathrm{U}(\beta + a\mathrm{V}a\rho)] = 0\,.$$

But

$$d(\beta + a\mathrm{V}a\rho) = a\mathrm{V}ad\rho = -d\rho - a\mathrm{S}ad\rho\,,$$

and

$$\mathrm{S}\,.\,a(\beta + a\mathrm{V}a\rho) = 0\,,$$

so that we have finally

$$\mathrm{S}\,.\,d\rho\mathrm{U}\rho - \mathrm{S}\,.\,d(\beta + a\mathrm{V}a\rho)\mathrm{U}(\beta + a\mathrm{V}a\rho) = 0\,,$$

which is the differential of the second equation above. A curious particular case is a parabolic cylinder, as may be easily seen geometrically. The general surface has a parabolic section in the plane of a, β; and a hyperbolic section in the plane of β, $a\beta$.

It is easy to see that this is but a single case of a large class of integrable scalar functions, whose general type is

$$\mathrm{S}\,.\,d\rho\left(\frac{\sigma-\rho}{\rho}\right)^{\frac{1}{2}}\rho = 0\,,$$

the equation of the reflecting surface; while

$$\mathrm{S}(\sigma - \rho)d\sigma = 0$$

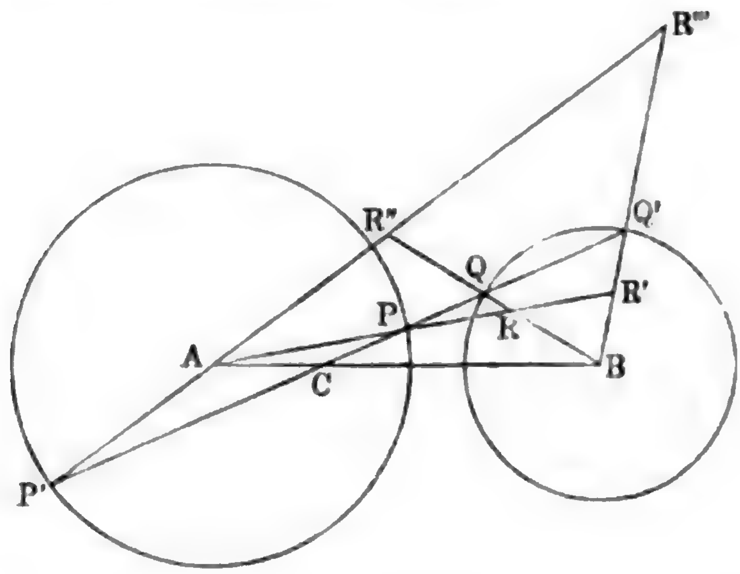
is the equation of the surface of the reflected wave: the integral

of the former equation being, by the help of the latter, at once obtained in the form

$$T\rho + T(\sigma - \rho) = \text{constant}.$$

2. On the Ovals of Descartes.

The following results were obtained lately while I was considering how most simply to describe by working sections surfaces analogous to that treated in the preceding note. They are so elementary that it is not likely that they can be new, but as they are novel to myself, and to several mathematicians whom I have consulted, I bring them before the Society:—



Let two coplanar circles be described, with centres A and B. Take any point, C, in the line of centres, and draw a line CPQ, cutting the circles in P and Q. Find the locus of R, the intersection of AP and BQ.

Expressing that CPQ is a straight line, we have, if θ and ϕ be the angles at A and B respectively,

$$\frac{\overline{AP}\sin\theta}{\overline{AP}\cos\theta \pm \overline{AC}} = \pm\frac{\overline{BQ}\sin\phi}{\overline{BC} \pm \overline{BQ}\cos\phi},$$

or

$$\overline{AP}\cdot\overline{BC}\sin\theta \pm \overline{AC}\cdot\overline{BQ}\sin\phi = \pm\overline{AP}\cdot\overline{BQ}\sin(\theta+\phi),$$

which, by substituting the sides of ARB for the sines of the angles opposite them, becomes

$$\overline{AP}\cdot\overline{BC}\cdot\overline{BR} \pm \overline{AC}\cdot\overline{BQ}\cdot\overline{AR} = \pm\overline{AP}\cdot\overline{BQ}\cdot\overline{AB} \quad \ldots\ldots \quad (1)$$

which is the general equation of Cartesian Ovals.

When $\overline{AP} \cdot \overline{BC} = \overline{AC} \cdot \overline{BQ}$ the curve becomes an ellipse or hyperbola. Of this the simplest case is

$$\overline{AP} = \overline{BQ}\,,\ \overline{BC} = \overline{CA}\,.$$

The normal at R is in all cases parallel to

$$\overline{AP}\,.\,\overline{BC}\cdot U(BR) \pm \overline{AC}\cdot\overline{BQ}\cdot U(AR)\,,$$

because we have

$$d\,.\,AR = d\,.\,BR\,.$$

But the general equation (1), on account of the identity

$$\overline{AP}\cdot\overline{BC}\,.\,\overline{BQ} \pm \overline{AC}\,.\,\overline{BQ}\,.\,\overline{AP} = \pm\,\overline{AP}\cdot\overline{BQ}\,.\,\overline{AB}\,,$$

may be written more simply, as

$$\overline{AP}\cdot\overline{BC}\,.\,\overline{RQ} - \overline{AC}\,.\,\overline{BQ}\cdot\overline{PR} = 0\,, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

a very singular and suggestive form; holding true, as it does, for all four points, R, R′, R″, R‴, in the figure.

Hence the normal is

$$\frac{U(BR)}{\overline{RQ}} \pm \frac{U(AR)}{\overline{PR}}\,,$$

which may be constructed by drawing at R a tangent to the circle circumscribing the triangle PQR. When the curve is a conic this line is parallel to CPQ, because by the condition above we have in this case

$$\overline{RQ} = \overline{PR}\,.$$

Of course the mode of tracing here adopted is at once capable of being effected mechanically.

The results above are easily derived from the general equation of Cartesian Ovals

$$er \pm e'r' = a\,,$$

by writing it in the form

$$e(r_0 + e'x) \pm e'(r_0' \mp ex) = a\,,$$

and showing from this that QP cuts AB in a fixed point.

But by a purely quaternion process it is easy to give in a very simple form the equation of the locus of R *when* C *is not in the line* AB. Let CA, CB, CR be denoted by α, β, ρ respectively, and let

$\overline{\mathrm{AP}} = a$, BQ $= b$. Then, by expressing that CP and CQ coincide in direction, we have at once the equation

$$\mathrm{V}\,.\,[\alpha + a\mathrm{U}(\rho - \alpha)]\,[\beta + b\mathrm{U}(\rho - \beta)] = 0\,,$$

in which the above results are included as a very particular case, and whose geometrical interpretation is elegant. It is a mere Scalar equation, since $\mathrm{V}\alpha\beta$ is a factor of the left side, and may be omitted.

Added, May 4th, 1871.—I have just been informed by Professor Cayley that the above results, so far as they concern the Cartesian Ovals, are to be found (some actually, some virtually) in Chasles' *Aperçu Historique*, a work of which, to my great regret, I have never been able even to see a copy.

The following Gentleman was elected a Fellow of the Society:—

JOHN SMITH, M.D., F.R.C.S.E.

Monday, 1st May 1871.

DR CHRISTISON, President, in the Chair.

The following Communications were read:—

1. On the remarkable Annelida of the Channel Islands, &c. By W. C. M'Intosh, M.D.

The extraordinary richness of the littoral region and the deeper water surrounding Guernsey and Herm, as well as the marked southern character of many of the Annelidan types, formed, for instance, an excellent comparison with the ample series of specimens which the dredgings of Mr Jeffreys in the Shetland seas had lately brought before us; or, again, with the valuable collections procured during the expeditions of the "Porcupine," in 1869 and 1870, the former chiefly from the Atlantic, the latter from the same region and the Mediterranean.

The object of the present paper is to give a short notice, chiefly

of the structural, or other, peculiarities, of the remarkable Nemerteans and Annelids found in this expedition, and of certain interesting questions in zoology connected therewith.

Amongst the Nemerteans is the curious *Ommatoplea spectabilis* of De Quatrefages, a species of much interest, in so far as its discoverer stated that it was furnished with a peculiar horny pectinated structure in its proboscis. Careful examination showed that the latter has a strictly Ommatoplean anatomy, the longitudinal bands of the reticulated layer of the pinkish organ being very apparent. In *Prosorhochmus claparedii,* Keferstein, the granules of the external circlet of glands round the stylet-region of the proboscis are unusually large and distinct. The granular basal sac of the central stylet is of a peculiar shape, having a straight border and sharp angles posteriorly, and obtuse angles at the sides anteriorly. The pale setting of this apparatus is comparatively limited in bulk; and the curved fibres of the region behind the latter pass outwards and forwards in a very distinct manner. The development of the ova in the bodies of the females of this viviparous species is very similar to that of the free ova and their products in other Ommatopleans, space being formed for the growing embryos by the enormous dilatation of the ovisacs. Indeed, the larger young specimens, which are often doubled within the body of the parent, appear to be in cavities produced by the coalescing of many ovisacs; at any rate, it is clear that to describe them, as former authors have done, as simply within the body-cavity of the worm, is wanting in structural accuracy. It seems to be a further stage of the type of development observed in *Nemertes carcinophilus,* Kolliker (*Polia involuta,* Van Beneden), in which, after the deposition of the majority, a few are left in the body of the parent for subsequent evolution. A still more remarkable Nemertean is the *Borlasia elisabethæ,* M'I., from Herm, a large species with a pointed, eyeless snout. In this form the powerful muscular layers of the body-wall are tinted of a fine reddish hue, so that the resemblance in this respect to the muscles of the higher animals is striking. The proboscis is extremely slender in proportion to the bulk of the animal, and its muscular walls are comparatively thin. A reddish coloration was frequently observed in the living animal at the white belts, showing that some contained fluid tinted the

cutaneous tissues during its passage. On puncturing the swollen anterior end, a copious exudation of a reddish-brown fluid occurred. This presented many fusiform and clavate corpuscles, probably from the proboscidian fluid; but there were also a vast number of minute granules, of a yellowish colour by transmitted light, though reddish in mass, which doubtless belonged to the blood-proper. Many of the latter bodies showed a contraction in the middle, so as to resemble the outline of a figure of eight.

In regard to the Annelids Proper, it is found that the northern *Aphrodita aculeata* and *Lœtmonice filicornis*, Kbg., are replaced by the southern *Hermione hystrix*, which occurs in great abundance in water from 10 to 20 fathoms in depth. Amongst the *Polynoidæ*, *P. areolata*, Grube, is remarkable in having greatly swollen cirri. The dorsal bristles are not very robust, while the ventral are in two sets, if the ends alone are viewed, but form a regularly diminishing series from the dorsal to the ventral surface as regards length of tip. The scales are boldly areolated. In this species there is a series of well-marked circular muscular fibres towards the outer half of the vertical coat of the proboscis. The new *Harmothoë marphysæ* accompanies *Marphysa sanguinea* in its tube.

The remarkable forms of the *Phyllodocidæ* and *Hesionidæ*; the great abundance of the *Nereidæ*, and the uses of the latter as bait, were next detailed.

The representatives of the Eunicidæ are very plentiful. Besides the gigantic *Marphysa sanguinea*, there occur *Marphysa belli*, *Eunice harassii* or *norvegica*, and *Eunice gallica*. The allied forms *Lysidice ninetta* and *Blainvillea filum* are also abundant, and impart a character to the fauna of the region. The same may be said of *Prionognathus Kefersteini* and *Staurocephalus rubrovittatus*.

Chætopterus norvegicus and other phosphorescent Annelida were then examined, and the facts observed in these, as well as in other luminous invertebrates were shown to give no support to the Abyssal Theory of Light as expounded in the "Report (1869) of H. M. ship 'Porcupine.'"

The structure and habits of the Annelida frequenting muddy ground in the Channel Islands, and the examination of those and other marine invertebrates elsewhere, exhibited grave objections to another theory, lately brought forward by Dr Carpenter ("Porcu-

pine" Report for 1870), viz., that the barrenness of the deeper parts of the Mediterranean is due to the turbidity (from mud) of the bottom-water.

2. Note. On the Use of the Scholastic Terms *Vetus Logica* and *Nova Logica*, with a Remark upon the corresponding Terms *Antiqui* and *Moderni*. By Thomas M. Lindsay, M.A., Examiner in Philosophy to the University of Edinburgh.

During the earlier part of the middle ages, or until the middle of the eleventh century, students of logic had a very incomplete knowledge of the logical works of Aristotle. They knew the translations which Boethius had made of Porphyry's Εἰσαγωγὴ, of Aristotle's περὶ κατεγορίων, and of his περὶ ἑρμηνείας, and they knew little else. Their labours did not go beyond the reproduction of, and commenting on, these old Greek writings.

Towards the beginning of the twelfth century, however, the gradual diffusion of knowledge had brought with it acquaintance with the remaining treatises of Aristotle's Organon. The old translations of Boethius were recovered, and new translations were made. We are told that "Jacobus Clericus of Venetia translated from Greek into Latin certain books of Aristotle, and commented on them, namely, the Topica, the Analytics Prior and Posterior, and the Elenchi, although," adds the chronicler, "an earlier translation of these same books may be had."* This was in 1128 A.D. It is more than probable that Roscellinus, who flourished 1080–1100, knew more of Aristotle's writings than the treatises on the Categories and on Interpretation. Abelard (b. 1079—d. 1142) must have known the greater part of Aristotle's Organon, and John of Salisbury (who died 1180), we know, knew the whole of it.

Hence, whereas at the middle of the eleventh century the knowledge of Aristotle was confined to acquaintance with the two first

* "Jacobus Clericus de Venetia transtulit de græco in latinum quosdam libros Aristotelis et commentatus est, scilicet Topica, Anal. priores et posteriores et Elenchos, quamvis antiquior translatis super eosdem libros haberetur." Robert de Monte Chronica ad Ann. 1128, in Pertz, Monument. viii. 489. Quoted from Prantl, Geschichte der Logik ii. p. 99.

books of the Organon, along with the Introduction of Porphyry, at the middle of the twelfth century there were two distinct sources of knowledge of Aristotle's opinions on Logic—that derived from the "old" tradition from the books on the Categories, and on Interpretation, and from the Introduction of Porphyry, and that derived from a "new" tradition from recovered translations made by Boethius of the Prior and Posterior Analytics, of the Topics and of the book on Fallacies, and from new translations.

This new tradition was looked upon with considerable mistrust by several of the steady going old schoolmen. It disturbed their view of logic. They had constructed a very fair well-rounded system from the material supplied by the old tradition. It had been sufficient for them then, and they wanted nothing new now. Even supposing that these new treatises were Aristotle's, they would not admit them to be logical, or, if they went so far, they would not allow them to have any real importance. The old doctrine had done very well for them and their fathers before them, and it might serve every one else. They saw no need for any change. On the other hand, more enterprising students were vastly taken with these new treatises, and found that they contained Aristotle's real logic. They revealed to them the doctrine of the syllogism, and its application in demonstrative, probable, and fallacious material of knowledge. The new tradition was Logic, the old not more than an introduction, even if worthy of that place.

When we consider that logic, with all its verbal niceties, was more studied than anything else in these days, we find in the very fact of these two different traditions, and the two ways of accepting them, all the elements for a severe and widely extended quarrel: and the quarrel soon arose. On the one side, the zeal shown in studying and commenting upon these new treatises was wholly attributed to the love of novelty, and the new opinions concerning logic and its sphere, which were coming into fashion, were set down as due to a restless, shallow, modern spirit. The logic of the new tradition was called the "*Nova Logica*," and those who advocated it, "*Moderni*." On the other hand, the Moderni thought that their opponents were prejudiced against their opinions, simply because they were not the old ones, and they despised them as old world thinkers, who had not the breadth of view required to accept

anything, however good in itself, which differed from their old theories. They called the logic of the old tradition the "*Vetus Logica*," and its upholders "*Antiqui*."

Now, curiously enough these terms had been applied half a century before, and in a very different manner. When Roscellinus had startled the orthodox world by saying that *universals* were only "*flatus vocis*," and had drawn many heretical conclusions in logic and in theology, from this doctrine, his opponents said that he was the author of a "new" kind of logic, and called his followers "moderni." The "old" logic, of the days of Roscellinus, treated logic from a *realist* point of view, the "new" logic treated logic from a nominalist point of view (so far as the words "realist" and "nominalist" can be used with accuracy of any doctrine at this early period of scholasticism). The Antiqui of the time of Roscellinus became realists in the time of Thomas of Aquino, and the "moderni" were the nominalists of later days.

Here then we have a confusion in the terminology, on the one hand Vetus Logica meant the introduction of Porphyry, the treatises on the Categories, and on Interpretation; Nova Logica, the Prior and Posterior Analytics, the Topics and the book on Fallacies; Antiqui, those who thought that Logic Proper was contained in this Vetus Logica; Moderni, those who thought that this Nova Logica was the true Logic. On the other hand, Vetus Logica meant logic treated from a realist point of view; Nova Logica, logic treated from a nominalist point of view; while Antiqui and Moderni corresponded very much to the latter terms of Realist and Nominalist.

This confusion does not really last throughout the period of Scholasticism. The meaning of the terms did fluctuate somewhat, as all terms do, but upon the whole they preserved a great uniformity of meaning. "Vetus" and "Nova Logica," became dissociated from "Antiqui" and "Moderni," with which they were at first so closely united, and, curiously enough, while the one set of terms kept to one of their primitive meanings, the other set kept to the opposite meaning. "Vetus" and "Nova Logica" were used of divisions of Aristotle's Organon; while Antiqui and Moderni became more or less, though never quite, equivalent to Realist and Nominalist.

"Vetus Logica," from the middle of the twelfth down to the beginning of the sixteenth century, meant the logic taught in the *εἰσαγωγὴ* of Porphyry, and in the *περὶ κατηγοριῶν* and the *περὶ ἑρμηνείας* of Aristotle.

"Nova Logica," during the same period, meant the logic of Aristotle's *ἀναλυτικὰ πρότερα, ἀναλυτικὰ ὕστερα, τοπικά* and *περὶ σοφιστικῶν ἐλέγχων*. This is the almost invariable scholastic use of the terms. Any other is accidental and variable.

Now, this assertion is made against the greatest authority in the history of scholastic Logic, Professor Prantl of Munich, whose "Geschichte der Logik im Abendlande," is one of the most trustworthy and laborious efforts in historical research. Dr Prantl recognises, as every one must do, that the meaning given here to "vetus" and "nova logica" was one of the principal scholastic uses of the terms, and every quotation to be made from logical treatises in support of our view of the question appears in his notes, but he seems to think that the expressions retained their relation to the names "Antiqui" and "Moderni," and that any signification which belongs to them apart from these names is entirely subordinate. He connects the term "Nova Logica" with the partly grammatical, partly logical additions to the doctrine which first became popular through the Summulæ Logicales of Petrus Hispanus; * he makes it occupy the middle place between the "old" logic and the "Ars Magna" of Raymond Sully; and he has proved by a quotation from a dialogue in that curious and amusing Manuale Scholarium or Mediæval Students' Guide-book, given in Zarnacke's Deutschen Universitäten im Mittelälter, that when the Antiqui were hard pressed by the Moderni, they always retired on the "Vetus Logica" as their stronghold. †

* *Prantl* believes that this addition to logic is due to a Byzantian influence, and therefore believes that the Summulae of Petrus Hispanus is almost a Latin translation from the Greek of Psellus. Sir W. Hamilton and many other authorities refuse to admit this Byzantian influence, and hold that the Greek work of Psellus is a copy or translation from the Latin of Petrus Hispanus. *Prantl, Gesch. der Logibr.*, ii. p. 264. *Hamilton Discus.* 2nd ed., p. 275.

† C. iv. De altricatione viarum et disciplinarum.

Camillus. Hunc magistrum tu quasi ad cœlum attuliste tamen modernus est.

Bartoldus. Quid tum?

It is not to be supposed that two names, especially when embodied in such vague words as "old" and "new" should have preserved the same invariable meanings in every writer during a period of three centuries. We may, therefore, admit, without prejudice to our statement, that the terms "Vetus" and "Nova Logica" did bear those significations which Prantl gives to them, and did preserve a more or less continuous connection with the terms "Antiqui" and "Moderni." But it may be proved that, from about the middle of the twelfth century down to the middle of the fifteenth at least, the first meaning which the term *Vetus Logica* would suggest to a mediæval student was "the logic treated in the Predicables of Porphyry, and in the Categories and De Interpretatione of Aristotle;" while the first meaning suggested by the term *Nova Logica*, was "the logic treated in Aristotle's Prior and Posterior Analytics, his Topics, and his book on Fallacies."

This may be directly proved from the quotations which Prantl himself gives.

Lambert of Auxerre, who lived in the middle of the 13th century, says, "Logica traditur in omnibus libris logicæ, qui sunt sex. sc. liber prædicamentorum, liber Peryermenias, qui nunc dicuntur *vetus logica*, liber Priorum, Posteriorum, Thopicorum et Elenchorum, *qui quatuor dicuntur nova logica*."—Cf. Prantl, iii. p. 26.

Cam. Nihil ab eo deinceps audiam.

Bart. Eo stultior es, si doctrinam despicis. Nam non solum realistæ verum etiam moderni magnam partem philosophiæ consecuti sunt.

Cam. Sed versantur in sophismatibus tantum, veram doctrinam aspernantur.

Bart. Offendis veritatem, nam erudissimi viri reperiuntur inter modernos. Nonne audisti, in quibusdam terris eos possidere integras universitates? ut Viennæ Erfordiæ, utque quondam hic erat. Nonne arbitraris, doctos hic bonosque fuisse? Et nostro aevo adhuc reperiuntur?

Cam. Scio quidem et intelligo, sed fama eorum parva est. Elaborant solum *in parvis logicalibus* et sophismaticis opinionibus.

Bart. Non recte intelligis, nam clari sunt in enunciationibus et syllogismis. Non reperies artium studiosos, qui syllogismos ceterasque species argumentationis facilius noscant quam moderni.

Cam. Et in vera scientia nihil sciunt.

Bart. Quam mihi facis veram scienciam?

Cam. *Predicabilia Porphyrii, cathegorias Aristotelis*, in quibus aut parum noveant aut nihil.—p. 11, 12.

Duns Scotus, who died in 1308, calls Syllogistic, *i.e.*, the Prior and Posterior Analytics and the Topics, the "Nova Logica," and the Categories, with the De Interpretatione, the "Vetus Logica."

In the 14th century we have commentaries Super Veterem Artem, *e.g.*, by Antonius Andreas, by Walter Burleigh, and by Gratiadei of Ascoli (Esculanus, as he is commonly called), and these are invariably expositions of the Predicables of Porphyry, the Categories, and the De Interpretatione of Aristotle.

Esculanus (d. 1341) says plainly, "Ars autem nova, quæ tota versatur circa ratiocinationem, oportet quod distinguatur secundum diversam considerationem eius; potest autem ratiocinatio dupliciter considerari, uno quidem modo simpliciter sine applicatione ad materiam aliquam, et alio modo considerari potest cum applicatione ad materiam specialem. De ratiocinatio quidem sumpta in sua comitate, agitur in libro priorum, sed ratiocinatio sumpta cum applicatione ad materiam specialem distinguitur; quia aut applicatur ad materiam demonstrativam; ac sic agitur de ipsa, in libro posteriorum; aut etiam applicatur ad materiam dialecticam. In materia autem dialecticam potest fieri ratiocinatio recta et ratiocinatio sophistica. De ratiocinatione recta agitur in libro topicorum; et de ratiocinatione sophistica in libro elenchorum." *

There is, however, another source of evidence which Prautl has not in this reference carefully investigated—the regulations and decrees of the universities. When any term whatever is found in a university-decree, we may take it for granted that its signification there was the standard one for the time being, and when we find the same terms occurring in the regulations of almost all the principal universities with the same meaning, we are warranted in adopting that meaning as the real signification of the term.

These terms, "Vetus" and "Nova Logica," are frequently found in the regulations of the mediæval universities, and they invariably mean the logic taught in the first two, and the logic taught in the last four, of the treatises of the Organon.

* Commentaria Graciadei Esculani ordinis predicatorum. In totam Artem veterem Aristotelis, f. 1.

Thus as early as 1215* the students of Paris University are commanded to read the *books of Aristotle* on Logic,—both the "Vetus" and the "Nova Logica."

In 1309 we find, among the Statuta Collegii Cluniacensis, a statute concerning scholars studying philosophy, in which students are told to work at—first the Summulæ in the college; then the Vetus Logica; and lastly the Nova Logica, either in the college or outside.† This passage is important, because it shows that the Summulæ are not part of the Nova Logica; elsewhere Summulists are distinguished from Logicos.

In 1366, at the reformation of the Faculty of Arts, it is ordained that students attending lectures in this faculty read the whole of the *vetus ars*, four books of the Topics and the books of the Elenchi, the Prior or the Posterior Analytics completely, and the books De Anima in whole or in part.‡

In the munimenta of the University of Oxford, published by the Master of the Rolls, we have many references to the vetus and nova logica; and in all cases the reference is evidently to books of Aristotle's Organon.§

Thus Artistæ are told, in 1340, that, before they can "incept" in arts, they must first have sworn that they have read two logical books at least, one of the vetus logica, and the other of the nova.‖

In the munimenta of the University of Glasgow, of the date 1460, or thereabout, we find it enacted in the regulations about reading in logic—"Ordinaria vero audienda sunt hæc; primus sc. in Veteri Arte liber universalium Porphyrii, liber Predicamentorum Aristotelis, duo libri Peri Hermeneias ejusdem. In Nova Logica duo libri priorum, duo posteriorum, quatuor ad minus Topicorum, sc. primus, secundus, sextus, et octavus, et duo elenchorum. . . . Item audiantur libri extraordinarii . . . in logica textus Petrus

* Bulæus. Hist. Univ. Paris, iii. p. 82.

† Ibid., iv. p. 122.

‡ Item quod audierunt veterem Artem totam, librum Topicorum, quoad 4 libros, et libros Elenchorum, Priorum aut Posteriorum complete; etiam librum de Anima in tota vel in parte.—*Bul. Hist. Univ. Paris*, iv. 390.

§ Munimenta Acad. Oxon. 128, 417, 422. Edited by Anstey.

‖ Ibid., 142, cf. 242, 286.

Hispanus cunc syncathegorematibus, tractatus de distributionibus liber sex principiorum." *

This reference is important, because it places those grammatico-logical treatises, which gave a distinctive character to the logic of the moderni, outside of the "nova logica."

In the Liber Decanorum of the University of Prague, the *Vetus ars Aristotelis* is always kept separate from the books of the Prior and Posterior Analytics, the Topics, and the book on Fallacies; † and this division is elsewhere referred to as that of "Vetus" and "Nova Logica." ‡

Aschbach, in his history of the University of Vienna, says that the *Ars Vetus* treated of the Predicables of Porphyry, and of the Categories or Predicaments, and of the de Interpretatione of Aristotle. The *Logica Nova* looked at argumentation as a whole, and considered—(1.) The Resolution or analyses of syllogisms given in the Prior and Posterior Analytics; (2.) Inventive, or ways of discovering true middle terms, given in the Topics; and (3.) Fallacies, given in the libri Elenchorum. Prof. Aschbach shows that Logic, as taught in Vienna, consisted of three parts—the Vetus Logica, which was studied as an introduction; the Parva Logicalia, for the Vienna Students were Moderni; and the Nova Logica.§ The lists which he quotes bears out his statement, with this exception, that after some time the Parva Logicalia, not the "Ars Vetus," came to be looked on as the introduction to Logic.‖

These quotations may, perhaps, serve to prove our assertion, that the scholastic use of the terms "vetus" and "nova logica" is almost exclusively confined to the designation of parts of the

* Munimenta Univ. Glasg., ii. 25, 26. This reference I owe to Professor Veitch of Glasgow.

† Liber Decanorum Fac. Phil. Univ. Prag. Pars. i. pp. 83, 126.

‡ Ibid., p. 127.

§ Ibid., p. 89, 90.

‖ Ibid., pp. 95, 135, 139, 142, 144, 147, 151, 154, 161. According to these lists a course of lectures on the *Ars Vetus* cost 5 groschen, but, if taken with exercises and colloquia, or quæstiones, it cost 18 groschen. A course on the Parva Logicalia cost 10 groschen, including quæstiones. While a course on the Nova Logica cost 12 groschen, and 36 including quæstiones (p. 95). In the last decade of the 14th century, the course on the Parva Logicalia consisted of 104 lectures, and cost a gulden; the length of the course on the Vetus Logica was the same, and the fee the same; while the courses on the Nova Logica consisted of 132 lectures, and the fee was 35 groschen (p. 352).

Organon of Aristotle—the part earlier and the part later known; and that the meaning of the terms did not vary with the significations of Antiqui and Moderni.

The point discussed in this note is of small importance on its own account, but it is one step, and a rather significant one, in the argument which tends to show that the new life in scholasticism which expressed itself most fully in the 14th century in William of Occam, and which afterwards developed, through the early natural philosophers of Italy, into those scientific methods which have rendered modern science possible, was due to the inborn genius of western Europe, and was not a foreign growth cut from the Greek stock and engrafted on the Latin.

3. On some Abnormal Cones of *Pinus Pinaster*. By Professor Alexander Dickson.

In their celebrated essay, "*Sur la disposition des feuilles curviseriées*,"* the brothers Bravais describe a cone of *Pinus Pinaster* (*Pin maritime*), where the lower part of the cone exhibited secondary spirals 7 S, 12 D (series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}, \frac{8}{19}$, &c.), while towards the apex the arrangement, in consequence of the disappearance of one of the spirals by 12, changed to 7 S, 11 D (series $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}$, &c.).† They describe another cone of the same species, in which the lower four-fifths exhibited secondary spirals 9 S, 13 D (series $\frac{1}{4}, \frac{2}{9}, \frac{3}{13}, \frac{5}{22}$, &c.), changing at the upper fifth to 8 S, 13 D (ordinary series $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}$, &c.) by suppression of one of the spirals by 9.‡ Such cases, along with some others chiefly in the capitula of *Dipsacus sylvestris*, lead these authors into a discussion of the general question of the possible transition from one arrangement to another by change in the number of secondary spirals. As regards their "curviserial" forms, however, they are disposed only to admit the occurrence of such transitions by way of

* Ann. des Sc. Nat. 2d ser. t. vii. † *L. c.* p. 93.
‡ *L. c.* p. 103.

convergence of secondary spirals, *i.e.*, by abortion of one, or possibly coalescence of two, resulting in diminution of number. For example, after referring to the possible derivation of an arrangement with 5 and 7 secondary spirals (series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c.), from an ordinary one with 5 and 8, by abortion of one of the spirals by 8, they add that "the series 1, 4, 5, 9 . . . does not admit of explanation by the way of abortion, and that one can deduce it from the ordinary series only by supposing a *superfœtation* or addition of a new spiral among the secondary spirals by 8." "This hypothesis," they continue, "appears to us altogether improbable, since in the face of an immense number of instances where two spirals *converge* into one, we cannot on the other hand cite one (apart from rectiserial stems) where one spiral *diverges* into two similar and parallel ones."*

The two cones of *Pinus Pinaster* which form the immediate subject of Dr Dickson's paper, and for which he is indebted to the kindness of R. Smyth, Esq., Emyvale, Co. Monaghan, Ireland, are interesting cases of *convergence* of spirals. These, together with a few other cases already noted by Dr Dickson, seem to throw some additional light upon this question of the origin of variations in the spiral arrangements in a given plant, where not unfrequently spirals belonging to several distinct systems occur.

In the first of the cones received from Mr Smyth, there is at the base a right-handed $\frac{8}{37}$ spiral (series $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}, \frac{8}{37}$, &c.) with the secondary spirals 9 S, 14 D, 23 S. A little above the base, however, two of the 9 spirals to the left run into one, leaving, from that point up to about the middle of the cone, an arrangement of secondary spirals 8 S, 14 D, 22 S = a left-handed bijugate of the series $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}$, &c., with divergence $\frac{5}{18 \times 2}$. About the middle of the cone two of the 14 spirals to the right run into one, leaving, from thence to the top of the cone, an arrangement of secondary spirals 8 S, 13 D, 21 S = a left-handed $\frac{13}{34}$ spiral of the ordinary series $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}$, &c.

* *L. c.* pp. 104, 105.

The second of Mr Smyth's cones exhibits from the base to near the top a right-handed $\frac{5}{18}$ spiral (series $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}$, &c.) with secondary spirals 7 S, 11 D. Near the top of the cone, however, two adjacent scales of two of the 7 spirals to the left have partially coalesced, and beyond that point the two spirals run into one, leaving an arrangement of secondary spirals 6 S, 10 D = a left-handed bijugate of the ordinary series $\frac{1}{2}, \frac{1}{3}, \frac{2}{5}, \frac{3}{8}$, &c., with divergence $\frac{3}{8 = 2}$.

In the cone of *Pinus Lambertiana*, recently exhibited to the Society, it will be recollected that at the bottom and top of the cone there was a left-handed $\frac{5}{23}$ spiral (series $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}$, &c.); while in the middle was a right-handed bijugate of the series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c., where the divergence in each of the two generating spirals = $\frac{5}{12 \times 2}$. In this cone the steepest secondary spirals at the bottom and top were 9 D, 14 S; while those in the middle were 10 D, 14 S.

In connection with the above, Dr Dickson recalled attention to the flower-spikes of *Banksia occidentalis* recently exhibited to the Society, where there were four different arrangements,—viz., one with secondary spirals 7 and 7 = alternate whorls of 7 (or, if preferred, a 7-jugate of the ordinary series with divergence $\frac{1}{2 \times 7}$), giving 14 vertical rows; one with secondary spirals 7 and 6 = a $\frac{2}{13}$ spiral (series $\frac{1}{6}, \frac{1}{7}, \frac{2}{13}$, &c), giving 13 vertical rows; one with secondary spirals 7 and 5 = a $\frac{5}{12}$ spiral (series $\frac{1}{2}, \frac{2}{5}, \frac{3}{7}, \frac{5}{12}$, &c.), giving 12 vertical rows; and one with secondary spirals 8 and 5 = a $\frac{5}{13}$ spiral (ordinary series) giving 13 vertical rows.

It will be noted that, contrary to the opinion of MM. Bravais, one arrangement does not necessarily or only originate from another by suppression of parts. To prove this, we have only to

look at the above-mentioned cone of *Pinus Lambertiana*, where the arrangement in the middle region results from an *augmentation* of parts as compared with the base of the cone; while the spiral at the top, which is the same as that at the base, is, of course, the result of a *diminution* as compared with the middle. It has been already observed by authors, moreover, that in such plants as Cacti and succulent Euphorbias* one vertical row may be split into two, or, conversely, two run into one, thus changing the spiral. Now, as vertical rows are, in one sense, only to be regarded as the steepest secondary spirals (a slight torsion readily converting them into actual spirals), such cases are in all essentials comparable to the above-described cones.

The arrangements above indicated will be rendered very readily intelligible by the accompanying tabular views.†

TABLE A.—*Cone of* Pinus Pinaster (*Mr Smyth—No.* 1).

	S	D	S	D	S	D	S	V	
Top,	1	2	3	5	8	13	21	34 =	$\frac{13}{34}$
Middle.	—	—	2	6	8	14	22	36 =	$\frac{5}{18 \times 2}$
Bottom,	—	1	4	5	9	14	23	37 =	$\frac{8}{37}$

TABLE B.—*Cone of* P. Pinaster (*Mr Smyth—No.* 2).

	D	S	D	S	D	V	
Top,	—	2	4	6	10	16 =	$\frac{3}{8 \times 2}$
Bottom,	1	3	4	7	11	18 =	$\frac{5}{18}$

* The greater number of these plants would be reckoned as truly rectiserial by MM. Bravais. Dr Dickson has no hesitation in referring to such cases in this argument, as he is strongly disposed to doubt as to there being any fundamental distinction between the "rectiserial" and the so-called "curviserial" spirals of these authors.

† In these tables, under S, are indicated the numbers of spirals, generating as well as secondary, running to the *left;* under D, the numbers of those running to the *right;* while under V are indicated the numbers of vertical rows.

TABLE C.—*Cone of* Pinus Lambertiana, *in Museum, Edinburgh Botanical Garden.*

	S	D	S	D	S	V	
Top,	1	4	5	9	14	23 =	$\frac{5}{23}$
Middle,	—	2	4	10	14	24 =	$\frac{5}{12 \times 2}$
Bottom,	1	4	5	9	14	23 =	$\frac{5}{23}$

Table D represents the four different arrangements in the flower-spikes of *Banksia occidentalis*, placed in series so as to show how, by slight diminution or augmentation in the number of secondary spirals, one arrangement may be conceived to originate from another. The directions of the spirals to right or left are stated arbitrarily, to suit the purpose of the diagram.

TABLE D.

	D	S	D	S	D	V	
No. 1,	—	—	—	7	7	14 =	$\frac{1}{2 \times 7}$
No. 2,	—	—	1	6	7	13 =	$\frac{2}{13}$
No. 3,	—	1	2	5	7	12 =	$\frac{5}{12}$
No. 4,	1	2	3	5	8	13 =	$\frac{5}{13}$

It is impossible to reflect on such cases as have been adduced and not be impressed forcibly with the idea that, as regards their production or origination, *diverse spiral arrangements are to be regarded as allied much more according to the numerical correspondence of their secondary spirals and verticals than in proportion to the correspondence of their angular divergences.* Such cases, moreover, show clearly how a generating spiral may change its direction on one and the same axis.

It is perhaps rash to speculate as to how the different systems of spirals in Fir cones originate. On the whole, Dr Dickson is inclined to assume the bijugate of the ordinary system as the fundamental arrangement. He is to some extent confirmed in this view by a remarkable abnormality in a cone of *P. Pinaster*, gathered by him at Muirhouse, near Edinburgh. This cone exhibits a left-

handed $\frac{8}{21}$ spiral. At the base of the cone, however, a number of rudimentary scales of small size and somewhat peculiar shape are intercalated with considerable regularity among the others, so as to appear as projections placed at the intersections of the lines formed by the margins of the larger scales. Now, if these small scales had been disposed with perfect regularity, and had been of equal size with the others, there would have been a left-handed bijugate arrangement, with divergence $\frac{8}{21 \times 2}$. Such a cone, in fact, suggests the possibility of single spirals of the ordinary series being derived from bijugates of the same series by suppression of one half of the scales.

Again, the ordinary trijugates are easily derivable from bijugates, as indicated in Table E.

Table E.—*Showing the possible derivation of ordinary Trijugate from the Bijugate Arrangement.*

D	S	D	S	V
—	3	6	9	$15 = \frac{2}{5 \times 3}$
2	4	6	10	$16 = \frac{3}{8 \times 2}$

From the ordinary trijugate, in turn, a spiral of the system, $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}$, &c., may be simply derived, as indicated in Table F.

Table F.—*Showing possible derivation of a Spiral of the System,* $\frac{1}{4}, \frac{1}{5}$, *&c., from the Ordinary Trijugate.*

D	S	D	S	D	S	V
1	4	5	9	14	23	$37 = \frac{8}{37}$
—	3	6	9	15	24	$39 = \frac{5}{13 \times 3}$

Again, it is clear that by augmentation of parts, a spiral of the system $\frac{1}{3}, \frac{1}{4}, \frac{2}{7}$, &c., may be derived from the ordinary bijugate, since the converse (by diminution) actually occurs in the second of Mr Smyth's cones indicated in Table B.

Lastly, the spiral $\frac{5}{22}$, series $\frac{1}{4}, \frac{2}{9}, \frac{3}{13}, \frac{5}{22}$, &c., which Dr Dickson formerly noted as occurring in a cone of *Pinus Pinaster*, in the Museum, Edinburgh Botanic Garden, may readily be derived, as MM. Bravais have suggested,* from a spiral of the series $\frac{1}{4}, \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}$, &c., thus,

Table G.—*Showing possible derivation of a* $\frac{5}{22}$ *Spiral from the System* $\frac{1}{4}, \frac{1}{5}$, *&c.*

D	S	D	S	D	V	
—	1	4	9	13	22	$= \frac{5}{22}$
1	4	5	9	14	23	$= \frac{5}{23}$

The following Gentleman was admitted a Fellow of the Society:—

Rev. Professor Crawford.

Monday, 15th May 1871.

Professor CHRISTISON, President, in the Chair.

At the request of the Council, Professor Tait gave an Address on Spectrum Analysis.

(*The following is a brief Abstract, consisting mainly of the Lecture Notes*) :—

I should not have thought of appearing before you to-night to lecture on so hackneyed a subject, had I not been assured by several members of the Council that such an address was really desired by many Fellows of the Society. It is a subject to which I have not paid very special attention, partly because it is in so many and such good hands, and partly because (except from the point of view of theory) it requires for its extension, especially to

* *L. c.* p. 103.

astronomy, very costly instrumental appliances and a great sacrifice of time. And the difficulty of transporting to the Society's rooms from the College the large amount of bulky and delicate apparatus required for its proper illustration, is (as I have just found) so great, that if on any future occasion the Society desire me to give such an address, I shall have to make it a condition that the meeting for that evening be held in my class-room in the University buildings.

The subject of spectrum analysis must always possess great interest for this Society, inasmuch as many of its most distinguished promoters have been, or are, among our Fellows, ordinary as well as honorary, and several of the most remarkable memoirs on various parts of the subject are to be found among our publications.

The objects of spectrum analysis may be briefly enuntiated as follows:—*To make, by optical methods, the qualitative chemical analysis of* (1) *a self-luminous body*; (2) *an absorbing medium, whether self-luminous or not.*

It is difficult now-a-days, when so many philosophers are engaged almost simultaneously at the same problem, to decide which of their successive steps in advance is that to which should really be attached the title of *discovery* (in its highest sense) as distinguished from mere *improvement* or *generalisation.* You have only to look at the recent voluminous discussions as to the discoverer of the Conservation of Energy, to see that critics may substantially agree as to facts and dates, while differing in the most extraordinary manner as to their deductions from them.* Some of these writers, no doubt, put themselves out of court at once by habitually attributing the gaseous laws of Boyle and Charles to Mariotte and Gay-Lussac. Men who persist in error on a point so absolutely clear as this, show themselves unfit to judge in any case of even a little more difficulty. Others, who strongly support the so-called claims of Mayer in the matter of Conservation of Energy, and who should (to be consistent) therefore far more strongly advocate the real claims of Talbot, Stokes, Ångström, Stewart, &c., to the discovery of spectrum analysis, are found to uphold Kirchhoff as alone en-

* Some frantic partisans of Papin, &c., deny almost all credit to Watt in the matter of the steam-engine! No farther examples need be cited.

titled to any merit in the matter. As a paper by Mr Talbot, on the early history of the subject, is to be read this evening, I shall content myself for the present with the remark, that, of the two objects of spectrum analysis above named, Talbot and Herschel were unquestionably foremost in the enuntiation of the first; Brewster, Ångström, and especially Stokes and Balfour Stewart, in that of the second. Why some of their statements were incomplete or inexact, and what was required to complete or to correct them, will be more usefully stated after I have given some preliminary explanations.

SPECTRUM.—Newton's fundamental experiment.
Reason of separation of colours.
Reason of impurity.
How to obtain a pure spectrum.
Object of trying to do so.
Effect of Additional Prisms.

Note that the source of light in all these experiments has been carbon heated to incandescence by resistance to a powerful current of voltaic electricity.

I. Incandescent solids and liquids give *generally* a continuous spectrum.

Its highest radiation, and the amount of radiation of each wave length, depend on the temperature.

Hence the necessity of using the highest temperature we can obtain.

Illustrate by different lengths of platinum wire heated by current.

II. Gaseous bodies, incandescent, give generally a (limited) number of perfectly definite wave lengths (though under certain circumstances of pressure, &c., they give a continuous spectrum). The number depends for each substance on its temperature and pressure, and their appearance is *characteristic of the substance.* For, under the same physical circumstances, we have always the same effect—as, indeed, must be assumed to be the case, if we think physics can be studied at all. This remark was virtually made by Carnot, and is all that was wanting in Talbot's earliest paper to make it the complete statement of this first part of the subject.

Illustrate by the spectra of the incandescent vapours of
Thallium,
Lithium,
Magnesium,
Sodium.

Illustrate the conductivity of the vapour of the latter by the increased breadth of the spectrum when it is present; also by its effect in improving the spectra of other substances when a weak battery is used.

Hydrogen—by induction-coil.

(Here refer again to Talbot's paper, presently to be read.)

SPECTROSCOPES.—Swan's paper, in Edinburgh Transactions—Introduction of Collimator—estimation of the excessively minute amount of sodium required to give the D line.

UNIVERSAL PREVALENCE OF SODIUM, LITHIUM, &c.

DISCOVERY OF NEW METALS.—Bunsen—Rubidium, Cæsium.
Crookes and Lamy—Thallium.
Reich and Richter—Indium.

DISCOVERIES IN ASTRONOMY AND METEOROLOGY.

Lightning.
Aurora.
Solar prominences and corona.
Nebulæ.
Comets.
Zodiacal light.
Temporary stars.

Huggins, Janssen, Lockyer, Secchi, &c.

III. Absorption by glowing gases, from otherwise continuous spectra.

Fraunhofer's lines (Wollaston).
Reversal of sodium line (exhibit).
Hence *atmospheres* of sun, stars, &c.

Brewster (in Edinburgh Transactions).

Nitric peroxide—effects of heat and pressure.
Atmospheric lines.

Foucault.—Spectrum of incandescent carbon points, seen (by reflection) through the voltaic arc (which itself gives them bright) shows the D lines reversed.

Stokes—about 1850, gave, in consequence of W. H. Miller's very accurate verification that the double bright line of sodium exactly corresponds in refrangibility with the double dark line D, the correct mechanical explanation of the phenomenon, with the mechanical illustrations still very often employed. Given, with general theory of solar and stellar chemistry, ever since (annually) by Thomson in his lectures. Give it.

Ångström—1853.—"Un gaz à l'état d'incandescence émet des rayons lumineux de la même réfrangibilité que ceux qu'il peut absorber."

B. Stewart (Edinburgh Transactions, 1858–9).

Extension of the Theory of Exchanges—The radiating power of a body is equal to its absorbing power, and that for every ray. Based on experimental facts.

Heated pottery ware, with marked pattern, looked at in the dark.

Coloured glasses lose their colour in the fire.

Kirchhoff, Oct. 1859.—Introduction of reasoning more *directly* based on the Second Law of Thermodynamics.

Proof that the absorbing flame must be colder than the source—Exception for Fluorescence.

Kirchhoff and Stewart.—Tourmaline, which polarises common light by absorbing polarised light, gives off, when hot, polarised light like that which it absorbs.

(Note that the discussion of the question of priority on this subject, in papers by Stokes, Thomson, Kirchhoff, and Stewart, in the *Phil. Mag.* 1863, is very interesting, and may still be read with profit).

Fluorescence is Degradation of Energy.

Exhibit Stokes' fundamental Experiments.

The question of priority just alluded to illustrates in a very curious way a singular and lamentable, though in one sense honourable, characteristic of many of the highest class of British scientific men; *i.e.*, their proneness to consider that what appears evident to them *cannot but* be known to others. I do not think that this can be called modesty; it is rather a species of diffidence due to their consciousness that in general their accurate knowledge of the published developments of science is confined mainly to those branches to which they have specially devoted themselves. Their foreign competitors, on the other hand (especially the Germans), are often profoundly aware of all that has been done, or, at least, have some one at hand who is, and can thus, when a new idea occurs to them, at once recognise, or have determined for them, its novelty, and so instantly put it in type and secure it. Neither Stokes nor Thomson, in 1850, seems to have had the least idea that he had hit on anything new, especially as they had a vague recollection that Foucault had previously attacked the problem—the matter appeared so simple and obvious to them—and, but for the fact that Thomson has given it in his public lectures ever since (at first giving it as something well known), they might have thus forfeited all claim to mention in connection with the discovery. I could mention many other striking instances of this peculiarity; one, in fact, appeared in our own *Proceedings* a few months ago; but to consider it more closely would lead me away from the subject of my lecture. It is sufficient to have called attention to a want which could easily be supplied, if we

had anything in this country equivalent to the *Fortschritte der Physik*, but published with considerably less delay.

Detailed study of Solar Spectrum—mainly due to the labours of two men.

Maps by Kirchhoff and Ångström, with the number of elements proved to exist in the sun's atmosphere.

According to Ångström, the following numbers of bright lines given by elements are found exactly coincident with dark lines in the solar spectrum :—

Hydrogen,	4	Manganese,	57
Sodium,	9	Chromium,	18
Barium,	11	Cobalt,	19
Calcium,	75	Nickel,	33
Magnesium,	4 + (3 ?)	Zinc,	2 (?)
Aluminium,	2 (?)	Copper,	7
Iron,	450	Titanium,	118

He notes that Thalén has found 200 coincidences with Titanium lines.

Types of Stars—Secchi.

I. White stars—Scarcely any absorption lines, except those due to Hydrogen, which are strongly marked. Sirius, Vega, &c.

II. Yellow stars—The Sun, Arcturus, Aldebaran, &c.—multitudes of fine lines.

III. Nebulous bands in addition to the fine lines—α Herculis, α Orionis, &c. In Mira Ceti these bands vary with the apparent magnitude. Similar appearances are observed in the spectra of sun-spots. On the contrary, Algol retains the first type through all its periodic changes.

IV. Feeble spectrum crossed by bright lines. The stars of this type are all of small apparent magnitude (*i.e.* of feeble luminosity), and usually of a blood-red colour. Temporary Stars—bright lines of hydrogen.

If to these be added

V. Resolvable Nebulæ—Continuous spectrum, as are those of the nebula in Andromeda, and of many others *not* resolvable; and

VI. Planetary Nebulæ, and others irresolvable, such as those of Orion, Lyra, &c., where the spectrum consists of a very few bright lines only.

it seems to me that we have a series of indications of what (for want of a better phrase) may be called the *period of life* of a star or group; beginning with the glowing gases developed by the impacts of the agglomerating cold masses (VI.), * then the almost perfect spectrum of white-hot liquid or compressed gas (V., I.), which (as it becomes colder) suffers absorption by the rise of still colder vapours (II.); then, as it farther cools, nebulous bands take the place of sharp lines (III.); anon the bursts of glowing gases are

* See the Abstract of my paper on Comets, Proc. R.S.E., 1868–9.

brighter than the photosphere (IV.), and, finally, no light but that of these gases is intense enough to reach us (VI.) That there is energy enough to produce these successive developments is obvious from the fact that, even at their immense distance, the *visible* portions of the nebulæ of Orion and of Argus subtend an angle of nearly *four degrees.*

Application of the spectroscope to determine the RELATIVE VELOCITY OF A STAR, OR OF A GASEOUS CURRENT IN THE SOLAR PHOTOSPHERE, WITH REGARD TO THE EARTH.

Analogy from sound.
Railway whistle.
Tuning-fork experiment.
Similar experiment with organ-pipe.

Finally, ABSORPTION BY BODIES AT ORDINARY TEMPERATURES.

Coloured glasses.
Chlorophyll.
Detection of blood, changes of the blood-spectrum by oxidation, &c., &c.
Microscopic spectroscope.

The following Communications were read :—

1. Note on the Early History of Spectrum Analysis. By H. Fox Talbot, Hon. F.R.S.E.

Newton, in his observations on the spectrum, appears never to have used a narrow aperture. In fact there was nothing, in the existing state of knowledge in his day, to lead him to suppose that this would alter the phenomena.

Wollaston was the first who observed some obscure bands in the spectrum, by viewing with a prism the aperture left by the shutters of his room when nearly closed. It is surprising that this acute philosopher did not follow up the hint thus accidentally presented to him, but contented himself with the rude observation above mentioned.

Fraunhofer was the first who detected the wonderful system of dark lines in the solar spectrum, by viewing a very narrow and accurately formed aperture with an excellent prism, aided by a small telescope. He likewise gave names to the principal dark lines which have been generally adopted, and he measured accurately their refractive indices by mounting the prism on a graduated brass circle movable round a centre.

After completing his observations on the solar spectrum, he

turned his attention to the spectrum of the stars, of which he described several. He likewise described the spectrum of electric light, but only that of sparks passing through atmospheric air. He has likewise left on record a very curious observation on the spectrum presented by the exterior flame of a wax candle. When the bright flame is intercepted by a screen, and only the faint exterior flame viewed, he found it to consist almost entirely of homogeneous yellow light; but his skill as an observer was so great that he perceived this yellow light to consist of two distinct rays very close together, and only separable by an excellent prism, and a very narrow aperture. As he remembered that there was a similar double ray in the yellow part of the solar spectrum which he had named D, the happy thought occurred to him of transmitting solar light through the same aperture. He did so, and found that the two rays of the line D coincided most accurately with the double yellow ray given by the exterior flame of a wax candle. He does not appear to have prosecuted this interesting research further. He merely records the fact. He was not aware that the yellow light of the candle was in any way caused by the presence of *sodium*, the existence of which in a wax candle would probably not occur to any one, unless perhaps to an experienced chemist on the look out for some extraneous substance.

About the same time Sir D. Brewster had been seeking for a source of homogeneous light, for the purpose of improving the microscope by destroying all chromatic aberration of the lenses. See his paper of 1822 in the Transactions of the Royal Society of Edinburgh, vol. ix. p. 433. Although acquainted with the effect of salt on the flame of burning alcohol, he had evidently only cursorily examined it, since he says "salt *or nitre*," which is incorrect, and speaks of its causing the flame to yield "insalubrious vapours." He therefore rejects the use of it, and merely recommends that the alcohol should be "largely diluted with water." The yellow light so obtained he refers to "imperfect combustion" (p. 435), and not in any way to *sodium*, observing that the combustion of paper, linen, cotton, or the flame of a blow-pipe, also contain the same homogeneous yellow light in tolerable abundance. His observations, therefore, have a certain resemblance to those of Fraunhofer.

About the year 1824 or 1825, Dr Wollaston gave one of his

evening parties, to which men of science and amateurs were invited, and it was the custom to exhibit scientific novelties, and to make them the subject of conversation.

On the evening in question I brought as my contribution to the meeting some very thin films of glass (such as are shown in glass-houses to visitors by a workman, who blows a portion of melted glass into a large balloon of extreme tenuity, and afterwards crushes the glass to shivers). Such a film of glass I brought to Dr Wollaston and his friends, and after showing that in the well-lighted apartment it displayed a uniform appearance without any markings, I removed it into another room, in which I had prepared a spirit lamp, the wick of which had been impregnated with common salt. When viewed by this light, the film of glass appeared covered with broad nearly parallel bands, which were almost black, and might be rudely compared to the skin of a zebra. Similar bands, but much fainter, were seen by transmitted light. All present agreed that this curious phenomenon could only be due to the extreme homogeneity of the light of the lamp with the salted wick, which much exceeded any previous estimate of it. It did not occur to any one that evening to procure a lens and a plate of glass, in order to try the effect of the light on Newton's rings. But such an experiment tried soon afterwards revealed an astonishing augmentation of the number of rings visible. I followed up this observation by publishing a paper in 1826 (Brewster's Journal, vol. v. p. 77), in which I determined, among other things, the following facts, namely, that all the salts of soda gave the yellow line D, which I therefore affirmed to be characteristic of sodium. That the salts of potash give a violet light, together with a single red ray situated almost at the end of the spectrum, and with no other light near it. [Subsequently Brewster made careful observations upon this ray, and found it to be coincident with A in the solar spectrum, a remark which recent researches with more powerful instruments have shown to be not entirely exact. Brewster did one great service in pointing out the fact that in inquiries like this an *achromatic* telescope is not necessary.]

The following is a quotation from this paper (vol. v. p. 77):—"The flame of nitre contains a red ray of remarkable nature. This red ray possesses a definite refrangibility, and appears to be cha-

racteristic of the salts of *potash*, as the yellow ray is of the salts of *soda*. *If this should be admitted, I would further suggest that whenever the prism shows a homogeneous ray of any colour to exist in a flame, this ray indicates the formation or the presence of a definite chemical compound.*"

Further on, speaking of the spectrum of red fire (such as is used in theatres and in fireworks), I said, "the other lines may be attributed to the antimony, strontia, &c., which enter into this composition. For instance, the orange ray may be the effect of the strontia, since Herschel found in the flame of *muriate of strontia* a ray of that colour If this opinion should be correct, and applicable to the other definite rays, *a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to detect.*"

An early paper by Herschel has been omitted in its proper place, the year 1822 (Transactions Royal Society of Edinburgh, vol. ix. p. 455). He there shortly describes the spectra of chloride of strontia, chloride of potassa, chloride of copper, nitrate of copper, and boracic acid.

In 1827 (after the publication of my experiments in 1826), he stated in the Encyclopædia Metropolitana, article on Light, p. 438, that salts of soda give a copious and purely homogeneous yellow; those of potash a beautiful pale violet. He also describes the spectra of lime, strontia, lithia, barytes, copper, and iron.

In another paper of mine (Phil. Mag. 1834, vol. iv. p. 114), the flames of strontia and lithia are examined. The following is an extract from this paper:—"The strontia flame exhibits a great number of red rays, well separated from each other by dark intervals, not to mention an orange, and a *very definite bright blue ray*. The lithia exhibits one single red ray. Hence I hesitate not to say that optical analysis can distinguish the minutest portions of these two substances with as much certainty, if not more, than any other known method."

Another passage, taken from the same page, records the first observation of those peculiar rays at the violet end of the spectrum. to which some years later Herschel gave the name of the *lavender* rays. "The flame of Cyanogen separates the violet end of the spectrum into three portions, with broad dark intervals between.

The last of those portions is so widely separated from the rest as to induce a suspicion that it may be more refracted than any rays in the solar spectrum. This separated portion has a pale undecided hue. I should hardly have called it *violet* were it not situated at the violet end of the spectrum. To my eye it had a somewhat whitish or greyish appearance."

This was followed by another paper of mine "On Prismatic Spectra" (Phil. Mag. 1836, vol. ix. p. 3), in which the spectra of gold, copper, zinc, boracic acid, and barytes are described.

Wheatstone, nearly at the same time, published some interesting analogous researches. I regret not to have his paper at hand at present, in order to give a full account of it.

Brewster then took up the subject, and described the spectra produced by the combustion of a great variety of substances, in a paper printed in the Manchester meeting (1842) of the British Association (see Proceedings of the Sections, p. 15). But in the same page there is another short paper by Brewster, of surpassing interest, since he there announces the fact that the bright rays which are characteristic of artificial flames are for the most part those which are deficient in solar light, a fact previously confined to the line D, and discovered, as we have said, by Fraunhofer. These observations of Brewster deserve to be quoted textually. His paper is entitled "On Luminous Lines in certain Flames corresponding to the defective Lines in the Sun's Light."

After noticing Fraunhofer's beautiful discovery as to the phenomena of the line D in the prismatic spectra, Sir David said—"He had received from Fraunhofer a splendid prism, and upon examining by it the spectrum of deflagrating nitre, he was surprised to find the red ray discovered by Mr Talbot, accompanied by several other rays, and that this extreme red ray occupied the exact place of the line A in Fraunhofer's spectrum, and equally surprised to see a luminous line corresponding to the line B of Fraunhofer. In fact, all the black lines of Fraunhofer were depicted in the spectrum in brilliant red light. The lines A and B in the spectrum of deflagrating nitre appeared to be both double lines, and upon examining a solar spectrum under favourable circumstances, he found bands corresponding to these double lines. He had looked with great anxiety to see if there was anything analogous in other

flames, and it would appear that this was a property which belonged to almost every flame."

One thing only was wanting in order to complete this discovery of Brewster's, namely, to explain why the rays which are bright in artificial flames should be dark in the solar spectrum. The explanation of this fact was reserved for later inquirers.

The above is far from exhausting the catalogue of Brewster's researches on the spectrum. He made numerous measurements of Fraunhofer's lines and maps of certain portions of the solar spectrum. He likewise discovered the extraordinary effect of nitrous gas upon the spectrum transmitted through it, which becomes covered with a vast multitude of lines, irregularly disposed, but always appearing in the same places in the spectrum, provided the density and temperature of the gas is the same.

2. On Some Optical Experiments. By H. F. Talbot, Hon. F.R.S.E.

I. On a New Mode of observing certain Spectra.

The attention of the scientific world has been for some years past fully awakened to the importance of observing the spectra exhibited during the combustion of chemical substances. But in making an extensive series of such experiments, it must often happen that the observer has to test substances of which he only possesses a very minute quantity. In that case, before he has viewed the spectrum long enough to feel fully satisfied of its nature, his stock of the substance is exhausted, and he is obliged to leave his observation imperfect. He might perhaps be testing some mineral in his cabinet, of which the native locality was unknown, and he might surmise it to contain a new metal, from its yielding a ray not before seen in the spectrum, yet after a short time his observations on it would come to an end, and he would have no means of showing this ray to other observers. Some years ago the metal thallium was so rare that it was only distributed by a few grains at a time to those who were interested in its discovery; and many of the rarer metals are absent from most chemical laboratories, or only represented by trifling specimens. About four or five years ago I devised a method of remedying, or, at least, greatly diminishing

this inconvenience, which, with some slight recent improvements, I will now proceed to describe. My method was founded on a fact which I had observed many years ago, namely, that the mere presence of a chemical substance in a flame frequently suffices to cause the appearance of its characteristic rays, and that it is not at all necessary that the substance should be consumed and dissipated. This dissipation is an accident, and if by any means it could be prevented, the flame would maintain its characters for a considerable time. For instance, in Brewster's Journal for 1826, vol. v. p. 77, &c., I remarked that alcohol burnt in an open vessel, or in a lamp with a metallic wick, gives but little yellow monochromatic light, while if the wick be of cotton, it gives a considerable quantity, and that for an unlimited time. And I added that I had found other instances of a change of colour in flames, owing to the *mere presence* of a substance which suffers *no diminution in consequence.* Thus, a particle of muriate of lime on the wick of a spirit lamp will produce a quantity of red and green rays for a whole evening without being itself sensibly diminished.

Mindful of these experiments of 1826, when a few years ago I wished to examine the spectra of thallium and other substances, I adopted the following plan :—A grain, or sometimes much less, of the substance was placed in a piece of strong glass tube about one inch long. Short platina wires were inserted into the tube at each end, approaching each other within about half an inch. The ends are then sealed by a blow-pipe, leaving enough of the platina wire outside the tube to allow of its being soldered to a long copper wire. One of these copper wires (with the external portion of the platinum wire soldered to it) was then coated with gutta percha for the space of three or four inches next the tube. To coat the other wire was found unnecessary. The mode of experimenting was as follows. The tube in a horizontal position, having the chemical substance nearly in its centre, was lowered into a glass of water about two or three inches below the surface. The two wires were then connected with a Ruhmkorff's coil, set in action by six of Grove's cells. When the sparks were allowed to pass through the tube, they speedily ignited the substance, and caused it to give forth its characteristic spectrum. Even after the sparks have been passing for several minutes, the tube remains perfectly cold. This

is the object of placing it under water, for if that precaution is not taken the tube will sometimes become very hot, and explode. The gutta percha covering is to prevent the spark passing through the water, and to oblige it to pass through the tube. It is sufficient, as I have said, to cover one wire. If a drop of water has been enclosed in the tube along with the chemical substance, the colours of the spectra are displayed with more vivacity; but if this is done, it is absolutely necessary to have the tube well under water. The bright light given off under these circumstances by strontia, sodium, thallium, and many other substances, is very beautiful, and so permanent that at the close of the experiment the original grain or half grain of the substance does not appear diminished, and even the drop of water is found remaining unchanged. Provided always that the chemical substance is one not liable to decomposition under these circumstances of heat and moisture. In these experiments a small Ruhmkorff's coil was found to answer better than a very large one.

This method might be usefully applied to the illumination of microscopic objects by homogeneous light. If the tube were placed immediately under the stage of the microscope, the full intensity of the yellow light would fall upon the object.

All these experiments were made in the Physical Laboratory of the University of Edinburgh by the kind permission and assistance of Professor Tait.

II. On the Nicol Prism.

Many years ago, when this beautiful and useful optical instrument was new and very little known, I wrote a paper in a scientific journal calling attention to its merits, and recommending its use. It was first described by its inventor in Jameson's Journal for 1828, p. 83. The title of the paper being "*On a Method of so far increasing the Divergency of the two Rays in Calcareous Spar that only one Image may be seen at a time.*" This paper was reviewed in Poggendorff's Annalen for 1833, p. 182, who says—That he perused Mr Nicol's account of his invention with very little hope of its proving successful, but that having constructed the instrument, he found that nothing could answer more perfectly than it did. Having read this testimony to its merits, I had one made by a London optician.

which proved very successful. I then published a paper on it in the Phil. Mag. for 1834, vol. iv. p. 289, from which I must ask leave to make an extract, as a necessary introduction to what I wish to say about it on the present occasion.

My paper begins by quoting the testimony of the German writer to the merits of the instrument, and continues thus:—

"Poggendorff then goes on to say, that as Mr Nicol had not attempted to explain the operation of the instrument, he would endeavour to do so, in which, however, I cannot say that I think he has been entirely successful. Now, it will be observed that the inventor attributed the fact of the instrument's producing only one image to a great 'divergency' which it causes in the images, throwing one of them aside out of the field of view. The German writer follows the same idea, but adds, that in his opinion such divergency is caused by the Canada balsam, whose index of refraction being 1·549, is intermediate between that of the ordinary ray 1 654 and that of the extraordinary ray 1·483, which circumstance will (in his opinion) account for the rays being 'thrown opposite ways.' He adds, that any one 'who was not afraid of the trouble' might easily calculate the path of both rays, a remark which shows that his idea was that they were both transmitted, and diverging from each other. But I find that this great divergency does not, in point of fact, exist, for by inclining the instrument a position may be found in which both images are seen, and they are then very little separated, not more so than they were by the same piece of spar before its bisection and cementation. On gradually altering the position of the instrument, the second image is not seen to move away from the first; but at a certain moment it vanishes suddenly without leaving the smallest trace of its existence behind. Having thus described the appearances as I have found them, I will give an explanation of them, which I hope will be more satisfactory. As long as the rays composing the images are incident upon the Canada balsam at moderate obliquities, it cannot exert any particular discriminating action upon them. But when the obliquity reaches a certain point, one of the images suffers total internal reflexion, because the Canada balsam is (with regard to that image) a less refractive medium than calc spar. But with regard to the other image, it is at the same

moment a more refractive medium than the spar, and therefore it suffers that image to pass alone."

The preceding remarks were published in the year 1834. Soon afterwards I perceived that if my explanation were correct, a Nicol prism might be made, half of calc spar and half of glass. Theory indicated this, but no actual experiment of the kind was made at that time. Recently, however, my attention has been once more directed to this subject, and I have had such an instrument constructed by Mr Bryson, optician, of Edinburgh, with a very satisfactory result. When light has been polarised by an ordinary Nicol prism, it is completely extinguished by the new prism held in a proper position; whereas when two Nicol prisms are combined, a small portion of light generally remains visible.

Either end of the new prism may be held foremost, a result which was not altogether expected. An idea is prevalent that the action of an ordinary Nicol prism is due to the circumstance that one surface of the calc spar is left *rough* to scatter one of the rays. But such is not the case. Both surfaces are highly polished by the best makers, and the ray is not scattered, but reflected, and may be seen by proper management.

3. Note on a New Scotch Acidulous Chalybeate Mineral Water. By James Dewar, F.R.S.E.

It is generally known that this country is extremely deficient in well-marked chalybeate waters. Plenty natural waters, containing small proportions of iron, are to be met with in the United Kingdom; but, with the exception of those of Tunbridge Wells, Harrogate, Sandrock (Isle of Wight), Heartfell, near Moffat, and Vicarsbridge, in the vicinity of Dollar, they contrast very unfavourably with those of the numerous spas of the continent of Europe. If we restrict ourselves to an examination of the chemical characters of the above-mentioned Scotch chalybeates, we observe that the iron is present in large quantities in the form of sulphate, along with sulphate of alumina, on which account they are more nauseous to invalids, and are at the present time rather unpopular.

Recently my brother, Dr Alexander Dewar, Melrose, sent me for

analysis a sample of a new well water, whose peculiarity had previously attracted his attention. A chemical examination of the water in question showed it to be a well-defined acidulous chalybeate, unusually rich in carbonate of iron. The following are the analytical details. (As the surface water gets access at present, a very exhaustive analysis appeared unnecessary):—

Carbonate of iron, . .	17·5	grains per gallon.
Alumina, . . .	1·8	„
Silica, . . .	8·5	„
Sulphate of magnesia, .	7·8	„
Chloride of calcium, .	16·0	„
Carbonate of calcium, .	4·1	„
Alkaline chlorides, .	11·4	„
Total residue, .	67·1	„

Carbonic acid gas per gallon 40 cubic inches.

With the exception of the celebrated "Dr Muspratt's chalybeate," at Harrogate, which contains 10·8 grains per gallon of carbonate of iron, along with 16·0 grains of protochloride, I do not know of any natural water in this country containing such a large proportion of iron in the form of carbonate. And it is to be observed that the water is not associated with a large quantity of other salts.

The well whence the foregoing sample was taken has not been long sunk, and its water is perfectly different from all of those in its immediate vicinity. Should it maintain its present character, I have no doubt that, judging from its own qualities, as well as from its favourable climatic situation, along with the general interest attached to the locality, this chalybeate is certain to recommend itself to the medical profession.

The following Gentleman was admitted a Fellow of the Society :—

THOMAS J. BOYD, Esq.

Monday, 29th May 1871.

PROFESSOR CHRISTISON, President, in the Chair.

The following Communications were read :—

1. On the Homologies of the Vertebral Skeleton in Osseous Fishes and in Man. By Professor Macdonald.

Abstract.

After a brief notice of the seven bi-vertebral segments of the cranium in man:—

1. The hypo-cranial, or the axis and atlas vertebræ, which is adopted as a key to the cranial segments;
2. Para-cranial, or occipital;
3. Wormi-epiotic parietal, or meta-cranial;
4. Sphenoidal, or meso-cranial;
5. Ethmo-frontal—pro-cranial;
6. Nasal, or apo-cranial.
7. Rhino-nasal.

Professor Macdonald gave a short outline of the osteology of the human cranium, in order to trace the homologous osteology of the osseous fishes, or ichthyia.

The great characteristic of the vertebralia is the centro-chord, or axis, extending through the whole length of the animal from stem to stern, around or upon which the vertebral column has been developed This has been demonstrated in the very earliest type, both by the late Professor Goodsir and Professor Owen in the Amphioxus, where the direction of the anterior portion, as far as the oral cleft, is to the tip of the nose from the anterior portion of the representative of the spinal marrow. The same proof may be adduced from the condition of the early human embryo, where the anterior of the embryo, consisting of the pro-cranium and part of the tubercles of the spine, are at once bent downwards, towards the upturned coccygeal extremity of the spine, where the umbilicus is afterwards formed, when the abdominal or ventral laminæ unite to close in the abdomen. There is another flexure of the pro-cranium and the meso-cranium in warm blooded vertebrata.

It is very important to notice this last flexure as distinctly marking the difference between the warm and cold-blooded animals, and to account for the necessity of the temporal squamo-zygomatic limb-bearing girdle connecting the anterior and posterior cranium.

From this zygoma, or limb-bearing zone or girdle, the maxilla depends as the anterior thoracic limbs, as seen in the annulozoa and arthrozoa. The condyle being articulated in the glenoid cavity, it is the upper or homotype of the brachium and femur, and the homologue of the quadratum of the bird, hypotympanic, and of osseous fishes (28, Owen).

He then directed the attention to the reduced scale of the fish cranium. The general form, from the great depression of the ethmo-frontal segment, prevents the formation of a pros-encephalon, and even the meso-encephalon is crushed back into the III. or wormi-epiotic parietal segment; the only encephalic cavity in the fish cranium, where not only the orbit and the convolutions and olfactory cells, but also the whole otic sensory apparatus with the cerebellum. This segment is closed in by the development of the wormi-epiotic spine, which has hitherto been described by all anatomists, from Cuvier and others on the Continent, and by Professors Owen, Huxley, Parker, and all their followers, as the occipital bone in the fish. A careful re-examination of the subject will correct this general and inconsiderate error. In the osseous fishes the occipital bone still exists in the bi-vertebral condition. It, however, contains the medulla oblongata, and their long spines extend upward, as they do in the human cranium, to nearly the wormi-epiotic spine.

Referring to the archetype of Owen, the basi-sphenoid (5.) was shown to be the last vertebral centrum, from whence the basi-cranium extended, without central joints, to the anterior glabella frontis. (13, incorrectly named vomer) is in fact the premandible or incisor bone. (13.) The vomer is a vertical, or mediastinal double osseous septum, set on the rostrum sphenoides (olivaris) in connection with the perpendicular plate of the ethmoid and septum nasi separating the olfactory cells.

From (4) the wormi-epiotic tuber or spine the upper part of the ischium is attached by a chain of transparent bent scale-bones containing a muscle, seems the principal part of the pelvis; it has a large

tuberosity; from the inner part the ramus rises.* From the inner and lower surface of the tuber ischii the femur (51) descends. It is from the inner articulation in the fishes, instead of the external acetabulum in the human pelvis, that the relation between the tibia (52) and fibula (58) is altered. The fibula is articulated within the head of the tibia; the femur overlaps the upper spine of the head of the tibia. The external malleolus tibiæ is very greatly prolonged, and forms the great osseous sub-opercular cleft, while the internal malleolus fibulæ is embedded in the skin behind the tarsal fin.

The tarsal fin consists of calcaneum (55), astragalus (53), scaphoid (54). These Cuvier named radius and ulna, in which he was followed by Owen, &c. Anterior cuneiform and cuboid tarsals (56). The phalangeal fin rays (57).

The mistaken homology of the pectoral fin for the anterior instead of the posterior extremity baffles all chance of correct homology, and I earnestly hope that all the living homologists will re-examine the subject, and adopt the system which I have wrought out for between forty and fifty years without succeeding to convince the anatomists. I put forth this final appeal of the oldest of living homologists who proposed an original scheme (my friend, Professor Grant, University College, London, introduced that of the brilliant but fanciful Geoffroy St. Hilaire some years earlier), with the firm conviction that ere long, after I have retired, the scheme now proposed will be adopted.

* Owen's Nomenclature.	Macdonald's Nomenclature.
50. Supra-scapula.	50. Ischium.
51. Scapula.	51. Femur.
52. Coracoid.	52. Tibia.
53. Humerus.	53. Astragalus.
54. Ulna.	54. Scaphoid.
55. Radius.	55. Calcaneum.
56. Carpal.	56. Tarsal.
57. Metacarp-phalanges.	57. Tarsal fin rays.
58. Epicoracoid.	58. Fibula.

2. Scheme for the Conservation of Remarkable Boulders in Scotland, and for the indication of their Positions on Maps. By D. Milne Home, Esq.

Among many geological questions which wait solution, there is probably none more interesting or perplexing than the agency by which Boulders or "blocs erratiques," as the French term them, have come to their present sites. I allude, of course, not to blocks lying at the foot of some mountain crag from which they have fallen by the decay or weathering of the overhanging rocks, but to blocks which have manifestly been transported great distances, after being detached from the rocks of which they originally formed part.

That many of the large isolated blocks lying on our mountain sides and on our plains have come from a distance, and by some means of tremendous power, is obvious even to an unscientific observer; and the perception of this truth by the popular mind has, in many cases, so invested these boulders with superstitious interest, that they have received names and given rise to legends, which impute the transport of them to supernatural agents.

There are two circumstances which very plainly indicate that these stones are strangers.

One is, that many of these blocks are on examination found to be different from any of the rocks prevailing in or near the district where they are situated.

The other is, that some of these blocks, whilst excessively hard,—so hard that it is difficult to break off a portion with the hammer, are nevertheless round in form—a form evidently acquired by enormous friction—such friction as would result from being rolled a long way over a rough surface.

The inference drawn from these two facts was confirmed when it was discovered, as in many cases it was, that rocks of the same nature as the block existed in a distant part of the country, and from which, therefore, it had probably come.

These round shaped blocks were the first to attract popular attention. The name given to them in Scotland of *boulders* has no doubt been suggested by their shape.

It is accordingly only the rounded boulders which possess the

traditionary names and curious legends by which many of them are known. Such names as the Carlin's Stane, the Witch's Stane, Pech or Pict's Stone, Clachannadruid, Kirk-Stane, Pedlar's Stane, Thuggart Stane, and Devil's Putting Stane, are all applicable to rounded blocks.

When the geologist turned *his* attention to the subject, it was soon discovered that there were many blocks equally entitled to be called erratic, not round but square shaped; and which, though discovered to belong probably to rocks at a great distance, yet showed signs of little or no attrition. Moreover, many of these angular or sharp-edged blocks were comparatively soft and loose in structure, so that they could not have been rolled, for any considerable distance, without being broken or crushed into pieces, or into sand or mud.

On a more minute inspection and study of these erratic blocks, certain features were noticed which seemed to indicate the forces to which they had been subjected. Thus on many of them, deep scratches, ruts, and groovings were found, as if sharp pebbles or stones harder than themselves had been pushed over them, or squeezed against them under great pressure. It was also observed that, when a block had a long and a short axis, the longer axis was generally parallel with any well marked scratches or striæ on their surface; and moreover that the direction of these striæ frequently coincided with the direction in which the block itself had apparently come from the parent rock.

These circumstances soon led geologists to speculate on the nature of the agencies which could have effected a transport of the blocks. Some blocks are of enormous size, exceeding 1000 tons in weight.* Many, before they could have reached the places where they were found, must have travelled fifty or sixty miles, and have crossed valleys and even ranges of hills. In the county of Berwick, for example, there is a large block of gneiss, a rock which exists nowhere in that county or in the south of Scotland; and if it came from some of the hills in the Highlands, it must have crossed, not only the valley of the Forth, but the Kilsyth, Pentland, and Lammermoor Hills.

* The celebrated block near Neufchâtel, called "Pierre à bot," contains about 1480 cubic yards of stone, and is supposed to weigh about 2000 tons.

Sir James Hall and Sir George Mackenzie in this Society, who were the first to study the subject, advocated the idea of diluvial agency. At a later period, ice in various forms was suggested as the agent,—First, in the condition of glaciers filling our valleys; next, in the condition of icebergs floating over our island, whilst under the sea; and latterly, as a great sheet or cake stretching from the Arctic regions, and overspreading the whole of northern Europe.

It is not my intention to discuss these theories, or say which appears the most probable. I allude to them now, merely to indicate the tremendous character of the agencies, which it is found necessary to invoke for the solution of the problem,—agencies all implying a very different condition of things in Scotland, as regards configuration of surface and climate, from what now prevails. These phenomena are the more interesting, because, as most of the erratic blocks lie above all the rocks, and very frequently even above the beds of clay, gravel, and sand, which constitute the surface of the land we inhabit, they indicate probably the very last geological changes which occurred in this part of the earth's surface, and which there are some grounds for supposing, may even have occurred since this country was inhabited by man.

The basis on which geologists have been obliged to build their theories, it must be admitted, is somewhat narrow. It consists merely of observations made casually by individuals, who have noticed certain appearances in districts of Scotland which they happen to have visited; and, therefore, it is little to be wondered at, that more than half a century has been required for procuring the information, scanty as it is, which has been obtained.

What appears desirable for expediting the solution of the problem, is to organise a staff of observers, and to parcel out the country amongst them, for the purpose of observing facts likely to throw light on the subject, and of making these facts known from time to time, both with a view to verification, and as a basis for further speculation.

It has occurred to me, that the numerous natural history societies and field clubs existing in Scotland, would be valuable agents in this investigation; and, moreover, that individual geologists would be pleased to co-operate in their respective districts.

I hope no one will think that the object for which I suggest this investigation, is not worthy of the trouble which it implies, and of the patronage which I ask this Society to bestow on it. These erratic blocks bear the same relation to the history of our planet, as the ancient standing or memorial stones do to the history of the early races of mankind. These last-mentioned stones,—sometimes with sculpturing on them not yet understood,—sometimes arranged in circles or other regular forms not yet explained,—sometimes found in connection with sepulture, are beheld and studied with interest, on account of the gleams of light which they throw on the people who erected them; and popular indignation justly rises, when any of these prehistoric records of our ancestors are destroyed or mutilated. The great boulder stones to which I have been referring would, if investigated and studied, in like manner cast light on the last tremendous agencies which have passed over whole regions of the earth. It is therefore important to have as many of these boulders as possible discovered and examined, and to have such of them preserved as seem worthy of study. I need not say how rapidly, during the last century, both classes of ancient stones have been disappearing; and therefore, if it be desirable to preserve the most remarkable boulders, or at all events to record their existence, and their geological features, the investigation which I advocate, cannot be too soon begun.

Alike in illustration and in recommendation of this suggestion, I will refer to an investigation for the same object commenced two years ago in Switzerland, and in the adjoining parts of France. The design was twofold,—*First*, the conservation of remarkable boulders situated on the Jura and in Dauphiny; and *second*, the recording of their positions by maps, and of their characteristic features by schedules.

With this view a circular was drawn out, and issued by the Swiss Geological Commission, pointing out the scientific bearings of the subject, and invoking the co-operation not only of provincial societies, but also of municipal authorities in the cantons, and of landed proprietors. A few extracts from the Swiss circular may not be inappropriate :—

"These erratic blocks are composed of granite, schist, or lime-" stone; but they rest on rocks of a different description. They

" were so remarkable by their number and size, that, from an
" early period, they attracted the attention of naturalists, and
" suggested scientific inquiries. It is, indeed, interesting to seek
" to comprehend how enormous masses, with from 40,000 to 50,000
" cubic feet of contents, and weighing from 800 to 1000 tons, could
" be transported from the Alps from which they were evidently
" detached, to spots 40 and 50 leagues distant, crossing deep
" valleys, such as the lakes of Geneva, Neufchatel, Zurich, Con-
" stance, Lucerne, &c.

" This great problem has been discussed by numerous philo-
" sophers, both of Switzerland and of foreign countries." Then follows a list of names, including those of our own Playfair, Lyell, Murchison, Forbes, Tyndall, and Ramsay.

" Unhappily," (the circular goes on to state), " during the last
" 100 or 150 years, these erratics have been broken up for building
" purposes, and even for road metal. Recently the work of destruc-
" tion has gone on more rapidly, and, unless stopped, the result
" will be to obliterate all traces of one of the greatest facts in the
" natural history of our country.

" Though the destruction of these blocks is now advancing with
" great rapidity, there are still a number of very large specimens
" left, and these the Geological Commission is anxious to pre-
" serve."

" The members of Archæological Societies are interested in the
" conservation of these blocks, for they often bear those curious
" sculpturings, to which much importance is now justly attached."

" The lovers of legends must regret to see these blocks disap-
" pearing, for ancient tradition tells how some have been flung by
" the Devil on a poor hermit; that another bears the name of a
" fish merchant in a town of which there is now no trace, &c.

" The Geological Commission considers that the time has come
" for appealing to all who have any power over the fate of these
" blocks, that is to say, to individual proprietors, to communal
" authorities, and to municipalities. The Commission also entreats
" natural history societies, Alpine clubs, and public bodies, to co-
" operate in this work, in order to preserve for Switzerland a
" feature of the country, which, if not altogether peculiar to it, is
" at all events better developed there than in any other

Besides making an appeal for the conservation of boulders, the same Swiss Geological Commission suggested the propriety of marking their exact position on the Government maps.

They farther expressed a hope that these measures might reach even beyond the frontiers of Switzerland, and they referred to an offer made by a French geologist to draw up an account of the Erratics of *Souabe*, with the view of obtaining co-operation from that quarter.

A committee was appointed to carry out these views, supply the necessary schedules and maps, and conduct the correspondence.

I shall next explain what resulted from the appeal. The circular containing it was issued in the autumn of 1867, and I now quote from a report presented to the Helvetic Society of Natural Sciences at a meeting in August 1869, drawn up by Messrs Favre and Soret.

They state that, very soon after the commencement of the investigation, it was found desirable not to limit it to boulders, but to include a description of enormous heaps of gravel, existing in many districts, having the appearance of ancient moraines, and in that view likely to throw light on the mode in which the boulders were transported. Accordingly, instructions were given to indicate on the maps the position of these gravel accumulations as well as of boulders.

Messrs Favre and Soret then narrate what had been done during the previous year in the different cantons, and from their report I give the following extracts :—

In the first place, they acknowledge the liberality of Colonel Siegfried, the Director of the Federal Topographical Department, in supplying maps to assist in recording the observations.

They farther acknowledge the assistance which Colonel Siegfried had given to the investigation, by issuing instructions to the engineers surveying the slopes of the Jura, to indicate on the maps, and to describe in their reports, any remarkable erratic blocks they met with.

Reference is next made to the proceedings of the societies and clubs in the different cantons. In some of the larger cantons, as *Lucerne* and *Vaud*, the country had been divided into five and six compartments, and a small sub-committee of members had been appointed to explore each. In one of these cantons, the municipal

authorities had given orders to the inspectors of roads and bridges to aid in the investigation.

In the canton of *Zurich*, notice is taken of one remarkable block, known as the "Stone for the sacrifices *of Hegsrüti*," which had been purchased by the Society of Antiquaries, and had been brought into the town of Zurich.

In the canton of *Soleure*, blocks of enormous size, and to the number of 228, had been marked, and appointed by the municipal authorities to be preserved, these blocks being situated on lands belonging to the canton. The celebrated block of Steinhof, weighing about 1400 tons, had been purchased by means of a special subscription, and made over in property to the Helvetic Society.

Several landed proprietors are named as having gifted particular boulder stones to the societies. Thus Mr Briganti, at *Monthey*, had gifted to the Helvetic Society one block out of a remarkable group, of which I well remember the late Principal Forbes once spoke in this Society, and which I had lately an opportunity of visiting. So also Mr Bonneton of Geneva had presented to the Alpine Club of that town a piece of land, containing what is described as a magnificent boulder, and known by the name of the "Stone of Beauregard."

Even the Federal Government of Switzerland had condescended to share in what really seems to amount almost to a national movement; for reference is made to an official communication from the Chancellor, stating that the Council of State had caused an order to be issued, that all erratic blocks situated in the cantonal forests should be preserved intact, till examined by the committee.

I have had sent to me a printed report of the steps taken in the canton of *Aargau*, drawn out by Professor Mühlberg. He mentions that one of the measures taken there, was the appointment of a referee to inspect the boulders which were discovered, with the view of determining whether they were worthy of being preserved. Professor Mühlberg mentions farther, that "the State "undertakes the expense of printing and postages, as well as of "the travelling of the canton referee to the sites of the most "important boulders, and had in the meantime advanced 100 francs "to defray expenses already incurred."

These extracts from the reports, of which printed copies have

been kindly sent to me by Professor Favre of Geneva, show what is doing in Switzerland for the promotion of an object which, under the auspices of this Royal Society, I should wish to see taken up in Scotland. And before concluding what I have to say about the Swiss movement, I may refer to one circumstance which ought to be gratifying to Scotchmen, viz., that the Swiss naturalists retain a grateful recollection of what has been done by Scotchmen for exploring and making known the interesting physical features of their beautiful country. Not only have they, in specifying the names of geologists who have written on Switzerland, included all the Scotchmen who have done so, but I see in one of Professor Favre's pamphlets, written in connection with this movement, allusion to the year 1741, "when (he says) the English first pene-" trated into the valley of Chamounix,"—"and gave to that valley " a celebrity, which the previous visits of several bishops had not " obtained for it." Professor Favre records the names of these English visitors, and among them are "Lord Haddington and his " brother, Mr Baillie." The pamphlet mentioning these names I sent to the present Earl of Haddington, that he might see the courteous allusion to his ancestor; and, in returning the pamphlet, he referred me to a paragraph in Douglas's Peerage, which mentions the fact that, in the year 1740, the Earl of Haddington and his brother, George, set out on their travels to the Continent, and were for some time located with other friends at Geneva—one of these being Stillingfleet, famous in his day as a naturalist, and who in one of his works alludes to the very agreeable *reunions* of his countrymen which took place at Geneva and the neighbourhood.

I will next refer briefly to the steps taken in the south of France in co-operation with the Swiss movement. These began by a communication from Professor Favre to Mons. Belgrand, who, besides being President of the Geological Society of France, was Inspector-General of Bridges and Roads, a Government Department. This communication, which explained the object of the Swiss investigations, and also what was being done by the different cantonal societies and municipalities, was referred by Mons. Bertrand to two members, Messrs Falsan and Chantre, to report on.

It is from their report, the remarks of Mons. Bertrand upon it, and some notes of a subsequent date, published in the Transactions

of the Geological Society of France for December 1869, that I make the following extracts:—

The great interest attaching to the investigation is allowed by the reporters, and a compliment is paid to the Swiss naturalists for commencing and urging it.

Reference is made to the rapid disappearance of the boulders, and especially limestone boulders, which were generally broken up for limekilns. The reporters state that near Lyons, the greater part of the boulders had been destroyed long ago, and in particular one weighing about 150 tons, which marked the point where the boundaries of three parishes met.

Examples, however, of remarkable boulders still untouched, with legends attached to some, are specified, such as the "Pierre du Bon Dieu," of 120 tons, and the "Pierre du Diable," of 56 tons, which it is strongly recommended should, with many others of less note, be saved from destruction or injury.

Reference is then made to the steps which should be taken to carry out these views. Circulars, it is said, should be drawn up, and sent not only to the public departments which superintend the management of Government or communal lands, but also to individual landed proprietors, pointing out the scientific interest attaching to these erratic blocks.

These suggestions were at once favourably responded to and acted on. Three public departments or functionaries, viz., the Minister of Public Works, the Director-General of Forests, and the Prefects in each of the provinces of Savoy, High-Savoy, Ain, Rhone, and Isère—all adjoining Switzerland—are stated to have lent their willing co-operation.

After the project had received the approbation of the Geological Society of France, and the promise of important official support, an appeal to the friends of Natural Science was drawn up by Messrs Falson and Chantre very similar to the appeal which had been previously drawn out and issued in Switzerland. This appeal, after describing the movement and proceedings in Switzerland, proceeds thus:—"Such is the object pursued vigorously in Switzer-"land with the co-operation of departments and of individuals. "In a word, see what is going on near ourselves. Can we remain "outside of, and indifferent to, this scientific enterprise, especially

"when Mons. Favre has asked us to engage in the same work, and "to undertake for our country what he is doing for his? We are "bound to answer this appeal. The solution of the same questions "ought to occupy us. These erratic phenomena abound every-"where in our district. The debris of rocks torn from the Alps "cover the plain of Dauphiny, the plateau of the Dombes, the hills "of Croix, Rousse, and Sainte-Foy. Already many geologists "have studied these erratic phenomena in our neighbourhood, "without being able to discover a solution. The truth, when we "seek it, seems to fly from us; but we must persevere and pursue "it till it is caught.

"Our desire is simply to prevent the destruction of the most "remarkable blocks, and leave them on their natural sites, and "also to obtain a collection of specimens to illustrate them, and "we hope that our administrations will in this object not be behind "those of Switzerland and the department of Haute Savoie. Their "example would, we doubt not, be followed by individual proprie-"tors, where boulders cease to be regarded as mere masses of stone "of unusual size, but without scientific value."

Besides this appeal, printed copies of which were extensively circulated, directions and schedules were drawn out to be transmitted to local societies as well as to individuals who should undertake the investigation, in particular districts, maps of these districts being at the same time supplied.

The documents from which I have made these extracts were, as I have said, transmitted to me by Professor Favre of Geneva. He wrote to me at the same time, and concluded his letter by saying, "Voila, Monsieur, un aperçu de la marche de cette entreprise. Je "serai bien heureux, de le voir s'etendre a l'Ecosse."

In a subsequent letter he repeats his suggestion thus:—"Si vous "pouvez organiser quelque chose de semblable en Ecosse, vous "m'obligerez infiniment, en me tenant au courant."

In a third letter, he says, "Permettez moi de vous renou-"veller la demande que je vous ai addressé, en vous priant de me "tenir au courant de ce que nous ferez pour les blocs erratiques de "l'Ecosse, et des resultats que vous obtiendrez."

I have given these details of the proceedings in Switzerland and France, and quoted these passages from Professor Favre's letters,

in order both to add weight to my proposal, and show how we may proceed to attain it.

I have alluded to the existence throughout Scotland of many provincial societies whose objects are not inconsistent with the investigation which I think they may be invited to engage in. Sir Walter Elliot of Wolflee has lately been at pains to make out a list of all the Natural History Societies and Field Clubs existing in Great Britain and Ireland.

I now give this list, in so far as it applies to Scotland, in the hope that, when our proceedings are published, this list may appear in it, so that if any societies or clubs are seen to have been omitted, the omission may be taken notice of and supplied.

1. Berwickshire Naturalist's Club. (*Secretary*, Mr Geo. Tate, Postmaster, Alnwick.)
2. Hawick Archæological Society. (*Secretary*, David Watson.)
3. Tweedside Physical and Antiquarian Society.
4. Dumfries and Galloway Natural History and Antiquarian Society.
5. Edinburgh Geological Society. (*Secretary*, Geo. A. Panton, Hope Terrace.)
6. Edinburgh Naturalists' Field Club. (*Secretary*, Andrew Taylor, 5 St Andrew Square.)
7. Glasgow Natural History Society. (*President*, John Young, M.D.; *Secretary*, Robert Gray, 2 Lawrence Place, Dowanhill.)
8. Glasgow Geological Society. (*President*, John Young, M.D.; *Secretary*, Dugald Bell, 136 Buchanan Street.)
9. Alloa Society of Natural History and Archæology.
10. Largo Field Natural History Society. (*Secretary*, Charles Howie.)
11. Perth Literary and Antiquarian Society.
12. Perthshire Society of Natural History. (*President*, Dr Buchanan White; *Secretary*, A. T. Scott.)
13. Montrose Natural History Society. (*Secretary*, Mr Robert Barclay.)

14. Aberdeen Natural History Society.

15. Aberdeen Philosophical Society. (*President*, Professor Ogilvie, M.D.; *Secretary*, Alex. D. Milne, 37 Thistle Street.)

16. Natural History Society, Elgin.

17. Orkney Natural History Society.

Being myself a member of one of these Societies, I know that some of its members have devoted themselves to the subject of boulders, and of moraine-looking deposits, occurring within the district over which the operations of the Society extend.

Sir Walter Elliot tells me that he has information of a Field Naturalists' Club in England which has specially directed its attention to the boulders of the district.

It is quite true that, in Switzerland and in the south of France boulders, considerable in size and numbers, are much more abundant than in Scotland, so that little searching is required to enable the provincial societies of these countries, to carry out the investigation proposed to them.

On the other hand, let it not be imagined, that in Scotland the boulders generally are not of such interest as to deserve the adoption of proceedings similar to those now being adopted in Switzerland and France. Even within the limited range of my own discoveries, I know and have measured eight boulders in the south-east of Scotland, the smallest of which is 10 tons and the largest 918 tons in weight, and all possessing features more or less significant.

There are others equally large which I have heard of, but have not seen. Moreover, almost all these boulders have old traditional names, and many of them legends which indicate, that they have been objects of popular and even superstitious regard.

There are two objects which ought to be aimed at. The first is to obtain a list of all boulders which appear remarkable; *i.e.*, remarkable for size, and instructive on account of polishing, ruts on the surface, or any other circumstance. The second is to put down on maps, a mark to represent the exact position of boulders, occurring in groups, or of large individual boulders.

Moreover, accumulations of gravel, sand, or clay in any district, in so far as they seem to have been produced by agents now no longer operating in the district, should be notified.

In order to carry out these suggestions, I would venture very respectfully to ask that the Council of this Society should pass a Special Minute expressing approval of the subject explained in this paper, and appointing a Committee of the Fellows of this Society to carry out farther proceedings. The circumstance that this Society had expressed its approval, and taken steps to aid the investigation, would alone ensure for it a favourable consideration.

The Committee would, of course, communicate with the various provincial societies throughout Scotland, by enclosing a copy of this paper or an abstract of it, and intimating readiness to send the necessary Schedules and Directions, should a willingness be expressed to enter on the investigation proposed.

I have in these remarks alluded only to the steps necessary for discovering the existence of remarkable boulders, indicating their position on a map, and obtaining a correct description of them. But the other object, which also engages attention so much in Switzerland and France, should not be lost sight of here. I allude to the conservation of boulders. The disappearance of numerous camps, buildings, standing stones, and other objects of archæological interest in all our counties, which every one now regrets, has been owing in a great measure to ignorance on the part of the proprietors and tenants on whose lands they were situated, of the value and even nature of these objects. But this work of destruction has been happily now stopped, and chiefly by the interference and influence of our Society of Antiquaries. In like manner, the demolition of Boulders which has been going on rapidly in Scotland, will, I hope, be arrested, when the proprietors and tenants on whose lands they stand, are made aware of the interest they excite, and of what is being done to preserve them in other countries. Of course, it would only be certain boulders which it would be desirable to preserve, boulders remarkable for size, or shape, or position, or for markings upon them; and when a report was made to the Committee of any boulder of this description, the Committee would judge whether an application should be made to the proprietor on whose lands it was situated, to spare the stone, so that it

might be preserved for examination and study. I have little doubt that such an appeal would be attended to. Indeed, in the great majority of cases, a proprietor would be pleased to learn, that an object of scientific interest had been discovered on his estate, and would be glad to have it in his power to accede to any request in relation to it coming from a Committee of this Society.

With regard to the mode of meeting the expenses attending the investigation and other proceedings suggested in this paper, it occurs to me that subscriptions from individuals should be chiefly relied on, and that the Council of this Society should only promise such aid as the state of the Society's funds and their appreciation of the proceedings of the Committee, may suggest to them. The Committee will, no doubt, make a Report at least once a year of their proceedings, which the Council may allow to be read at a meeting of the Society, if its contents were sufficiently interesting.

3. Note of a New Form of Armature and Break for a Magneto-Electric Machine. By R. M. Ferguson, Ph.D.

The magneto-electric machine, which I am about to describe, approximates in its general arrangements to Ladd's hand-machine. In it Mr Ladd makes use of a compound Siemens' armature, consisting of two separate armatures placed in length, and revolving round the same axis, with their coils at right angles to each other. The armature revolves between the poles of an electro-magnet, of the description introduced by Mr Wilde. The electro-magnet, in the present instance, is made of a rectangular piece of boiler-plate, three-quarters of an inch in thickness, bent so as to form three sides at right angles to each other, as shown (in section) in fig. 1. The upright sides (P P′ P) are nearly 9 inches high and 11 inches in length, and the top of the same length is 6 inches broad. Pieces of cast-iron (N and S) are put in the open end to form the poles of the magnet. About 300 yards of a double No. 14 wire, wrapped round the upright sides, make the coil (C C C C) of the electro-magnet. One of the armatures in Ladd's machine furnishes a current to the coil of the electro-magnet; the other gives out an external current. To distinguish the two, the counterparts of which occur in the arrangement I bring before you, I shall call the first the inter-

nal current, and the second the external current; and the coils furnishing them I shall designate the magnetic coil and the electric coil respectively. The action of the magnetic coil is based on Siemens' and Wheatstone's principle of reciprocal increase. When a Siemens' armature revolves between the poles of an electro-magnet, what feeble magnetism there may be in the iron core generates a feeble current in the armature coil. This current, by a commutating arrangement of revolving collar and springs, is sent into the coils of the electro-magnet, which in consequence rises in

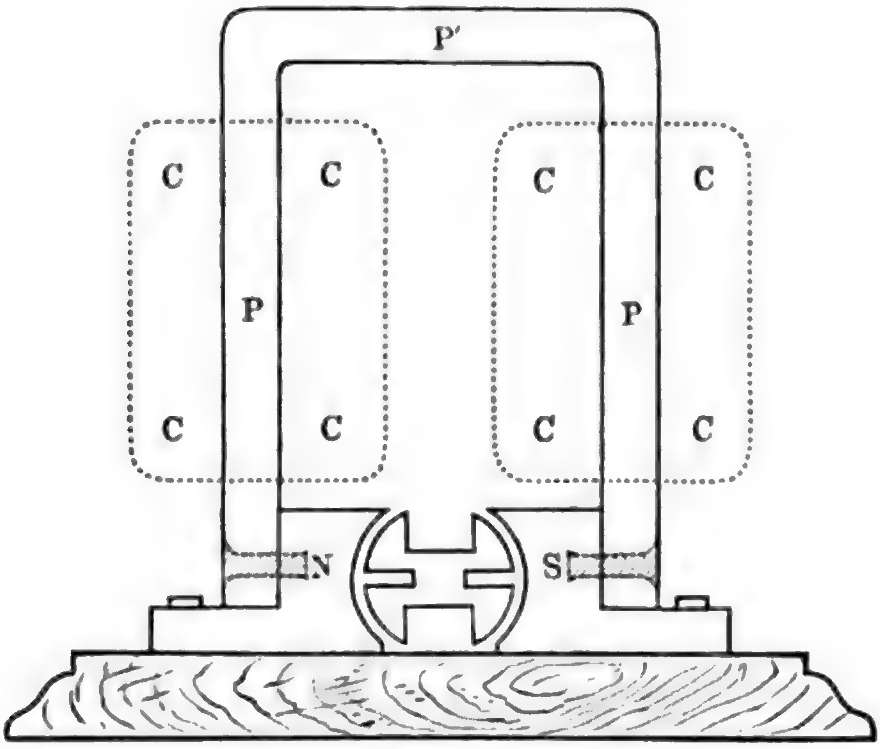


Fig. 1.

power. It is now able to excite a stronger armature current, thereby rendering itself still more powerful, and this mutual action goes on until the driving force is insufficient to continue the action. Ladd has ingeniously turned this principle to account in his machine, the magnetic coil of which furnishes electricity for the electro-magnet, and this last is thereby rendered competent to generate electricity in the electric coil available for external use.

Wishing to make a machine to give off a current equal to a few cells of Bunsen, I thought of trying the following deviation from Ladd's construction:—Instead of having two separate armatures revolving on the same axis, I thought one might serve, in which two coils were inserted, the one at right angles to the other. In the revolution of a Siemens' armature there are two polarities, so

to speak, one only of which is utilised, viz., that which takes place (fig. 2) when the greatest length of the iron core lies in the line joining the two poles; the other polarity ensues when this main axis is perpendicular to the line of poles (fig. 3). This second

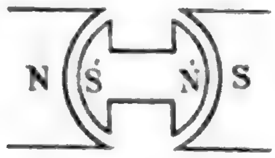


Fig. 2.

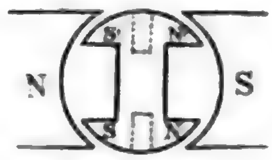


Fig. 3.

polarity is, from the less favourable position of the core, necessarily weaker than the first; but it struck me that it might be quite sufficient to furnish the internal current, leaving to the more powerful polarity the task of generating the external current. Another advantage seemed to flow from this utilisation. When an armature without coil or closed circuit revolves within a magnet, the energy expended in its motion heats its particles. When the core is provided with a coil and closed circuit, part of this energy, instead of assuming the form of heat, is transmuted into the energy of an electric current, and the electricity induced is so much deducted from the heat that would otherwise appear in the armature. In the ordinary construction the weaker polarity, being unprovided with a coil, results only in heat; but if it be furnished with such, as in the arrangement I suggest, and its molecular energy thereby tapped, so to speak, the heat of the armature may be partially withdrawn in the shape of an electric current. A current sufficient to magnetise the electro-magnet may thus be got, for no additional expenditure of force, but only by the conversion of heat that would otherwise be mere waste, so far as the action of the machine was concerned. When one of Wilde's small machines, in which a battery of permanent magnets is used instead of an electro-magnet, is turned by the hand, additional resistance is felt on the armature circuit being closed more especially by a short wire. The current got from the armature would thus seem to be formed partially from the conversion just mentioned, and partially from a new access of force demanded by the creation of the current. In the arrangement I here describe, a different action takes place, for when the coil of the electro-magnet is disjoined from the magnetic coil and included in the circuit of a single Bunsen cell, the feeling

of diminished resistance is nearly as decidedly felt as that of increased resistance in Wilde's machine on closing the electric coil circuit. The same feeling is not so decided in the case of the magnetic coil, and this, no doubt, arises from its smaller dimensions; at any rate, there is no additional force needed. Whether this action has its origin in an essential difference in the action of permanent magnets and electro-magnets in these circumstances, or in some peculiarity of construction, is immaterial to the present inquiry, for to all appearance the armature currents cost no additional energy, but are got entirely from the waste heat of the armature.

The core of the armature (fig. 4 *a*) is 11 inches long and $2\frac{1}{2}$ inches in diameter. The main longitudinal cut or groove is $1\frac{3}{8}$ inch wide and $\frac{1}{2}$ inch deep. The small cut is $\frac{3}{8}$ of an inch wide and $\frac{3}{4}$ of an inch deep.* In the large cut is wound the electric coil, consisting of a cable of 8 silk-insulated wires, $\frac{1}{40}$ of an inch in diameter, and 82 feet long. The magnetic coil in the small cut is made of a cable of four such wires, 46 feet in length. The electric coil thus contains about four times as much wire, and offers about the same electric resistance as the magnetic coil.

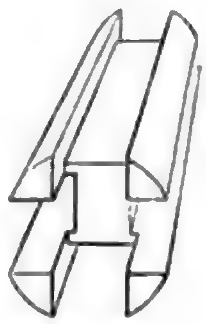


Fig. 4 *a*.

The two grooves leave four protruding ends at each end of the armature. To these are screwed a bronze cap and spindle of revolution (figs. 4 and 5, which are on a larger scale than fig. 4 *a*).

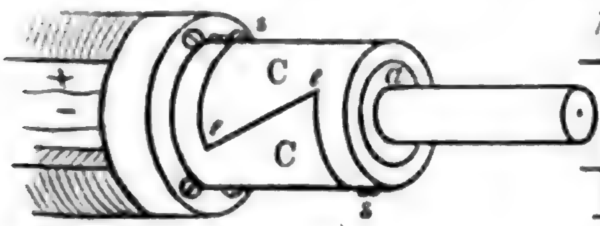


Fig. 4.

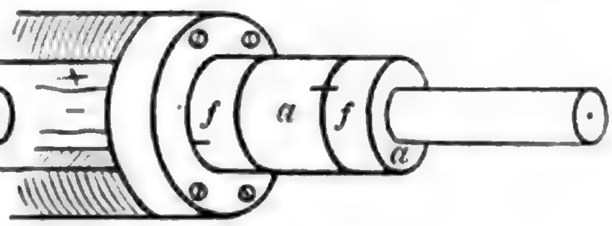


Fig. 5.

A collar of wood (*a*) is fixed next to the spindle, and on this collar two ferrules of iron (*f f* fig. 5) are put, separated by the wood to prevent contact. To these ferrules the wires from the coils (+ -) are soldered, care being taken to prevent unnecessary contact. A cylindrical collar (C C fig. 4) turns on the ferrules, and can be turned round and fixed in any position by screws (*s s* fig. 4). The collar is made up of three parts, two pieces of iron (one is shown

* In the figure both cuts to be shown clearly appear of the same size.

in fig. 7) cut out of the same tube and kept from touching, by being fixed to a vulcanite ferrule (v in fig. 6, which shows the inside of half the collar) placed inside and between them. The ends of the iron pieces slide on the iron ferrules beneath, and are in conducting connection with them. Electrical contact is made by springs pressing on this composite collar, and which are metallically connected with the binding screws, the poles of the armature coils. The collar and springs at each end form the breaks or commutating arrangement of their respective coils. The cross line of separation (e e fig. 4) can be fixed in any position, and currents in one or different directions thereby obtained in the course of a revolution. The pressure of the springs against the collars is regulated by screws.

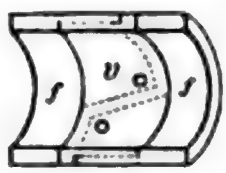


Fig. 6.

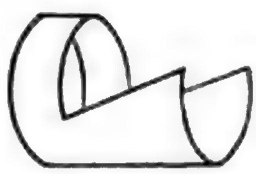


Fig. 7.

When the machine is prepared for working, the cross lines of the commutating collar of the magnetic coil are placed at right angles to the plane of the coil, the position of maximum effect. If the handle of the machine be turned when the circuit of the electric coil is open, one or two turns bring the hand of the operator to something like a dead halt; the resistance to further motion is so great as to challenge its continuance. If, now, the external circuit be closed, immediate relief is felt, as if part of the internal current had been diverted into the external circuit from the coils of the electro-magnet. The relief thus experienced, moreover, bears some proportion to the conductivity of the external circuit. With an easy circuit, the work expended in turning the handle is easy; with a resisting circuit, the driving resistance becomes correspondingly great. The hand is thus made to sympathise with the nature of the external circuit, and the experimenter feels as if he were charged mechanically with a resistance offered electrically. Suppose, for instance, we have a piece of thin wire to heat or melt; at first little or no driving resistance is felt, but the moment that the wire begins to get hot, the arm becomes charged with a heavy resistance, which grows as the wire rises in temperature till it melts, and then suddenly the excessive no-circuit

resistance is felt. The moment that there is hard work to be done in the external circuit, the strength of the arm is put to the proof. When water is decomposed by the machine, the strain upon the arm does not rise beyond a certain amount, at whatever speed the handle be driven. In working an induction coil, the load on the arm appears capable of rising to any extent, and the length or density of the spark bears something like a proportion to the burden of work. With an electric resistance great enough, and an inexhaustible driving power, there seems no limit to the electric effect attainable, and that, too, with little increase of speed.

When a tangent galvanometer is interposed in the external circuit, something may be learned of the way this takes place. With an easy circuit, where little difficulty is felt in driving, a current of about 60° may be got. When a thin wire is now interposed, the needle does not reach this point, for the wire (iron wire $\frac{1}{85}$ inch in diameter) melts or ignites between 30° and 40°, and yet while the heating lasts the strain is enormously greater than before. If the galvanometer be inclosed in the internal circuit, and the wire melted in the electric circuit, just at the point when the heating begins, the needle takes a sudden swing upwards. Thus, if it be at 20° before the heating sets in, it will rise to 30°, and stay there till the wire melts, when, if the motion be continued, it again takes a start upwards. If the magnetic coil be detached from the coil of the electro-magnet, and if its function be performed by one Bunsen cell, this increase of load is not felt, a greater effect in the external circuit being only attainable by an increase in velocity, and the same holds with a battery of permanent magnets.

That two separate coils, by being imbedded in the same piece of iron, should thus act upon each other seems strange. One might almost think that it arose from the particles of iron refusing to polarise and unpolarise quick enough. The maximum speed of revolution of the armature is about 2500 times a minute. The driving gear multiplies 22 times, so that this speed is nearly as much as the arm can effect. A particle of iron would have thus 10,000 times to polarise and unpolarise in a minute. A little consideration will show, however, that it is from no such incapacity on the part of the iron; for at the same rate of revolution, the two effects are felt with the different circuits. Speed in these cases, therefore, has not

overshot the mark. The cause of the action appears to me as follows:—When the line of the armature (fig. 8) is vertical—when, in fact, the strongest action is taking place in the small coil—the wires of the large coil cut the lines of magnetic force between N and S at right angles, the best time and the best place for a current to be induced in them. Although, then, the longitudinal polarity of the iron has disappeared, the coil takes up the action and makes a north and a south end, even when the main line of the armature is upright, and should be free from polarity. This coil induction or polarity is feeble, contrasted with that resulting through the iron, and would have little effect if the coils were near each other in size. It is only in the present case, where there is such a disparity between the coils, that the interference grows to a sensible amount. In support of this view of the matter, it may be mentioned that when the larger coil is connected with the electro-magnet, little relief is felt on an easy circuit being made for the smaller coil. The effect of the interference is to lessen the current induced in the smaller coil. A particle at *a*, for instance (fig. 8), which when left to the action of the poles of the electro-magnet would give its full quota of electric induction, is by the cross polarity magnetically forced round, so to speak, into a less favourable position for doing so. But how is this interference stopped by a resisting external circuit? In this way, I imagine. The available electro-motive power may take the form of large quantity in an easy circuit, or little quantity in a resisting circuit. On consulting the galvanometer in a resisting circuit, while the strength is taxed to the utmost, the current is often found weak. It is the quantity of electricity that is the cause of the interference, and not the work value of the circuit. When the strength of the electric current is great with a resisting circuit, that of the magnetic current has been proportionally exalted.

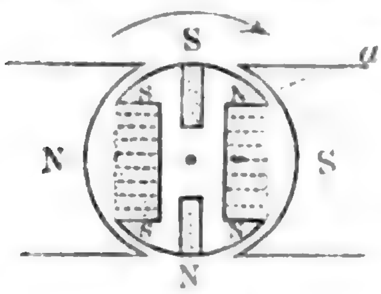


Fig. 8.

The interference of the two coils with each other can be shown in a simple way. When the coil of the electro-magnet is detached from the magnetic coil and joined up with a Bunsen cell, we have, on turning the handle, both armature coils prepared to give ex-

ternal currents. If, in the circuit of the electric coil, a few inches of fine platinum wire be included, and the circuit of the magnetic coil half completed, so that one end of the connecting wire has only to touch the other binding screw to close it, and the handle be put in sufficient motion, the platinum wire becomes white hot, and this sinks to a dull red when contact in the magnetic circuit is made. The same takes place when the coils are reversed. Such an action as this suggests the supposition that what appears in the second coil is but electricity stolen from the first, and that the arrangement effects only a convenient distribution, and not an increase of the electricity available. I cannot, with the observations I have yet made, say that such is not true in all cases, but in one case, at least, the only one I have examined, such a supposition cannot be entertained, and that is when both coils work together in the same circuit. When both coils, as just mentioned, are ready to give external currents under the magnetism induced by one Bunsen cell, it is quite possible, by accustoming the ear to the note produced by the springs rubbing on the revolving collars, to get the arm to work at a uniform speed. If the cell be steady, you can, within a fraction of a degree, produce the same angle in the galvanometer in the same circumstances. I have made repeated observations in this way as to what current the electric coil would give when acting alone, as to what the magnetic coil would give, and as to what both together would effect. The circuits in these cases consisted of the coils themselves and the wires leading to a tangent galvanometer some 12 feet off, and the working of the machine and the observing of angles were done by different persons. The resistances in both circuits were sensibly the same. The resistance of the electric coil was 32 inches of a German silver wire in my possession, that of the magnetic coil 34, and that of the galvanometer wire 5 inches. To these must be added the resistance introduced by the imperfect contact of the break-springs, which, at a high speed, and especially in the case of the machine exhibited where the armature is not quite truly centred, must be considerable. The difference between the two coils would thus almost disappear on the total resistances of their respective circuits. This being the case, the work value of the electricity appearing in each will be as the squares of the tangents of the angles observed. Now, in all the

observations I have made, the sum of these for the two coils separately was approximately equal to that obtained when both currents were sent into the galvanometer circuit. To give an idea of how nearly this comes out, I may cite one observation repeated three times in succession with the same result. I found the angle of both together to be $47\frac{1}{2}°$, that of the electric coil separately 40°, and that of the magnetic coil separately 34°. Now the square of the tangent of $47\frac{1}{2}°$ is 1·1909, and the sum of those of the other two 1·15905.

The theory of the machine, as I understand it, may be thus shortly summed up. In one case, namely, that of an easy common circuit, and it is likely to be more or less so in all cases, the two coils contribute each their full quota to the total electric fund of the armature. When the resistance of the circuits differ, this fund is divided inversely in some function of the relative resistance, but whether this takes place so as to excite the electro-magnet at no original expense of driving energy is still a matter for further determination. The results got from the machine would lead us to suspect as much, for they compare favourably with machines where a permanent battery of magnets is used; but this test, though so far satisfactory, is far from exact.

The interference of the coils seems to me to be a hopeful feature of the arrangement, as it does not make increased power simply dependent on increased velocity. There is a promise in it that by adjusting the relative sizes of the coils a powerful current may be got at a really practicable speed, and there would thus be obviated the serious objection to this class of machines, which, however astonishing in their power, are apt to wear themselves out by their rapid rate of motion when kept in action for days together. Even in the machine before you, if the collars were properly turned and centered, so as to give good contact with the springs at all rates of revolution, I have reason to believe that its effective speed of revolution would be very much diminished.

In mentioning what a machine like this can do, considerable latitude must be understood in interpreting results. The strength or ardour of different workers may tell very differently. The only fair way would be to give the electric effect corresponding to a weight falling so far per second, but this involves opportunities of

experiment which I have not at my command. When I say that 6 inches of soft iron wire $\frac{1}{80}$ of an inch in diameter can be melted or ignited by it, I only mean to say that the arm of an ordinary man, working briskly for a second or two, can accomplish this, though it would be hard work for him to continue the same for a minute. A stronger arm than usual, or a more ardent labourer, would do much more than this. A battery of six Bunsen cells, each with an effective surface of 42 square inches, melted 5 inches of the same wire. With an induction coil a spark of $1\frac{1}{2}$ inches can be got with an expenditure of labour that may be continued for a minute or two; with intense exertion a spark of 5 or even more inches may be got. By working reasonably for a minute from $2\frac{1}{2}$ to 4 cubic inches of explosive gas can be got from a voltameter; working very hard for a quarter of a minute at the rate of 6 inches or more may be obtained. To turn a handle some 100 times a minute, more especially against some resistance, is not work that can be easily continued for minutes; and such machines, when driven by the hand, are only good for incidental, not continuous use. To keep down the pull on the hand with a resisting circuit, the commutating collar of the magnetic coil has to be turned round from its position of maximum effect. There is a certain speed at which the hand can best work, for slow and difficult motion is not so convenient nor attended by so good results as quick and easy motion.

The machine is well adapted for an educational instrument, viz., for illustrating electro-magnetic action. If the electro-magnetic coil be joined with one cell of Bunsen, and the electric coil with five or six cells, the conditions of the machine are reversed; and now electricity produces motion, instead of motion producing electricity. The handle is made to go round with considerable velocity, and if the belt that connects the gearing with the handle be removed, the armature alone spins round at a great rate. If now the poles of the magnetic coil be joined, the armature instantly slows, and the slowing is all the more marked the less the resistance of the circuit offered. The current of this new circuit can raise to a white heat about a $\frac{1}{4}$ inch of fine platinum wire. It may be worth mentioning, that the current given off by the magnetic coil under these conditions is singularly steady, and that its strength is something like inversely proportional to the circuit resistance. This slowing of

the armature seems at variance with what I have stated before, that less instead of more driving resistance is felt in closing either of the armature circuits, for here the new current seems to be paid for out of the motion of the armature. The discrepancy may possibly be accounted for by the consideration that both coils are now antagonistic in their action, and that whatever part of the induced current appears in the magnetic coil, from whatever source derived, goes directly to oppose the conditions favourable to motion, and that between the opposing actions more heating in the core may be the accompaniment or equivalent of slower motion. When the coil of the electro-magnet is joined with the larger (electric) coil, so that a wire has only to touch the unconnected binding screw to close the circuit, and when the arm puts the machine into rapid motion, it is brought to an instant, one might say an impotent halt, on the wire touching the binding screw. One cannot help thinking, in trying such an experiment, that coil-brakes or drags may be yet extensively used in machinery.

Whether this machine be any improvement or even a rival to existing machines, I do not pretend to say. I only wish in this paper to bring the peculiarities of its action before the notice of the Society.

4. Mathematical Notes. By Professor Tait.

1. On a Property of Self-Conjugate Linear and Vector Functions.

In the course of an investigation connected with the free rotation of a rigid body I was led to the remark that, if ξ and η be two vectors related to one another so that

$$\xi = \mathrm{V}.\eta\phi\eta\ ,$$

where ϕ is a self-conjugate linear and vector function, we have also

$$\eta = \mathrm{V}.\xi\phi\xi\ ,$$

(so that the relation is reciprocal) provided

$$\mathrm{S}.\eta\phi\eta\phi^2\eta = 1\ ,$$

which implies also the corresponding equation

$$\mathrm{S}.\xi\phi\xi\phi^2\xi = 1\ .$$

The surface of the third order, represented by either of the two latter equations, is well known, and the property above shows a curious relation between certain of its vectors and those of a central surface of the second order. It has also interesting applications to the lines of curvature of the surface.

If ξ and η be unrestricted, the theorem above may be put in the more general form that the two following equations are consequences one of the other, viz.:—

$$\frac{\xi}{S^{\frac{1}{3}}.\xi\phi\xi\phi^2\xi} = \frac{V.\eta\phi\eta}{S^{\frac{2}{3}}.\eta\phi\eta\phi^2\eta},$$

$$\frac{\eta}{S^{\frac{1}{3}}.\eta\phi\eta\phi^2\eta} = \frac{V.\xi\phi\xi}{S^{\frac{2}{3}}.\xi\phi\xi\phi^2\xi},$$

From either of them we obtain the equation

$$S\phi\xi\phi\eta = S^{\frac{1}{3}}.\xi\phi\xi\phi^2\xi\, S^{\frac{1}{3}}.\eta\phi\eta\phi^2\eta,$$

which, taken along with one of the others, gives a singular theorem when translated into ordinary algebra.

2. Relation between corresponding Ordinates of two Parabolas.

Two projectiles are anyhow projected simultaneously from a point, what is the relation between their vertical heights at any instant?

This simple inquiry, which was instituted in consequence of some results recently obtained from thermo-electric experiments (see *ante*. p. 311) carried on at high temperatures, where the indications given by two separate circuits, immersed in the same hot and cold bodies, were used as ordinate and abscissa, leads to a very curious consequence.

Let

$$x = At(B - t)$$

and

$$y = A't(B' - t)$$

be any two parabolas whose axes are vertical, and which pass through the origin. We have

$$y = \frac{A'x - Ay}{A(B - B')}\left[B - \frac{A'x - Ay}{AA'(B - B')}\right].$$

or

$$(A'x - Ay)^2 = AA'(B' - B)(ABy - A'B'x).$$

This, again, is the equation of a parabola, which passes, like the others, through the origin, but whose axis is no longer vertical. The converse suggests another easy but interesting problem.

If we write ξ for $\frac{x}{A}$, η for $\frac{y}{A'}$, and f and f' for the halves of B and B′, we easily see that the last equation above becomes

$$(\xi - \eta)^2 = 4\overline{f - f'}\,(f'\xi - f\eta).$$

Every parabola passing through the origin may have its equation put in this form. Hence, as ξ and η are dependent on one another (in the thermo-electric as in the projectile case) only as being both functions of temperature, or of time, it is obvious that we must seek to break this expression up into a linear relation between functions of ξ and η separately. A well known transformation leads to

$$\sqrt{f^2 - \xi} - \sqrt{f'^2 - \eta} = \pm(f - f').$$

whence

$$\sqrt{f^2 - \xi} = \pm(\tau - f).$$

$$\sqrt{f'^2 - \eta} = \pm(\tau - f'),$$

where τ is some function of time or of temperature. These give

$$\xi = \tau(2f - \tau),$$

$$\eta = \tau(2f' - \tau).$$

Hence, in the thermo-electric case, if we obtain a parabola by using, as ordinate and abscissa, the simultaneous indications of any two circuits whose junctions are at the same temperatures, and if one of them gives a parabola (with axis vertical) in terms of absolute temperature, τ must be a linear function of the difference of absolute temperatures of the junctions, and, therefore, the other circuit gives a similarly situated parabola in terms of the absolute temperature.

3. On some Quaternion Transformations.

(*Abstract.*)

Since the algebraic operator

$$\epsilon^{h\frac{d}{dx}},$$

when applied to any function of x, simply changes x into $x+h$, it is obvious that if σ be a vector not acted on by

$$\nabla = i\frac{d}{dx} + j\frac{d}{dy} + k\frac{d}{dz},$$

we have

$$\epsilon^{-S\sigma\nabla} f(\rho) = f(\rho+\sigma),$$

whatever function f may be.

If Δ bear to the constituents of σ the same relation as ∇ bears to those of ρ, and if f and F be any two functions which satisfy the commutative law in multiplication, this theorem takes the curious form

$$\epsilon^{-S\Delta\nabla} f(\rho)\,\mathrm{F}(\sigma) = f(\rho+\Delta)\,\mathrm{F}(\sigma) = \mathrm{F}(\sigma+\nabla) f(\rho);$$

of which a particular case is

$$\epsilon^{\frac{d^2}{dxdy}} f(x)\,\mathrm{F}(y) = f\left(x+\frac{d}{dy}\right)\mathrm{F}(y) = \mathrm{F}\left(y+\frac{d}{dx}\right)f(x).$$

The modifications which the general expression undergoes, when f and F are not commutative, are easily seen and need not be indicated in this abstract.

If one of these be an inverse function, such as for instance may occur in the solution of a linear differential equation, these theorems of course do not give the arbitrary part of the integral, but they often materially aid in the determination of the rest.

Other theorems are given, involving operators such as $\epsilon^{S\rho\nabla}$, $\epsilon^{S.\alpha\rho\nabla}$, &c. &c.

The paper contains numerous applications, extensions, and interpretations of these fundamental theorems.

But there are among them results which appear startling from the excessively free use made of the separation of symbols. Of

these I now give but one, which, however, with that in the succeeding Note, is quite sufficient to show their general nature.

Let P be any scalar function of ρ. It is required to find the difference between the value of P at ρ, and its *mean* value throughout a very small sphere, of radius r and volume v, which has the extremity of ρ as centre.

From what is said above, it is easy to see that we have the following expression for the required result:—

$$\frac{1}{v}\iiint\left(\epsilon^{-S\sigma\Delta}-1\right)\mathrm{P}d\varsigma.$$

where σ is the vector joining the centre of the sphere with the element of volume $d\varsigma$, and the integration (which relates to σ and $d\varsigma$ alone) extends through the whole volume of the sphere. Expanding the exponential, we may write this expression in the form

$$-\frac{1}{v}\iiint\left(S\sigma\nabla-\frac{1}{2}(S\sigma\nabla)^2+\ldots\right)\mathrm{P}d\varsigma$$

$$=-\frac{1}{v}S.\nabla\mathrm{P}\iiint\sigma\,d\varsigma+\frac{1}{2v}\iiint(S\sigma\nabla)^2\mathrm{P}d\varsigma-\&c.,$$

higher terms being omitted on account of the smallness of r, the limit of $T\sigma$.

Now, symmetry shows at once that

$$\iiint\sigma\,d\varsigma=0.$$

Also, whatever constant vector be denoted by a,

$$\iiint(Sa\sigma)^2d\varsigma=-a^2\iiint(S\sigma Ua)^2d\varsigma.$$

Since the integration extends throughout a sphere, it is obvious that the integral on the right is half of what we may call the moment of inertia of the volume about a diameter. Hence

$$\iiint(S\sigma Ua)^2d\varsigma=\frac{vr^2}{5}.$$

If we now write ∇ for a, *as the integration does not refer to* ∇, we have by the foregoing results (neglecting higher powers of r)

$$\frac{1}{v}\iiint\left(\epsilon^{-S\sigma\nabla}-1\right)\mathrm{P}d\varsigma=-\frac{r^2}{10}\nabla^2\mathrm{P},$$

which is the expression given by Maxwell (*London Math. Soc. Proc.*, Vol. III., No. 34, 1871). Although, for simplicity, P has here been supposed a scalar, it is obvious that in the result above it may at once be written as a quaternion.

4. On an Expression for the Potential of a Surface-distribution, and on the Operator $T\nabla = \sqrt{\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2}$.

If ρ be the vector of the element ds, where the surface density is $f\rho$, the potential at σ is

$$\iint ds f\rho \, FT(\rho - \sigma),$$

F being the potential function, which may have any form whatever.

By the preceding Note this may be transformed into

$$\iint ds f\rho \, \epsilon^{S\sigma\nabla} FT\rho;$$

or, far more conveniently for the integration, into

$$\iint ds f\rho \, \epsilon^{S\rho\Delta} FT\sigma,$$

where Δ depends on the constituents of σ in the same manner as ∇ depends on those of ρ.

A still farther simplification may be introduced by using a vector σ_0, which is finally to be made zero, along with its corresponding operator Δ_0, for the above expression then becomes

$$\iint ds \, \epsilon^{S\rho(\Delta - \Delta_0)} f\sigma_0 FT\sigma,$$

where ρ appears in a comparatively manageable form. This is the expression to which the title of the Note refers. It is obvious that, so far, our formulæ are applicable to any distribution. We now restrict them to a superficial one.

Integration of this last *form* can always be easily effected in the case of a surface of revolution, the origin being a point in the axis. For the expression, so far as the integration is concerned, can in that case be exhibited as a single integral

$$\int_p^q dx \phi x \epsilon^{ax},$$

where φ may be any scalar function, and x depends on the cosine of the inclination of ρ to the axis. Now

$$\int_p^q dx\epsilon^{ax} = \frac{1}{a}\left(\epsilon^{qa} - \epsilon^{pa}\right),$$

and by operating by $\left(\frac{d}{da}\right)$ we have

$$\int_p^q dx x^m \epsilon^{ax} = \left(\frac{d}{da}\right)^m \frac{\epsilon^{qa} - \epsilon^{pa}}{a},$$

so that

$$\int_p^q dx\varphi x\epsilon^{ax} = \varphi\left(\frac{d}{da}\right) \cdot \frac{\epsilon^{qa} - \epsilon^{pa}}{a}.$$

But (as the interpretation of the general results is a little troublesome) I confine myself at present to the case of a spherical shell, the origin being the centre and the density unity, which, while much simpler, sufficiently illustrates the proposed mode of treating the subject. I hope to return to the question, and to develope at length the proposed mode of solution.

We easily see that in the above simple case, α being any constant vector whatever, and a being the radius of the sphere,

$$\iint ds\,\epsilon^{S\alpha\rho} = 2\pi a\int_{-a}^{+a} \epsilon^{x T\alpha} dx = \frac{2\pi a}{T\alpha}\left(\epsilon^{aT\alpha} - \epsilon^{-aT\alpha}\right).$$

Now, it appears (though I cannot say that I am yet quite satisfied with the *logic* of any of the proofs that have occurred to me) that *we are at liberty to treat* Δ *as* α *has just been treated.* It is necessary, therefore, to find the effects of such operators as $T\Delta$, $\epsilon^{aT\Delta}$, &c., which seem to be novel, upon a scalar function of $T\sigma$; or T, as we may for the present call it.

Now

$$(T\Delta)^2 F = -\Delta^2 F = F'' + \frac{2F'}{T},$$

whence it is easy to guess at a particular form of $T\Delta$. To be sure that it is the only one, assume

$$T\Delta = \varphi\frac{d}{dT} + \psi,$$

where φ and ψ are scalar functions of T to be found. This gives

$$(\mathrm{T}\Delta)^2\mathrm{F} = \left(\varphi\frac{d}{d\mathrm{T}} + \psi\right)\left(\varphi\,\mathrm{F}' + \psi\,\mathrm{F}\right)$$

$$= \varphi^2\mathrm{F}'' + (\varphi\varphi' + \psi\varphi + \varphi\psi)\,\mathrm{F}' + (\varphi\psi' + \psi^2)\,\mathrm{F}\,.$$

Comparing, we have

$$\varphi^2 = 1\,,$$

$$\varphi\varphi' + \psi\varphi + \varphi\psi = \frac{2}{\mathrm{T}}\,,$$

$$\varphi\psi' + \psi^2 = 0\,.$$

From the first,

$$\varphi = \pm\,1\,,$$

whence the second gives

$$\psi = \pm\,\frac{1}{\mathrm{T}}\,,$$

and the third shows that the *upper* sign must be taken. That is

$$\mathrm{T}\Delta = \frac{d}{d\mathrm{T}} + \frac{1}{\mathrm{T}}\,.$$

Also, an easy induction shows that

$$(\mathrm{T}\Delta)^n = \left(\frac{d}{d\mathrm{T}}\right)^n + \frac{n}{\mathrm{T}}\left(\frac{d}{d\mathrm{T}}\right)^{n-1}.$$

Hence we have at once

$$\epsilon^{a\mathrm{T}\Delta} = 1 + a\left(\frac{d}{d\mathrm{T}} + \frac{1}{\mathrm{T}}\right) + \ldots + \frac{a^n}{1\,.\,2\ldots n}\left[\left(\frac{d}{d\mathrm{T}}\right)^n + \frac{n}{\mathrm{T}}\left(\frac{d}{d\mathrm{T}}\right)^{n-1}\right] + \text{\&c.}$$

$$= \epsilon^{a\frac{d}{d\mathrm{T}}} + \frac{a}{\mathrm{T}}\,\epsilon^{a\frac{d}{d\mathrm{T}}}\,,$$

so that

$$\epsilon^{a\mathrm{T}\Delta}\,\mathrm{F}\mathrm{T}\sigma = \frac{\mathrm{T}\sigma + a}{\mathrm{T}\sigma}\,\mathrm{F}(\mathrm{T}\sigma + a)\,.$$

In using such a formula we must carefully remark that F is defined as a function of a *tensor, i.e.,* of *a quantity essentially positive,* so that should a be negative and of greater magnitude than $\mathrm{T}\sigma$ the quantity of which F is a function becomes $a - \mathrm{T}\sigma$.

Hence, putting Δ for a in the integrated result above, we have for the potential at σ

$$\frac{2\pi a}{T\Delta}\left(\epsilon^{aT\Delta} - \epsilon^{-aT\Delta}\right)FT\sigma .$$

That this may be constant, $= 2\pi a V$ suppose, so long as $T\sigma \nless a$, we must have

$$(T\Delta)V = \frac{2}{T\sigma}\Big[(T\sigma + a)F(T\sigma + a) - (T\sigma - a)F(a - T\sigma)\Big];$$

which gives at once

$$V = \text{constant} = 2 \text{, suppose,}$$

and

$$Fx = \frac{1}{x},$$

the gravitation law.

And as the operator becomes, for $T\sigma \geqq a$, by expansion

$$2a\left\{1 - \frac{a^2\Delta^2}{1.2.3} + \frac{a^4\Delta^4}{1.2.3.4.5} - \ldots\right\}$$

while

$$\Delta^2\frac{1}{T\sigma} = 0,$$

we have for the external potential the usual expression

$$\frac{4\pi a^2}{T\sigma}.$$

5. An Experimental Research on the Antagonism between the Actions of Physostigma and Atropia. By Dr Thomas R. Fraser. (With a diagram.)

(*Abstract.*)

In a Preliminary Note, read before this Society on the 31st of May 1869 (see *Proceedings*), a number of experiments were described, which proved that the lethal action of certain doses of physostigma can be prevented by the administration of atropia.*

* June 1871.—While this Abstract is passing through the press, the author has received a paper by M. Bourneville, in which the above result is satisfactorily confirmed by experiments on guinea-pigs.

Further, it was pointed out, that antagonism between any two substances, in the sense of the lethal action of the one being preventible by the physiological action of the other, had not previously been shown to exist by any certain and satisfactory evidence. In the various instances where experiment seemed to indicate the existence of such an antagonism, sufficient proof was not given that the dose of the substance whose action appeared to be antagonised was certainly a lethal one. The conflicting opinions and doubts this fallacy has given origin to, have induced the author to follow a plan whereby it may be completely avoided.

In the first place, the minimum fatal dose of physostigma for the species of animal employed was accurately determined by a number of preliminary experiments; so that the weight of the animal being ascertained, it was an easy matter to be certain of the dose that could kill it. Then, in those experiments where an animal recovered after the administration of a dose of atropia given in combination with a dose of physostigma, equal to or in excess of the minimum fatal, it was killed many days afterwards, and when the effects of the two substances had completely disappeared, by a dose of physostigma, equal to or less than that from which it had previously recovered. *Therefore, when the administration of atropia prevented an otherwise fatal dose of physostigma from causing death, a perfect demonstration was obtained of the power of atropia to produce some physiological action or actions that counteracted some otherwise lethal action or actions of physostigma.*

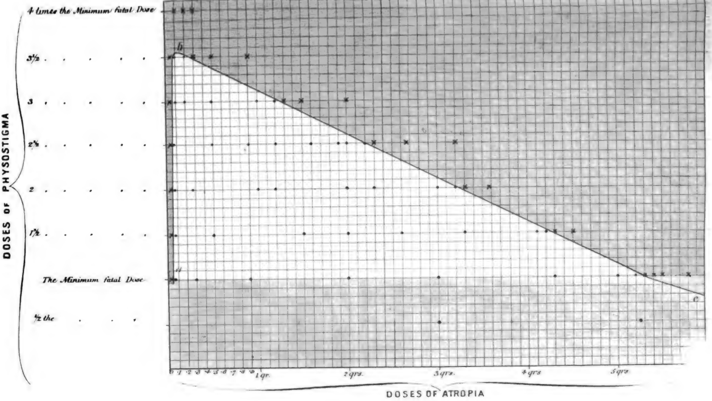
In the preliminary note referred to, it was suggested that, as both atropia and physostigma are capable of producing a number of different actions, several of which may not be mutually antagonistic, and that, as both substances are capable of producing several actions of a similar kind, considerably less potent to cause death than those by which their fatal effects are usually induced, it would probably be found that a region exists where the non-antagonised and the similar actions are present in sufficient degrees of activity to be themselves able to produce fatal results. This anticipation has proved to be correct. A large number of experiments have been made, by which the region of the successful antagonism of fatal doses of physostigma has been defined with considerable exactness. The smallest and the largest doses of atropia that are able to pre-

vent death after the administration of different fatal doses of physostigma, and the maximum fatal dose of physostigma that is capable of being rendered non-fatal by atropia were ascertained, and it was found that beyond these various points death may be produced by combined doses of the two substances, either by some non-antagonised action belonging to one or other of them, or by a combination of similar actions belonging to both.

As the above results could be obtained only by performing a very large number of experiments, rabbits were the animals selected, it being impossible to obtain a sufficient number of dogs, or other convenient animal. The weight of animal employed was, as nearly as possible, three pounds; and when below or in excess of this a correction was made, so that each dose represented three pounds weight of animal.

In one portion of this investigation, experiments were performed in which physostigma was given five minutes after atropia, both substances being injected under the skin. In the first series, the dose of physostigma was the minimum fatal, and the doses of atropia ranged from one that was too small to prevent the lethal action of this dose of physostigma, through a number of gradually increasing doses of atropia that were able to prevent death, until a dose was found whose administration resulted in death. Similar series of experiments were made with doses of physostigma one and a-half times, twice, two and a-half times, thrice, and three and a-half times as large as the minimum fatal. With the minimum fatal dose of physostigma, it was found that while ·01 grain of atropia is too small to prevent death, ·015 grain is able to do so; and that with any dose ranging from ·015 grain to 5·2 grains the lethal effect of this dose of physostigma may be prevented; while if the dose of atropia be 5·3 grains or more, the region of successful antagonism is left, and death occurs. With one and a-half times the minimum fatal dose of physostigma, successful antagonism was produced with doses of atropia ranging from ·02 grain to 4·2 grains; with twice the minimum fatal of physostigma, with doses of atropia ranging from ·025 grain to 3·2 grains; with two and a-half times the minimum fatal of physostigma, with doses of atropia ranging from ·035 grains to 2·2 grains; with thrice the minimum fatal of physostigma, with doses of atropia ranging from ·06 grain

ANTAGONISM BETWEEN PHYSOSTIGMA AND ATROPIA (ATROPIA ADMINISTERED 5 MINUTES BEFORE PHYSOSTIGMA)



to 1·2 grain; and with three and a-half times the minimum fatal dose of physostigma, with doses of atropia ranging from ·1 grain to ·2 grain. Successful antagonism could not be obtained above this dose, and, accordingly, three and a-half times the minimum fatal dose of physostigma would appear to be about the largest quantity whose lethal action may be prevented by administering atropia five minutes previously.

A similar series of experiments has been made, in which physostigma was administered five minutes before atropia, and the results were essentially the same, excepting that the region of successful antagonism was found to be more limited.

These results may be graphically represented by means of diagrams. The diagram accompanying this abstract is a reduced copy of one exhibited by the author to illustrate the series of experiments above described, in which atropia was administered five minutes before physostigma. The experiments that terminated in death are marked by crosses, and those that terminated in recovery by dots, while the position assigned to each experiment is determined by the doses of physostigma and atropia, calculated, when necessary, for three pounds weight of rabbit. The doses of atropia increase according to the distance, in a horizontal direction, from the perpendicular line forming the left margin of the diagram, and the increase proceeds at the rate of one-tenth of a grain for each subdivision of the horizontal lines. The doses of physostigma increase from below upwards, the same horizontal line always representing the same dose of physostigma. The curved line, *a b c*, separates the fatal experiments (crosses) from those which terminated in recovery (dots), and, accordingly, it defines the region of successful antagonism—a region further distinguished in the diagram by the absence of shading. The *darkly* shaded region is that in which antagonism is not successful, death being produced because the doses of atropia given in combination with one or other of the doses of physostigma employed are either too small or too large. In the *lightly* shaded region, below the horizontal line representing the minimum fatal dose of physostigma, the doses of physostigma are too small of themselves to cause death. The lateral extension of the diagram is, however, insufficient to exhibit the chief interest of this region. Were the diagram extended, it

would show that fatal experiments occur in this region, not only with fatal doses of atropia given in combination with less than fatal doses of physostigma, but also with less than fatal doses of atropia given in combination with less than fatal doses of physostigma.

In this manner, the entire *superficial area* of the region of successful antagonism has been defined, when physostigma is given five minutes after and five minutes before atropia. In addition to this, what may be termed the *thickness* of the region has been determined. For this purpose, series of experiments were made, in each of which the doses of physostigma were the same, and the doses of atropia varied; while with each dose of atropia, several experiments were made which differed from each other by a difference in the interval of time between the administration of the two substances. From the data thus obtained, curves have been constructed; the dose of physostigma serving as the base-line, the various doses of atropia as the abscissæ, and the different intervals of time that separate successful from unsuccessful experiments as the summits of the ordinates. When these curves are brought into relation with a diagram of the superficial area of the region of successful antagonism, in such a manner that the base-lines, representing the doses of physostigma, correspond to each other, and that the ordinates of these curves extend at right angles to those in the diagram of the superficial area, the lateral extension of the region of successful antagonism may be defined. In this way, its lateral as well as its superficial extent has been indicated with atropia and physostigma.

After defining the superficial area and the thickness of the region of successful antagonism, it seemed of interest to ascertain what dose of atropia is required to produce death with a dose of physostigma below the minimum fatal. The experiments performed for this purpose show that when one-half of the minimum fatal dose of physostigma is given five minutes after atropia, so large a dose of the latter substance as 9·8 grains is required in order to cause death; recovery taking place with doses ranging from 3 to 9·5 grains.

The minimum fatal dose of sulphate of atropia given alone was found to be twenty-one grains for a rabbit weighing three pounds.

It is, therefore, remarkable that the $\frac{3}{800}$ths of a grain can prevent a dose of physostigma, equal to the minimum fatal, from causing death, and that the $\frac{1}{10}$th of a grain is capable of rendering non-fatal a dose of physostigma, equal to three and a-half times the minimum fatal.

Excepting dilatation of the pupils, these minute doses of atropia, and indeed any dose capable of antagonising the lethal action of physostigma, are unable to produce any symptom recognisable by a mere inspection of the animal. Still, they undoubtedly produce energetic physiological effects—effects, however, which it is unnecessary to describe in this brief abstract. It is sufficient to point out that the notion, which exists in many quarters, that rabbits can scarcely be affected by atropia is an erroneous one.

Without referring to the other results obtained in his investigation, the author pointed out, in conclusion, that unless the antagonism between any two active substances be examined in the manner indicated in this communication, no satisfactory proof of its existence can be obtained. The superficial area of the region should always be defined, otherwise indications of antagonism obtained by one observer will be liable to be discredited by those who subsequently examine the subject. The first observer may succeed in performing an experiment within the area of successful antagonism, and thus feel satisfied of its existence; but his successors may fail in obtaining any proof by so varying the dose of one or other substance as to pass the limits of the region of success (see diagram). Feeling assured that many examples of successful antagonism, besides the one he had the honour of bringing before the Society, will yet be discovered, the author could not avoid the conclusion that the imperfect methods of investigation hitherto pursued are accountable for the absence of success that has attended the numerous researches made on this subject—a subject, it need scarcely be added, of the greatest importance to toxicology and to scientific therapeutics.

6. On the Homological Relations of the Cœlenterata. By Professor Allman, F.R.S.E.

Abstract.

In this communication an Actinozoon (*Actinia*) was compared with a Hydrozoon (*Hydra*), and the various Sub-orders of the *Hydrozoa* were compared with one another.

The author agreed with Agassiz in regarding the radiating chambers of an *Actinia* as the homologues of the radiating canals of a medusa, but he differed from him as to the true homologies of the differentiated stomach-sac of *Actinia;* for while Agassiz regards this as represented by the proboscis or hypostome of the *Hydra* inverted into its body cavity, Professor Allman maintains that it is impossible on this supposition to conceive of the structure of *Actinia;* and on comparing a *Hydra* with an *Actinia*, he imagines the tentacle to become connate for a greater or less extent with the sides of the hypostome and with one another, so that the hypostome of the hydra, while retaining its normal position, will thus become the stomach of the Actinia, and will at the same time become connected with the outer walls by a series of radiating lamellæ—the connate tentacle walls—separated from one another by radiating chambers, the cavities of the tentacles; while such portions of the tentacles of *Hydra* as still continue free will be represented by a single circle of the tentacles of *Actinia*.

The author had formerly compared the radiating canals of a hydroid medusa to the immersed portions of the tentacles of a *Hydra*, and he still maintains this view.

The strict parallelism of a siphonophore with a hydroid was pointed out, and each of the zooids which combine to form the heteromorphic siphonophorous colony was shown—as indeed Huxley and others had already done—to have its representative in the hydroid colony, and to be but a slightly modified form of some hydral zooid.

In order to understand the relations of a discophorous or steganophthalmic medusa to the other *hydrozoa*, he supposes the "atrium" of a hydroid medusa, or that part of the main body cavity which is still immersed in the solid proximal portion of the

umbella, at the base of the manubrium, to be expanded laterally, and the gelatinous extoderm of its floor to be projected along four or eight symmetrically disposed radiating lines into as many thick pillars, which converge towards the axis, and there meet the manubrium, while the thin intervening portions between the pillars become developed into generative pouches, the velum at the same time disappearing. A hydroid medusa would thus, in all essential points, become converted into a discophorous medusa.

A *Lucernaria* was conceived of by imagining a *Hydra* to have its tentacles reduced to four in number, and expanded laterally until their sides meet and coalesce; while the hypostome continues free, the solid hydrorhizal basis becoming at the same time extended into a peduncle of attachment traversed longitudinally by four canal-like prolongations of the body cavity, or else by a simple continuation of this cavity.

Lastly, a *Beroe* was taken as a type of the *Ctenophora*, and was conceived of as a hydroid medusa so modified as to become reduced to the atrial region alone. The two lateral canals which spring from the somatic cavity in *Beroe*, and subdivide so as to form ultimately the eight meridional canals, correspond to the greatly developed basal portion of the radiating canals of the medusa, or that portion of those canals which is still contained within the solid summit of the umbella; the affinities of the *Ctenophora* being thus directly with the *Hydrozoa* instead of the *Actinozoa*.

The author finds the key to the homology of *Beroe*, and the transition between the *Ctenophora* and the *Hydroida* in the singular ambulatory gonophore of *Clavatella*.

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Transactions of the Pathological Society. Vol. XXI. 8vo. —*From the Society.*

Transactions of the Royal Society. Vol. CLX. Part I., II. List of Members. 4to.—*From the Society.*

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Royal Society Catalogue of Scientific Papers. Vol. IV. 4to.—*From the Society.*

Quarterly Weather Report of the Meteorological Office. Parts 2, 3, 4. 1869. 4to.—*From the Meteorological Committee of the Royal Society.*

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Transactions of the Zoological Society. Vol. VII. Parts 3, 4, 5. 4to.—*From the Society.*

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Milan. Atte della Societá Italiana di Scienze Naturali. Vol. XII. Fasc. 4. Vol. XIII., Fasc. 1, 2, 3. Vol. XIV. Fasc. 1. 8vo.—*From the Society.*

Moscow.—Bulletin de la Société Impériale des Naturalistes. 1870. Nos. 1, 2. 8vo.—*From the Society.*

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Whitby.—Forty-eighth Report of the Literary and Philosophical Society, 1870. 8vo.—*From the Society.*

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. VII. | 1871–72. | No. 84.

EIGHTY-NINTH SESSION.

Monday, 27th November 1871.

SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Council were elected:—

President.
SIR ROBERT CHRISTISON, BART., M.D., D.C.L.

Honorary Vice-President.
HIS GRACE THE DUKE OF ARGYLL.

Vice-Presidents.

Professor KELLAND.
The Hon. Lord NEAVES.
Professor Sir WILLIAM THOMSON.
Principal Sir ALEX. GRANT, Bart.
Sir W. STIRLING-MAXWELL, Bart.
Professor W. J. MACQUORN RANKINE.

General Secretary—Dr JOHN HUTTON BALFOUR.

Secretaries to Ordinary Meetings.
Professor TAIT.
Professor TURNER.

Treasurer—DAVID SMITH, Esq.
Curator of Library and Museum—Dr MACLAGAN.

Councillors.

Professor GEIKIE.
Professor A. CRUM BROWN.
Rev. W. LINDSAY ALEXANDER.
Professor FLEEMING JENKIN.
Prof. WYVILLE THOMSON.
JAMES DONALDSON, Esq.
Dr THOMAS R. FRASER.
Dr ARTHUR GAMGEE.
ALEXANDER BUCHAN, Esq.
Prof. A. DICKSON.
D. MILNE HOME, Esq.
JAMES LESLIE, Esq., C.E.

Monday, 4th December 1871.

A Marble Bust of the late Sir Roderick I. Murchison, Bart., by Weekes, was presented.

Although the Bust was only placed in the Hall at this time, the offer of it to the Society was made by Sir Roderick I. Murchison in June 1871, in the following letter to the President :—

16 Belgrave Square, *26th June* 1871.

My dear Professor,—As it is very improbable, indeed—nay, almost a certainty—that I shall not be able to attend the meeting of the British Association at Edinburgh this year, I wish to send, as my representative, a marble bust of myself, executed by Mr Henry Weekes, R.A., and which is on the point of completion.

I beg to be informed if the Council of the Royal Society of Edinburgh, over which you preside, will accept this bust as a donation from myself, in gratitude for the great honour they conferred on me many years ago, by enrolling my name in their distinguished list of honorary members; also in recollection of another great honour which they conferred on me, by granting to me the first Brisbane gold medal for my labours in Scottish geology. If you assent to this proposal, I will direct Mr Weekes to transmit the bust to the Secretary of your Royal Society, in the hope that you will place it in the same building as the busts of our other scientific countrymen whom you have thus honoured.

I have also written to David Milne Home on this point, and have assured him, at the same time, that I will do everything in my power to support the memorial to the Government to assist the Royal Society of Edinburgh in carrying out their meritorious researches, as signed by yourself.—An early reply will oblige, yours sincerely,

RODERICK I. MURCHISON.

To Professor Christison,
President, R.S. Edin.

Sir Robert Christison, Bart., the President, read the following Opening Address:—

At the commencement of this, the 89th session of the Royal Society of Edinburgh, I beg to congratulate you on the successful issue of that which has just come to an end. The number of our members has increased, in consequence both of a low proportion of deaths among us, and likewise of an increase of new members beyond the average; so that, from 326 at the same period last year, the Society has grown to 331 at the present time.

We may appeal with equal, and even more, satisfaction to the success of our late meetings; which, in the first place, were carried on a full month longer than usual before exhausting the list of communications approved by your Council as worthy of being read before you; and which, in the second place, attracted from first to last unusual attendance and interest, on the part both of ourselves and of our visitors, by reason of the variety and value of the inquiries communicated at them.

Nor, amidst these grounds of direct gratification on account of the proceedings of last year in the Royal Society itself, will it appear out of place that I further congratulate you on the great success which attended the late meeting in Edinburgh of The British Association for the Advancement of Science. Whether we consider who was the founder of this most prosperous institution—or that the Royal Society of Edinburgh and the Association were established very much for the same objects—or that our Fellows have taken an active part in its proceedings, wheresoever it may have held its meetings—or that our endeavours contributed greatly to bring it on the recent occasion to our city—or that many of us did much, or at least as much as we could, to receive our eminent guests with the cordiality due to their distinction in science—we are equally entitled to rejoice that, in respect of the number of remarkable men who were attracted hither, the excellence of the matter produced before the several sections, the interest of the excursions which the unrivalled opportunities in our neighbourhood enabled us to offer, the oft-expressed obligations of our guests for the reception they met from us and our fellow-citizens, and, I

may add, the eight days of glorious weather, upon which in Scotland much of the comfort of so great an assemblage depends—this forty-first meeting of the British Association proved in truth to be a great success.

Although the deaths in the Society have not been numerous during last year, we have nevertheless to lament the loss of several of the most distinguished among our Fellows, both ordinary and honorary. From the list of ordinary Fellows we have to strike out the names, in alphabetical order, of Dr William Anderson, Mr Charles Babbage, Mr Robert Chambers, Dr Robert Daun, Mr Alexander Keith Johnston, Dr Sheridan Muspratt, Mr Robert Russell, Sir William Scott, Dr Fraser Thomson, and Mr Moses Steven. Our honorary list no longer bears the names of Sir John Herschell, Sir William Haidinger, and Sir Roderick Impey Murchison.

Mr Robert Russell, an eminent practical and scientific agriculturist in the county of Fife, was led to connect himself with the Society by his taste for meteorological pursuits.

Sir William Scott, Baronet, of Ancrum, an enterprising country gentleman, a soldier in his youth, and afterwards for some time member of Parliament for his county, was well known for his attachment to scientific society, and for the regularity of his attendance at our meetings at a period when his avocations allowed him to reside occasionally in Edinburgh.

Dr Robert Daun, Deputy Inspector-General of Army Hospitals, also a frequent attender at one time of the meetings of the Society, died in June last at a very great age [86]. He served his country with distinction in the medical service of the army throughout nearly the whole of the most momentous period, and the most critical trials, in the military history of our country. He was highly esteemed publicly for his knowledge in all departments of his profession, and his powers of organisation in his own branch of service; and he was no less prized by his friends for his acquaintance with various branches of science and literature.

Dr Fraser Thomson, son of the Rev. Dr William Thomson of Perth, and nephew of the late eminent clergyman of Edinburgh, Dr Andrew Thomson, the first minister of St George's parish, graduated at the University of Edinburgh, where he had been a distinguished student of medicine. He settled as a medical practitioner in his native city, and for most of his life was much engrossed by the cares of an extensive practice in town and country. But, like many of his profession in our county towns, he made natural history his recreation for his short leisure hours, and applied himself eagerly to microscopical research in that department of science. In this he acquired great expertness and accuracy, and would easily have become an original inquirer, were it not that his fondness for such pursuits had not fame for its object, but simply relief from the cares and fatigues of professional life. He died, after a short illness, in the month of October, in his 65th year.

James Sheridan Muspratt, a native of Dublin, was trained in the science to which he dedicated his life, under two of the greatest chemists of their day in Europe—Graham and Liebig. At the age of twenty-three he published the results of investigations carried on as a student in Liebig's laboratory on the sulphites, showing their analogy with the carbonates. Returning to Giessen three years later, he resumed his inquiries into the sulphur acids, the fruit of which was an interesting paper on the Hyposulphites, and also on Sulpho-cyanic Ether. In the interval he did good service to practical chemistry in this country by making generally known in a translation Plattner's standard work on the Blowpipe; and in 1854 he published a "Dictionary of Chemistry," which has been of great use in diffusing a knowledge of chemistry among those engaged in the practical working of chemical problems. Mr Muspratt died in the 47th year of his age.

Mr Robert Chambers, long one of the most attached and working Fellows of the Royal Society, is one of the many instances, observed at all times in Scotland, of men raising themselves in a short time, by the sheer unaided gifts of native talent and indomitable perseverance, from an obscure position in society to a promi-

nent place in public estimation. Born, as we are told by one of his biographers, who evidently knew him and his history well, of parents respectable, but not fortunate in life, he had to struggle in his early years with difficulties. Nevertheless he was not prevented from reaping the inestimable advantages which in Edinburgh a parent of even moderate means could always command, for a son of promising parts, from an education at the High School.

Like other prolific writers, Mr Chambers began the career of authorship at a very early age. He must have been not above eighteen, when, having not long before chosen for his occupation in life that of bookseller, he determined to be publisher and author too, projecting and conducting a periodical called the "Kaleidoscope," to which he himself also contributed articles from his own pen. Soon afterwards he published "Illustrations of the Author of Waverley;" and in 1823, when only twenty years old, he added the work by which he has been longest and most familiarly known as a writer, his "Traditions of Edinburgh." Work upon work then followed in quick succession on all sorts of literary subjects, but chiefly historical and antiquarian—works which it would be out of place even to enumerate in so short a sketch as that to which this brief notice must be confined.

At last, in conjunction with his elder brother, Mr William Chambers, was begun in 1832 the now famous "Chambers' Edinburgh Journal,"—the first idea, and as such a great invention, of a weekly periodical devoted to short productions, original, as well as critical, on nearly all literary and also some scientific subjects, suited for the information, as well as for the purse, not alone of the educated classes ordinarily so called, but likewise for the educated in the humbler walks of life. This undertaking met soon with extraordinary success—in so much, indeed, that it became the parent of many others identical or similar in their aims, and not a few of them not less prosperous than that of the two brothers Chambers.

While adhering steadily to his literary tastes, and giving forth in various works the results of his literary labours, Mr R. Chambers' attention was turned to a totally different object of study, which in all probability he first followed as a diversion, or distraction

from the severity of professional toil. This was geology, which in the end captivated him, and first made him an active, energetic member of this Society. Cultivating his new pursuit with his inherent fervour unabated, he soon became an original inquirer in this fascinating branch of natural science. Besides making himself acquainted with the rock structure of many parts of his own country, he visited as a geologist Switzerland, Norway, Sweden, Iceland, the Faroe Islands, and parts of Canada and the United States. Few geological amateurs, engaged in a profession usually so engrossing as that of Robert Chambers, have acquired such intimate knowledge of geology. Many of us can recall the interest of his discussion of geological questions at our ordinary meetings; and his "Ancient Sea Margins" will long be known as one of the earliest, most exact, and most lively descriptions of that particular branch of his favourite study.

Mr Chambers was distinguished, alike in his public appearances, as in social intercourse, by a great fund of information on most diversified topics of interest in literature and science, by his caution and politeness in criticism, and by his courteous kindliness in every relation of life. In the last respect he will be long missed by a numerous circle of attached friends, many of whom were his fellow-members of the Royal Society of Edinburgh. In March 1871, after a tedious and enfeebling illness, borne with singular patience, he died in the 69th year of his age.

I turn next to another no less serious loss sustained during the past year by science and this Society in the death of Mr Alexander Keith Johnston. Mr Keith Johnston at first intended to join the medical profession; but, at an early age, he betook himself to the art of engraving, which again led him to the study of geography; and from that time geography became his ruling pursuit, and the object of his professional life.

In 1830, having had occasion, during a pedestrian trip in the Highlands, to remark the inaccuracy of the maps of Scotland, he published an improved collection in a Guide Book. At the same time, to facilitate the development of his geographical enterprises, he joined the firm of his two brothers, Sir William and Thomas Johnston, which had been established in this city some years

before for carrying on the business of engraving and printing, in which they have been long famous among the skilful engravers of Edinburgh. In his thirty-ninth year he attracted the regard of scientific geographers at large by the publication of his "National Atlas," and still more, five years later, by his "Atlas of Physical Geography." For the task he had thus set himself he had been thoroughly prepared by assiduous study of the best works in the various languages of Europe, by frequent visits to many European countries, and by acquaintance and personal intercourse with the greatest continental geographers and travellers. Not long afterwards Mr Keith Johnston brought out in succession a "Dictionary of Geography," a "Military Atlas" for Alison's "History of Europe," the "Royal Atlas of Modern Geography," and subsequently a variety of cheap atlases for the use of schools. By these productions he raised himself to a position in which he had no superior rival as a geographer in this country; and his merit in this respect received the stamp of the Royal Geographical Society of London in the last year of his life by the award of the Geographical Victoria Medal.

But Mr Johnston took also great interest in almost every branch of physical research, with many of which he had no mean acquaintance, and whose cultivation in this city he seized every opportunity to encourage and promote. Among other obligations to him, we are greatly indebted for the foundation of "The Meteorological Society" of Scotland,—an institution which, under the able direction of its present Secretary, promises important results, certain, indeed, to be realised if the Society receive due public support in the line of inquiry in which it has already been for some years successfully engaged. It is also known to me that the city and University are mainly indebted to him for the early foundation of the Chair of Geology, through the munificence of his friend the late Sir Roderick Murchison. At the direct instance of Mr Johnston, and through the weight which his genuine love of science commanded with many men of influence, Sir Roderick was induced to alter his intentions, from a "post-obit" foundation, to an immediate gift, of the Chair, in conjunction with a Royal Foundation and additional endowment.

In such proceedings as these Mr Johnston did good with no

ulterior view, and from no love of being what our neighbours across the channel aptly call a "grand faiseur." Hence we scarcely know how much we owe to him. His extensive acquaintance with the upper ranks of what it has become the custom to call the "citizen class" in Edinburgh, enabled him often quietly to direct public opinion in the nice exercise of scientific, literary, and professional patronage, when sound direction was greatly needed; and his acknowledged prudence, probity, impartiality, and knowledge of men, never failed to guide himself soundly in such conjunctures.

Throughout his whole life he was faithful and fruitful in his calling, and no less a sincere and active Christian. Seldom has there been a more affable, agreeable, and profitable companion in social life in all its phases.

Although far from being a young man at his death,—for he died in his 67th year,—we have to lament that he was struck down while in full possession of his powerful intellect, and enjoying shortly before a vigour which promised long continuance of his useful labours.

Wilhelm Ritter von Haidinger, one of our Honorary Fellows, was a favourite pupil of Mohs; who, during great part of the first half of this century, was celebrated as one of the foremost mineralogists of his day in Europe, and as the able Professor of Mineralogy in the University of Vienna. While yet a young man, William Haidinger possessed an extraordinary extent and accuracy of knowledge of minerals. On account of his talents as a descriptive mineralogist, he came to Edinburgh, about the year 1824, to arrange and catalogue the splendid mineralogical collection of a former curator of our Society, Mr Thomas Allan, banker in this city,—a collection unrivalled, for extent and careful costly selection, among the private mineralogical museums of Europe. In discharging this duty Mr Haidinger was enabled to establish several species as new to science; which he investigated and communicated to our meetings in conjunction with the late Edward Turner, the chemist, at the time lecturer here, and soon afterwards first Professor of Chemistry in University College, London. Haidinger took the descriptive, Turner the analytical, part of these inquiries; and, in both respects, their papers are models of

mineralogical investigation. I was at this time intimately acquainted with Haidinger, and could well appreciate his mineralogical facility and acuteness, his varied knowledge of natural history and physical science, and his remarkable command of languages,—so that, for example, in our own tongue, he could tell a jocular story, make a pun, and extemporise a clever couplet,—which I take to be about the severest of all tests of a man's familiarity with a foreign language.

No one who knew him at that time could fail to see that Haidinger would one day become a man of mark among the mineralogists of his own land, to which he returned soon after completing his labours in Mr Allan's museum. He then travelled for some time with Mr Allan's son, Robert, who died a few years ago a Fellow of this Society; and the main object of the travellers was the pursuit of mineralogy. Ere long Mohs died, and Haidinger succeeded him in his University Chair. His office put him naturally at the head of all relative Government undertakings, which in their turn brought him promotion, till at length he filled the highest office in his profession, that of Director of the Mineralogical and Geological Survey of Austria. For his many scientific and practical services to his country he received from his sovereign the honour of knighthood a few years before his death, which took place last April in, as I understand, the 71st year of his age.

Coming nearer home, I have next to deal with the scientific life of another lost Honorary Fellow of the highest rank in Physical Philosophy, Sir Roderick Impey Murchison, Baronet. But though very willing, and not altogether unable, to do justice to his remarkable labours in his science, I felt that I should be acting with injustice to his memory, and to the claims of a far superior biographer and eulogist, if I did not transfer from myself to Professor Geikie the pleasing task of recalling to our recollection the main points in the life and the work of his patron and friend. The following summary is accordingly the tribute which Professor Geikie has kindly enabled the Society to pay to the fame of Sir Roderick Murchison :—

"Among our recent losses there is none which we have more reason to deplore than his. The name of Sir Roderick Murchison

has been a household word in geology for nearly half a century, not in Britain only, but also over all the world. While we share in the wide regret at the injury which the general cause of science sustained by his removal, we add also the sadness which arises from the recollection of the relation which he bore to the progress of geology in Scotland, and from what he has recently done for the advancement of its study in the University of this city.

"Born in 1792 at Tavadale, in Ross-shire, he was educated for the military profession, and served during part of the Peninsular War. But on the arrival of peace in 1815, finding that the army no longer opened up the same prospect of activity for which he longed, he gave up his commission, married, and settled in England. The succeeding part of his life, prior to 1824, he used to speak of as his "Fox-hunting period," when he threw himself with all the ardour of his nature into the field sports of a country residence. Part of that period, however, he spent abroad, making, with his wife, tours in search of picture galleries and old art, and keeping an elaborate diary, with criticisms on the character of the fine arts in each tour or collection visited. It was by a kind of happy accident that his energies were at last directed into the channel of science,—the merit of which change was due partly to his wife's taste for natural history, and partly to the friendly counsel of Sir Humphrey Davy. He joined the Geological Society of London, and soon became one of its most enthusiastic members. From that time forward his love for geology, and his activity in its pursuit, never waned. He travelled over every part of Britain, and year after year he resorted to the Continent, traversing it in detail from the Alps to Scandinavia, and from the coasts of France to the far bounds of the Ural Mountains. As the result of these journeys, there came from his pen more than a hundred memoirs, besides two separate and classical works on 'The Silurian System,' and on 'Russia.'

"Sir Roderick was essentially a geologist, and he chose one special branch as his own domain. Perhaps no man ever had the same power,—which seemed sometimes almost an intuition,—of seizing the dominant features of the geographical and palæontological details of a district. With a keen eye to detect the characters as they rose before him, and a faculty of rapidly appreciating their

significance, he could, as it were, read off the geology of a country after a few traverses only, when most men would have been puzzling over their first section. This was the secret of his broad generalisations regarding the geological structure of a large part of Europe,—generalisations which, though of course requiring to be corrected and modified by subsequent more detailed investigations, still remain true in the main, and still astound by their marvellous grasp and suggestiveness. The leading idea of his scientific life was to establish the order of succession among rocks, and through that order to show the successive stages in the history of life on our globe. With the more speculative parts of geology he meddled little; nor did he ever travel outside the bounds of his own science. He early recognised the limits within which his powers could find the fullest and most free development, and he was seldom found making even a short excursion beyond them.

"The special part of his work on which his chief title to fame rests is undoubtedly his establishment of 'The Silurian System.' Before his time, the early chapters of the history of life on our globe had been but dimly deciphered. William Smith had thrown a new flood of light upon that history by showing the order of succession among the secondary rocks of England, and had done more than any other man to dispel the prejudices with which the doctrines of Werner seemed naturally to fill the mind. But the rocks older than secondary, to which Werner had given the name of 'Transition,' remained still in deep Wernerian darkness. Sir Roderick Murchison saw that it might be possible to bring order and light out of these rocks, even as had been done with those of more recent origin; and that a double interest would attach to them if, as he supposed, they should reveal to us the first beginnings of life upon our globe. Choosing a part of the broken land of England where the rocks are well exposed, he set himself to unravel their order of succession. Patiently year after year he laboured at his self-appointed task, communicating his results sometimes in writing to his friends, sometimes in the form of a short paper to the Geological Society of London, until at last, in 1838, he gathered up the whole into his great work, 'The Silurian System.' In that book the early chapters of the history of life on the earth were first unfolded, and a system of classification was

chosen with such skill that it has been found applicable, with minor modifications, even in the most distant quarters of the globe.

"Round this early work all his after-labours seemed to range themselves by a natural sequence. His choice had led him into the most ancient fossiliferous rocks, and to that first love he remained true. Whether in the glades of Shropshire, or the glens of his own Highlands, among the fjelds and fjords of Norway, or in the wilds of the Urals, it was with the Palæozoic formations that he mainly busied himself. They were to him a kind of patrimony which had claims on his constant supervision. With his friend Sedgwick he unravelled the structure of the middle Palæozoic rocks of Devonshire, and with Keyserling and De Verneuil he showed the true relations of the upper Palæozoic rocks of Russia. The Silurian, Devonian, and Permian systems, representing each a vast cycle in the history of our earth as a habitable globe, received in this way from him their first clear elucidation, and the very names by which they are now universally known.

"But if we seek to measure the influence which Sir Roderick Murchison exercised on the progress of the science of the time merely by the original work which he himself accomplished, we should fail duly to appreciate the measure and the power of that influence, and the extent of the loss which his death has caused. Fortunate in the possession of wealth and high social position, he was enabled to act as a constant friend and guardian to the cause of science. He moved about as one of the representative scientific men of his day. To no man more than to him do we owe the public recognition of the claims of scientific culture in this country. For he not only stood out as the acknowledged chief in his own domain, but had also the faculty of gathering round him men of all sciences, among whom his kindliness of nature, his courteous dignity of manners, his tact and knowledge of the world, and his wide range of social connections marked him out as spokesman and leader. Nowhere were these features of his character and influence more conspicuous than in his conduct of the affairs of the Geographical Society, of which he was for many years the very life and soul, and which owes in large measure to him the stimulus it has given to geographical science.

"Here in his own native country, and more especially here in

Edinburgh, we have peculiar cause to mourn the loss of such a man. Though his residence from boyhood had been chiefly in London, he never to the last relinquished his enthusiastic regard for the land of his birth. He never lost an opportunity of boasting that he was a Scot. During the last ten years of his life he made frequent and protracted tours in the Highlands; and, in unravelling their complicated geological structure, he accomplished one of the most brilliant generalisations of his long and illustrious scientific career. There is something touching in the reflection that, after having travelled and toiled all over Europe, gaining the highest position and rewards which a scientific man can attain, he should at last, ripe in years and in honours, have come back to his own Highlands, and there completed his life-work by bringing into order the chaos of the primary rocks, and laying such an impress on Scottish geology as had never been laid before by any single observer. For these and other researches he received from this Society the first Brisbane Medal—an honour conferred on him at the Aberdeen meeting of the British Association, and of which he often spoke as one that gave him the deepest gratification. He used to boast, too, of being an honorary Fellow of this Society, and to quote a remark made to him by the late Robert Brown, that his election into the list of our honorary Fellows was one of the highest marks of distinction he could receive. His kindly interest in our prosperity was often expressed; and we have a token of it in the presentation to us of his bust by Weekes, which this evening is formally delivered to the Society.

"Of the closing acts of his life, there is one which cannot be mentioned without peculiar pride—the institution of a Chair of Geology and Mineralogy in the University of Edinburgh. He intended to found this Chair by bequest; but on the retirement of Dr Allman from the Chair of Natural History, he determined to do in his lifetime what would otherwise have been accomplished not till after his death. He gave to the University a sum of £6000; and the Crown having consented to add an annual grant of £200, the Chair was founded in the spring of the present year. Sir Roderick has not lived to witness the first beginnings of the tuition which he had started. But long after the memory of his personal character shall fade, men will remember the work which

he did; they will recognise the impetus his researches have given to geology all over the world; and let us hope also they will see in the Chair he has founded the starting-point of a new and active school of Scottish geology."

I have left to the last in this biographical sketch of our lately deceased Fellows two of the most eminent men of British science in their day—Herschel and Babbage. For as I could not pretend to do justice to the lives of men whose pursuits, in the highest range of physical science, were so far removed from my own, I think it right to keep quite apart the following eulogium, the preparation of which my university colleague, Professor Tait, has kindly allowed me to impose on him, and which I will give in his own words:—

"Of Sir John F. W. Herschel and Charles Babbage, who may be fitly mentioned together, it is not necessary that much should be said, as their contributions to science cannot fail to be set forth at length in the Proceedings of other Societies, with which they were more connected than with our own. Intimate friends during their undergraduate career at Cambridge, they joined us as ordinary Fellows shortly after taking their degrees, and when they were just commencing, along with the late Dean Peacock, what all must consider, in spite of their other grand contributions to science, the greatest work of their lives—the restoration of mathematical science in Britain. It is impossible even now to overestimate the value of this service. Few know to what a state of ignorance we had fallen at the time when Lagrange, Laplace, Fourier, Cauchy, Poisson, and Gauss, and many others abroad, were advancing with breathless rapidity in the track, neglected by us, of James Bernoulli and Euler. Partly from a mistaken notion that they were honouring Newton by adhering to his published methods, partly owing to the British dislike to men and things foreign, which at this time was pushed, perhaps not unnaturally, to extreme lengths in all matters, and partly in consequence of our long state of war with France, our mathematicians had never even learned those unpublished methods by which Newton made his discoveries, which, as soon as they were to some extent divined

abroad, were at once estimated at their true value, and pursued with zeal and genius.*

"Little by little, first by translating Lacroix's elementary treatise on the differential and integral calculus, and by thus introducing, in face of determined opposition, the notation of differential coefficients into Cambridge, so as for the first time to enable her mathematicians to understand a foreign treatise; secondly, by publishing an excellent collection of examples; and thirdly, by their separate original treatises on different special parts of analysis, they put this country on a level with France and Germany, so far at least as opportunities of progress are concerned. It is to them mainly that we owe, not merely our modern British school of mathematicians, which is now certainly second to none in the world, but even the very possibility of the existence in this country of such great departed masters as Boole and Hamilton.

"Herschel's 'Treatise on Finite Differences,' which appeared as a supplement to the translation of Lacroix, is one of the most charming mathematical works ever written, everywhere showing

* Professor Tait has urged me to make known a reminiscence of my youth that at the time here referred to there were in Edinburgh, and in this Society, no fewer than three mathematical amateurs, who, though they never made themselves publicly felt as such, in some measure saved this corner of the land from the censure dealt in the text. These were Sir William Miller, Baronet, of Glenlee, better known as Lord Glenlee of the Scottish bench; William Archibald Cadell, of the family of Cadell of Grange, who finished his earthly career but a few years ago; and my own father, Professor of Latin in our University. Lord Glenlee, a man of very retiring habits and disposition, was usually called the first amateur mathematician in Scotland. Mr Cadell, also a man of great reserve and shyness, nevertheless, in order to carry out his admiration of the modern continental mathematics, contrived to obtain, during the very hottest of our struggles with France, from that generally unyielding potentate, the First Napoleon, permission, through the influence of one of the great mathematicians of Paris, to repair to the French capital, to dwell there for seven years, and to return unhindered to Scotland, at a period when no other Briton was known to have put his foot on French soil without being made a *detenu*. My father, during the last ten years of his life, which ended in 1820, betook himself, as his idea of relaxation from routine professional life, to the differential calculus, and to Newton, Bernoulli, Euler, Lagrange, Laplace, Lacroix, &c., whose works were always at hand when not in his hands. As he made a vigorous attempt to indoctrinate me at a very early age in his favourite pursuits, I know well what these were, and what he knew of the kindred spirits Glenlee and Cadell.

power and originality, as well as elegance. In all these respects it far surpasses his subsequent mathematical writings, excellent as are many of them; for instance his celebrated treatises on 'Light' and on 'Sound' in the 'Encyclopædia Metropolitana.' The appendix to Lacroix which was written by Babbage, was devoted to the 'calculus of functions,' a strangely weird branch of analysis, which remains even now much as Babbage left it. That in this direction there is a splendid field open for the inquirer, is evident to any one who consults Babbage's papers on it; and it is wonderful that it has not been greatly developed of late years, when so many mathematicians, especially at home, have been found to apply themselves almost exclusively to those branches of the science which seem the least likely ever to have useful applications.

"In their after-life the careers of these great workers and thinkers led them widely apart. Herschel devoted himself mainly to astronomy, but also to chemistry, photography, and occasionally to mathematics. His astronomical work is all of the very highest class, whether it consisted in his seclusion, for several of the best years of his life, at the Cape of Good Hope in the close observation of the stars and nebulæ of the Southern Hemisphere; or in first writing, and then, as edition after edition was called for, extending and improving his splendid semi-popular work, the 'Outlines of Astronomy,' which none, even of men of science, can read without deriving from it at once pleasure and profit.

"Babbage, on the other hand, applied himself mainly to machinery and manufactures. His so-called 'Ninth Bridgewater Treatise' was pre-eminent even among the best of that singular series; his 'Economy of Machines and Manufactures' is still a wonderfully suggestive work; and his 'Mechanical Notation' supplies us with an insight into the kinematics of all possible combinations of machinery, which none can have any conception of without making it a special subject of study. He was led to its invention by his celebrated attempts to achieve the construction of a difference-engine, and even of an analytical engine—machines totally unintelligible, in their conception, to the majority even of those who are capable of understanding the nature of the work for which they were designed. Enough was constructed, though it was a very small part, of the first of these engines to show not only that

the device was completely successful, but also to exhibit the extraordinary talent of the inventor in such a light as to convince scientific men that in his hands the astounding problem of constructing the second was capable of solution. A paltry economy of the Treasury prevented the completion of the first engine, and made it obvious to Babbage that there was no hope of assistance from Government to construct the second. Yet it has been allowed by the best authorities that the money spent on the finished portion of the difference-engine was far more than repaid to the country by the extraordinary improvement in tools of every kind, which was required for the new engine, and was at once supplied by the fertile, inventive brain of Babbage as the work proceeded.

"No one can read the obviously true story of this miserable affair, as it appears in the strange autobiography of Babbage—his 'Passages from the Life of a Philosopher'—without a blush for the short-sightedness of British rulers. Had Babbage been a Frenchman or Russian, had he even belonged to the then poor kingdom of Prussia, do we not all feel assured that these grand conceptions of his would long ere now have been realised as powerful agents in the working world, instead of lying dormant, in mouldering, worm-eaten plans and sections.

"Strange the contrast between the careers of these early friends! They began, indeed, by a grand joint success, for which alone their memory will always be justly cherished. But while the one, encouraged, yet never unduly elated, by success, steadily at work, though not of late years brilliantly, ended a long and happy life, every day of which had added its share to his scientific services; the other, enraged by the petty persecutions of men unable to understand scientific merit, or even its mere pecuniary value, spending lavishly from his private fortune to be enabled to leave to some possibly enlightened posterity a complete record of the working details for the construction of his splendid inventions, was never understood by his countrymen.

"But so it has ever been in this country. Herschel's father was a German; so of course we could appreciate him. Babbage was an Englishman; the only person who took the trouble to understand his invention was a foreigner, the skilful mathematician Menabrea, ex-minister of Victor Emmanuel."

Observations on the Fresh Waters of Scotland.

Looking around me for some general theme suitable for the subject of this introductory address, I became oppressed with the persuasion, that no such subject, worthy of your acceptance, had been left unexhausted by the able men who have lately had to treat of scientific topics of a general nature in circumstances akin to my own on the present occasion. I therefore thought I might trust to your indulgence, and substitute for a general address a notice of some inquiries, which have been carried on from time to time during my late occasional autumn holidays, and which promise results of some interest, illustrating the hydrography of the fresh waters of Scotland. These inquiries have in several respects been pushed not so far as to satisfy me completely. But as I may not be able to carry them through according to my present design, and I hope that others may be led to interest themselves in also pursuing them, I beg to submit the results to the Society, such as they are.

The topics I propose now to bring forward,—which are rather diverse in nature, yet not altogether unconnected with one another, —are three in number,—*First,* The composition of the water of certain lakes and their leading streams in Scotland, and the changes their waters undergo in the streams which the lakes feed; *Secondly,* The temperature of these lakes at various depths; and, *Thirdly,* The action of their waters upon lead.

I shall commence by recalling shortly the geological structure of our country, by which in a great measure the nature of its waters is regulated.

In the primitive formations which constitute the "Scottish Highlands" of ordinary speech,—for in correct language many parts of the so-called "Lowlands" are as well entitled to the other name,—we find that the mountain summits are either pointed or rounded, but seldom table-topped; that their spurs are commonly rather sharply ridged; that their surface abounds in precipices, crags, loose blocks, rocks, and stones; and that the valleys between them, except in the course of our largest rivers, are narrow, gravelly, or rocky, thinly covered with vegetative soil, and consequently little fit for plough cultivation. Not infrequently, however, the spurs or buttresses, instead of being ridgy, are broad and flat,

smoothly covered with fine heather, the favourite breeding-place for grouse, and tolerably dry, except where small patches of peaty bog show themselves here and there. This structure is often well exemplified among the mountains of Glen-Shee. Again, when the spurs of a mountain are ridgy, the ridges are sometimes separated from one another by an upland valley, often very grassy, especially towards its head or "corrie," but likewise apt in many places to be boggy, and there abounding in peat, and in denuding cuts which expose the peat to atmospheric influences. Good examples of such upland valleys are to be seen on the Cobbler, and on its higher northern neighbour Ben-Arnen, where they face Arrochar eastward, and also on Ben-Lomond northward from its peak. Exposed peat constitutes on the whole no great proportion of the surface of most mountains in the Highlands.

It follows from this structure, that in most districts of the Highlands rain and melted snow find little to dissolve in descending the mountain sides; and their steepness causes the streams to tarry a very short time in their descent, and to drain off quickly the excess of water in flood-time. All these circumstances combine to render the streams and lakes of the Highlands uncommonly pure in dry weather, and not materially less so even in heavy floods. Among the granite ranges, such as in the Goat-Fell district of Arran, the streams, such as the Rosa and Sannox, are beautifully clear and colourless in the highest floods. The temporary water-falls which then streak the mountain slopes, present to the eye the purest whiteness; and on filling a glass tumbler from a stream, the water, after the instant subsidence of a few coarse particles of granite sand, is seen to be perfectly transparent and free from colour. In the mica-slate districts of the near Grampians the streams are equally pure in dry weather. But after rains they are visibly brownish, yet so slightly that in a common water-bottle on a dinner-table the colour may readily escape notice.

During last autumn I had frequent opportunities of examining, in various circumstances, the water of one of these mica-slate streamlets, which is used for supplying a villa near Loch-Goil-head. The stream descends the steep eastern slope of "The Cruach," a hill which land-locks the upper part of Loch Goil on its west shore at a point about a mile and a half from the Head. Although only

2000 feet high, "The Cruach" presents an imposing, rugged, conical sky-line to one entering Loch Goil from Loch Long. The east face, precipitous at the summit, is entirely grassy lower down, unless where broken by other precipices, out-cropping rocks, or stream-courses, also always rocky. There is little peat to be seen anywhere, and no agriculture. From various trials around Loch Goil and Loch Lomond I am satisfied that this streamlet is a fair type, both in its ordinary state and in its occasional variations, of most of the streams which tumble into these sheets of water from the mica-slate mountains around them.

When I examined this water in the end of September, after ten days of perfectly dry weather, following a heavy twelve-hours' rain two days earlier, it was beautifully clear and sparkling. In the first place, it was entirely free from colour. The absence of colour was tested conveniently and delicately by means of a glass tube 16 inches long and six-tenths of an inch in diameter, which is nearly filled with the water to be examined, and is held over, but not touching, a sheet of white paper in a bright light. For security, a very fine colourless spring water was always kept at hand for comparison in another tube. The slightest coloration is thus seen by looking perpendicularly down the tube. Or it may be equally recognised by looking at the surface of the water obliquely through the upper part of the tube from a distance of 18 inches or 2 feet; for the colour is thrown up by the paper, and concentrated, as it were, on the surface of the water, though the long subjacent column, as seen through the glass, appears colourless. Very few waters, except that of springs, withstand altogether this test of the presence of colour.* Mr Dewar has suggested that it admits of being made a water-chromometer, by employing for comparison,—distilled water being used for fixing the zero point,—a solution of some invariable strength of a permanent per-oxide salt of iron, such as the acetate, and diluting the solution to uniformity of depth of colour with the water to be compared. The amount of dilution would denote the degree of coloration relatively to a fixed standard.

In the second place, this water contained a very small propor-

* This method, devised for the occasion, I have since found to be a mere variety, but more convenient, of one proposed some years ago by Dr Letheby, and adopted by the late Professor Miller.

tion of saline matter. In by far the greater number of streams and lakes in Scotland, whether Highland or Lowland, the salts met with are the same, viz., carbonates and sulphates of the three bases, lime, magnesia, and soda, and the chloride of their metalloids, calcium, magnesium, and sodium. Of these the chlorides are usually most abundant, the sulphates least so; and of the bases, lime is commonly predominant, magnesia the contrary. But frequently in the Highland streams the proportion of all is so small that most of the ordinary liquid tests scarcely affect them. In the water now under consideration, for example, magnesia, among the bases, was not indicated by the alkaline phosphate of ammonia; nor was sulphuric acid, among the acids, by nitrate of baryta; even lime was doubtfully indicated by oxalate of ammonia; chlorine, too, was scarcely indicated by nitrate of silver in a small test-glass, and required a quantity amounting to six or seven ounces to yield an undoubted faint mist; and permanganate of potash did not denote organic matter except faintly. Acetate of lead, however, by acting on both combined carbonic acid and organic matter, showed a haze even in a small quantity of the water; and so did tincture of potash-soap, by virtue of the decomposing influence on it of earthy carbonates and free carbonic acid together.

After frequent trials I am inclined to think, that for practical purposes, when organic matter does not require to be taken into account, we seldom need any other test for ascertaining the relative purity and usefulness of these waters than the late Professor Clark's soap-test. In the present instance this denoted in several trials only 1·04 degrees of hardness, which is equivalent to that much of carbonate of lime in an imperial gallon of 70,000 grains of water. From frequent observation of the effects of this and other liquid tests, I feel assured that the total solid contents could not have been more than a 25,000th of the water, and was probably nearer a 30,000th.

In the third place, this composition, viz., little saline and extremely little organic matter, would lead to the expectation that the water will corrode lead. And so it does, but not powerfully. A thin plate of lead, with 4½ square inches of surface, weighing 437 raine, was suspended by a lead rod in this water. In twenty-eight

days it lost only 0·42 grain in weight, and crystals of carbonate of lead were deposited scantily. In circumstances exactly the same, distilled water will form carbonate of lead in abundance, and the loss of lead is 3·4 grains, or eight times as much.

In times of flood the condition of the water in such streamlets necessarily undergoes change. But the difference is not so great as might naturally be expected. In the night of 19th September last and subsequent morning rain fell steadily at Loch Goil, and heavily for twelve hours; and, consequently, in the forenoon of the 20th the streamlet described above was considerably flooded. The water, seen in bulk, was somewhat brownish; it was even faintly brownish in a dining-room water-bottle; and in a 16-inch glass tube it appeared yellowish. Nevertheless, it looked well enough in a glass tumbler, and it was not in the slightest degree turbid. Its purity, apart from its colour, was very great. No liquid test for inorganic salts but one,—not oxalate of ammonia, not nitrate of silver, not even acetate of lead, had any visible effect. The soap-test alone exerted any manifest action; and this indicated only 0·8 degrees of hardness, which is equivalent to little more than an 80,000th of carbonate of lime in the water. In correspondence with this condition, lead underwent rapid corrosion in it. A plate, an inch and a half square, lost in twenty-eight days 3·09 grains in weight, or about $\frac{9}{10}$ths of the loss in distilled water in the same time; and crystals of carbonate of lead were formed in abundance.

I examined the same stream on a previous occasion after a furious tempest and rain-flood on the 24th August last. Much rain had fallen at Loch Goil previously for several days. But on the 24th it fell in torrents, and for half-an-hour that forenoon like a tropical deluge. During this period a great extent of grassy turf was torn off in the upper part of the stream, probably by a water-spout. In a few minutes the streamlet, already in high flood, became a muddy tumultuous torrent in which no man could have stood or lived; swiftly its muddy waters spread out over the salt water of Loch Goil; and then meeting similar floods first at its own side, and afterwards from the opposite shore, the united muddy torrents covered the whole upper reach of the loch in less than half-an-hour to the extent of two miles in length, and three-quarters

of a mile in average breadth. A rainy day followed, and then four days of uninterrupted dry weather, during which the stream returned nearly to the same state in volume and appearance as after the moderate flood already described. There was this difference, however, even in its composition; nitrate of silver feebly indicated chlorides, and acetate of lead also feebly indicated carbonates. The difference wss probably owing to a material difference in the direction and force of the wind. On the former occasion the wind blew from the north-east, with no great violence, over about 90 miles of land; but on the latter occasion it blew with fury from west to south-west over Loch Fyne at distances varying from 18 to 15 miles only. In the latter case sea-spray must have been swept up into the air and carried far by the storm. In the former less would be raised into the atmosphere, and much would be deposited again in passing over 90 miles of land. In 1845 I found chlorides distinctly indicated by a white cloudiness, when nitrate of silver was added to rain-water collected on the top of Goat-Fell in Arran, towards the close of a violent four days' south-westerly gale, attended with frequent heavy rain, the sea in the direction of the wind being 12 miles distant, and 2800 feet below.

The facts now stated, which I have often corroborated by less minute observation of other streams in the mica-slate district of Loch Long, Loch Goil, and Loch Lomond, will convey some idea of the constitution of these waters in three conditions, viz., after high floods, moderate floods, and dry weather. To complete the series, it is an object of interest to add their condition after very prolonged drought. In that case the streamlets, except those fed by small upland "tarns," will come at last to convey only the water proceeding from springs; and many not so supplied will dry up altogether. For the composition of those which continue to run we may look to the springs themselves which feed them, because in their then very low state, running chiefly over rocks and stones, their waters will contract little additional impregnation in their course downwards. I have examined several springs in the mica-slate district under consideration. They have generally presented rather more saline constituents than the streams in their ordinary state, and invariably no colour appreciable by any of the ocular

tests I have used as described above. Sometimes their salts are scanty; but always they are quite colourless. Their solids appear to vary from a 16,000th to a 21,000th; and chlorides and lime-salts are, for the most part, indicated by their proper liquid tests rather more distinctly than in the general run of stream waters in their ordinary state of fulness. Several small springs high on the hill slopes have yielded these results. Similar in that respect is a copious spring in Glen Beg, more familiarly known by the name of Hell's Glen, about three miles from Loch-Goil-head in the narrow pass to St Catherine's on Loch Fyne. This spring, which gushes in force near the highway and close to the valley stream, is at all times beautifully limpid, and seems to be little affected in volume by droughts or floods. Its temperature is 44° when the air is 64° and more, though its site is not much over 300 feet above the sea-level. Its water is perfectly colourless, but contains rather more chlorides and earthy salts than the waters of the streams in their ordinary condition. Another more remarkable spring of great volume issues from the south flank of the Cobbler, about 1500 feet perpendicular above the bottom of Glen Croe, and leaping from rock to rock, joins the Croe about half-way up the glen. In the very dry season of 1870, its course was the only one which showed any water among the many which score the steep slope of the mountain where it overlooks the glen from the north. I found the water last autumn, after ten days of complete drought, to be perfectly colourless, and to be so free from saline matter as to be barely affected even by the delicate liquid tests for chlorine and for lime.

As the various streams now described are the feeders of the fresh-water lakes, which abound in the mica-slate districts, the composition of the water of the lakes must be the same with that of the average water of the streams. The small upland "tarns" are peaty, owing to the peat which paves and surrounds them. But the great low-lying lakes present very little solid matter of any kind in their waters; their scanty salts consist of chlorides, carbonates, and sulphates, the bases being lime, soda, and magnesia; and the organic colouring matter is so small as to be discoverable by delicate tests only. In all instances, however, our purest lake

waters in a mica-slate country are slightly — very slightly coloured.

The water of Loch Katrine is a well-known and characteristic example. Some years before the proposal was first entertained to use it for supplying Glasgow, I found it to contain only a 40,000th of solids. When compared with a fine spring water, however, it now presents in a 16-inch glass tube an appreciable, yet very faint, yellowness. In hardness it indicates only 0·65 by the soap-test, or the equivalent of a 108,000th of carbonate of lime. In correspondence with this great purity it acts powerfully on lead. In three weeks, a lead plate one inch and a half square, lost 2·53 grains in weight, which is exactly the loss sustained in distilled water in the same time; and crystals of carbonate of lead were formed in profusion.

The water of Loch Lomond is a less familiar instance of the same kind.

Loch Lomond is twenty miles long, and at its southern or outlet end, rather more than four miles and a half wide. Its average elevation is only 22 feet above high-water mark. Eight miles north of its outlet it suddenly contracts at Ross Point to rather less than a mile across; and the northern division of twelve miles in length varies in breadth between a mile and only a fourth so much. The lower wide division of the loch, at a short distance from the shore, varies in depth on the whole from 8 to 12 fathoms; and these soundings continue till near Point Ross, where there is a rapid increase to 32 fathoms. This continues to be the average in the middle of the lake, till at the next contraction in its width, opposite Rowardennan Point, where it singularly shallows at once to 9, 8, and 7 fathoms. A mile further up, after another swell, it quickly deepens at a new contraction at Rhuda Mor (the Great Point) to 65 fathoms; and for five miles further north the soundings first steadily deepen by degrees to 105 fathoms, and then shelve to 80 opposite Inversnaid; above which point the lake becomes both much narrower and greatly less deep (Admiralty Map). My observations on its waters were made near Tarbet, which faces the middle of the very deep five-mile reach, where the soundings in mid-channel are never under 85, and at one place, opposite Culness farm-house, attain the extreme depth of 100 and

even 105 fathoms,—the width there being barely three-fourths of a mile.

The surface water over these great depths is of remarkable purity. Its saline matter is very scanty, and the colouring organic matter equally so. Still it has a faint yellowish colour. On September 21st, the second day after heavy rain, incessant for twelve hours, a white porcelain basin, 4 inches in diameter, disappeared in 18 feet of water; on 11th October, after many days of alternate rain and drought, in 15 feet; and on 18th November, after four days of dry weather, in 14 feet, but in feeble sunshine.* After long drought there is little doubt that the colour would be less, for it will be seen subsequently, that as the streams pour in fresh supplies of water, there is reason to suppose that these penetrate little before they run off, and consequently the coloured flood water from the streams will colour for some time the superficial waters of the lake.

On 18th November, the water taken from the surface of Loch

* This is a good method of ascertaining the relative colour of waters if it be employed with due precautions. The trial should be made in sunshine—when the sheet of water is quite calm—between 9 A.M. and 3 P.M., so that the sun's rays may not fall too obliquely on the water, and with the back to the sun, and, best of all, on the shady side of a boat. If all these conditions be reversed, vision will penetrate scarcely half so deep as when they are all observed. In my recent trials I have not found a white object visible at a greater depth than 21 feet, viz., on Loch Lomond on the 6th May. But, from observations made many years ago, I am satisfied that, after long dry weather, some river waters will allow such an object as a white porcelain basin to be seen at a much greater depth, with due attention to the conditions now mentioned. Having a recollection of seeing it stated long ago, that the water of the Lake of Geneva was so clear, that objects could be distinguished in it at a very great depth, I applied to Dr Coindet of Geneva for precise information, for which he referred me to Professor Forel of Lausanne. To Professor Forel's kindness I am indebted for the following interesting facts:—In the spring of 1869, using a white-painted sheet of iron, 15 inches by 12, he found that the utmost depth at which it could be seen was 13 metres, or 44 feet. The transparency is much affected by locality, and very much too by season. In winter and spring it is greatest, in summer and autumn least. In the Bay of Morges, objects may be seen distinctly at the bottom in winter at a depth from 13½ to 20 feet, while in summer they are barely visible through 7 feet. This difference is greatest near the shore, at the bottom of bays, and near villages or towns. It is least around promontories, far from land, and at a distance from human habitations. In autumn the change from obscurity to transparency usually takes place early in October, and is completed in three days; in summer, the reverse change takes place

Lomond over a depth of 102 fathoms, or 612 feet, presented in a 16-inch tube as exactly as possible the same degree of faint yellowish hue as the water of Loch Katrine. Evaporated to dryness, it left a pale, greyish film, amounting to a 33,000th of the water. It had only 0·70 degrees of hardness by Clark's soap-test. Of the other liquid reagents, acetate of lead alone caused at once a slight haze; oxalate of ammonia and nitrate of silver had at first no effect, but in time caused an extremely faint haziness; nitrate of baryta, and ammoniacal phosphate of soda had no effect at all. When the water was much concentrated, however, sulphates, carbonates, and chlorides, as well as the bases, lime, soda, and magnesia, were clearly indicated by their ordinary tests, exactly as in the springs and streams of the adjacent country.

I examined also the water taken at the same place from the bottom at the depth of 102 fathoms. This differed in some respects from the surface water directly above it. It contained the same salts. But nitrate of silver indicated rather less chlorides; acetate of lead more carbonates; the soap-test denoted a trifling additional hardness, namely 0·74 degrees, and the total solids amounted to a 28,000th instead of a 33,000th. Farther,

about the beginning of May, and is more gradual. By filtering a large quantity of turbid water, he found the obscuring cause to be a collection of amorphous dust, living and dead diatoms, vegetable debris, a few living infusoria and crustaceans, and debris of insect larvæ and microscopic crustacea. They naturally collect slowly in the summer; but the first cold of approaching winter sends them quickly down with the water as it cools.

In the case of Loch Lomond, these inquiries of Professor Forel would lead one to expect little influence from organic or inorganic dust in obscuring water where it is so deep as at the places chosen for my observations. Accordingly, the surface water was remarkably free from turbidity, or deposit on standing at rest. But the yellowish colour, faint though it be, constitutes a no less powerful obstruction to the penetration of light. The depth of colour, and consequently the transparency, vary at different periods, not so much with the seasons as with the times of floods. In advanced summer and in autumn, the floods increase the colour decidedly, and lessen for a time transparency. But my single observation on 6th May, when I found the transparency greatest of all a few days after heavy north-east rain, raises a question whether floods have the same effect in spring or the end of winter. A probable reason for the contrary may be, that the soluble matters of the peat-fields and stream-courses, developed by heat, growth, and atmospheric action in summer and autumn, are much exhausted by the frequent winter floods before the arrival of the floods of spring.

although the colour is the same at the bottom as at the surface, and very slight, it is distinctly deeper in shade when seen in a 16-inch tube; and the film left on evaporation, instead of being light grey, is of a rather deep yellowish-brown tint.

[*May* 16*th*, 1872.—As supplementary to these observations, I may here add the following, which I had an opportunity of making on the 10th of last month:—During the five winter months intermediate between my previous visit in November, the winter had been unusually open. Until the middle of March, indeed, there had been very little frost, and no severe cold. During the latter half of March frosty northerly winds prevailed, but without any very great fall of the thermometer. In the last days of March and first three days of April, snow fell frequently, covering the Highland mountains to their bases. Ben Lomond and the adjacent Arrochar mountains shared in the change. On 4th April the wind veered to west and south-west; bright sunshine and warmth soon dissolved most of the snow, and this weather continued, with scarcely any rain, till after my visit. The ground around Loch Lomond was consequently dry, the hill streams very low, and the streamlets dried up, or nearly so.

The surface water corresponded with these antecedent circumstances. Frequent winter floods had swept from the mountains most of the soluble matter from their beds; and for some days the streams, reduced to rills, would have little remaining to remove from their stony channels. Hence the surface water was of great purity. A white porcelain basin, two inches in diameter, was visible at the depth of 16 feet, although a light breeze rippled the surface. In a 16-inch tube the yellowish colour was extremely faint. The solid contents amounted to only a 32,000th of the water, and lost a fourth by incineration.* Nitrate of silver occasioned in the water only the faintest haze, and oxalate of ammonia did not visibly affect it. The soap-test indicated 0·49 of hardness, which is equivalent to a 145,000th of carbonate of lime. In accordance with its purity this water acted powerfully on lead. Action commenced at once, loose crystals of carbonate of lead were formed

* 26,250 grains left 0·83 at 300° F., and 0·62 after incineration.

in abundance, and in twenty-three days a plate an inch and a half square lost 1·11 grain in weight.

The bottom water, taken where the depth was 594 feet, differed materially in these characters. The cistern brought up some finely comminuted peat-like matter, in which the microscope detected a profusion of various diatoms, and two species of active microcosmic animals. The colour of the water was deeper than that of the surface, and became the same not till the addition of half its volume of colourless distilled water. Nitrate of silver produced an immediate scanty precipitate, oxalate of ammonia scarcely any effect. The soap-test indicated 1·015 of hardness, which is the equivalent of a 69,000th of carbonate of lime. The solids amounted to a 16,000th of the water, and lost a third by incineration.* When the water was evaporated to a tenth of its volume, nitrate of silver indicated chlorides in abundance, nitrate of baryta sulphates feebly, oxalate of ammonia lime sparingly, and phosphate of ammonia magnesia faintly. The original water had no action at all on lead. The lead plate became dull in a few hours, but no other change ensued which the eye could discover; and in twenty-three days the plate, which originally weighed 405·73 grains, weighed 405·74 grains.

These differences between the bottom and surface waters were so great, that it became desirable to repeat the examination, which I was able to do on the 6th of the present month. A good deal of easterly rain had fallen for some days until two days before this visit; but the hill streams had already become low. The waters were collected near the same place as before,—the bottom water from a depth of 94 fathoms, or 564 feet. The cistern brought up, as formerly, some peaty-like matter, which speedily subsided, and was promptly removed by decantation. Both specimens of water were very pure. But the bottom water was more affected than the surface water both by nitrate of silver and by oxalate of ammonia, and its colour was decidedly deeper, so that fully more than half its volume of colourless distilled water required to be added, to produce the feeble tint of the water from the surface.† The peaty matter

* 13,125 grains left 0·82 grains at 300, and 0·55 after incineration.

† The cistern which brought up the water was new, made of copper, and furnished, for valves, with spherical copper balls resting on hemispherical beds,

was found by microscopical examination to abound in diatoms and skeleton tissues of graminaceous and other vegetables. The bottom water contained a 25,000th of solids.

It has been proposed, in projects for introducing lake water into a town for domestic uses, to draw the water from a considerable depth, instead of from the surface, under the supposition that the deep water is the purest. The preceding observations show that this is a mistake, at least in the case of some lakes. On every occasion I have found the water of Loch Lomond somewhat more saline in its deepest parts than at the surface immediately above, and decidedly more coloured. The cause is easily understood, if the preceding chemical examination be taken in connection with the observations to be subsequently made on the temperature of Loch Lomond at various depths. For the results of both inquiries concur in indicating that, in the very deep parts, there is a vast body of still water which undergoes little, or, perhaps, no change or movement, and which, therefore, at the bottom, will become impregnated with whatever is soluble in the bed on which it rests.

Let me now change the scene to the hills and the waters of the Lowlands.

In the course of late notorious proceedings in this city for obtaining a more abundant water supply, it was stated by good chemical authorities that the water of St Mary's Loch in Selkirkshire, although of remarkable purity, does not exert upon metallic lead that eroding action which is a singular property of all pure waters previously subjected to trial. This statement was so opposed to the principles regulating the action of waters upon lead, as propounded by me so long ago as 1829, and also to the facts brought forward both then and in a paper read to this Society in 1842, that I resolved to investigate the question for myself.

This undertaking, in spite of my strong repugnance and steady refusal to be involved on either side of the Edinburgh water-controversy, led indirectly to my being compelled to concern myself with it as a parliamentary witness. But let it be clearly understood

and it was never used except for these experiments. The cistern was emptied at once into stoppered bottles on being drawn into the boat, and was carefully dried in a current of air with the valves open.

that my inquiries were undertaken quite irrespective of all controversial proceedings, parliamentary or otherwise, and for a purely scientific object—in which point of view alone I shall now proceed to state them. In the present place, I shall notice the lead question slightly, reserving that inquiry for another head of my observations. At present I have to say a few words of other matters which arose incidentally before me in the course of my inquiries.

St Mary's Loch is a lonely lake, retired among the hills of Selkirkshire, 37 miles south from Edinburgh. It is three miles long, and about half a mile in width at its broadest parts; but it may be said to be prolonged nearly another mile by the Loch of the Lowes above it, which is separated only by a space of 150 yards, through which the upper loch is joined to St Mary's Loch by a small stream. The lake in most parts shelves rapidly to a depth of 30 or 40 feet; in various parts it is said to deepen to 80, 100, and even 150 feet; and at a place pointed out to me as the deepest, I found 144 feet of water. It discharges itself in a goodly body of water, by a broad, shallow outlet to constitute the Yarrow Water. This joins the Ettrick a mile and a quarter above Selkirk; and the united waters, under the name of Ettrick, are poured, after a course of about four miles more, into the river Tweed. The Yarrow runs over 11 miles in a right line, but 14 miles by its windings, in a very stony channel, obviously of great width in floods.

The country of the Yarrow and St Mary's Loch is almost entirely pastoral, except where covered at the lower end of the stream by the beautiful woods of Bowhill, Philipshaugh, Hangingshaw, and other country seats. Around the lake itself the land may be described as consisting purely of pastoral hills, the attempts at arable culture being as yet very limited, and wood hitherto a scanty and stunted ornament. The level of the lake is almost exactly 800 feet above the sea. It is bordered everywhere, and abruptly, by hills rising from 750 to 1000 feet above it, showing long sky-lines, and steep slopes which present no rocks, no woods, nothing but smooth grass, unbroken save where scored by a few stream courses, mostly waterless in dry weather. But the Meggat Water is a considerable permanent stream, seven miles in direct length, which falls into St Mary's Loch about its middle line on the north; and the Little Yarrow, three miles in direct length, feeds the Loch of

the Lowes at its upper end. These streams, though short, are voluminous, because constantly supplied by numberless hill tributaries.

A traveller on the loch-side sees no peat anywhere. The district was therefore pronounced by recent one-eyed visitors to be free from peat. An inquisitive observer might have suspected the reverse from one of the highest surrounding hills being called Peat-Law; and on the high sky-line of another, a telescope would have betrayed to him a very suspicious circumstance in a crowd of little peat-stacks. Any one, not content with creeping along the bottom of valleys, but familiar with the summits of the mountains of the Scottish Lowlands, would then have known that the sky-line seen from the loch-side is not,—as it very often is in the primitive mountains of the Highlands,—a mere ridge, but forms the edge of a great table-top, which, in most cases, is chiefly composed of peat. In point of fact Professor Geikie has shown last summer, from the Government Geological Survey, that a vast proportion of the hill-tops in the St Mary's district consists of peat table-lands.

The consequences which flow from this structure of the country are peculiar. In dry weather the high peaty summits of the hills will cease to supply moisture enough to drain into the streamlets which score their sides. These will then convey to the lake chiefly the drainage of the grassy slopes, and the produce of the scanty springs in the lower regions. But when a rain-flood sets in, the peat, whether previously dry or moist, will send down a profusion of peaty water. Had the Yarrow flowed as a river through the vale at St Mary's, the peaty flood would have been swept quickly down towards the sea; and in two or three days the waters would have recovered from their peaty impregnation. But the two lochs, with a superficial area of two square miles, store up the peaty water, and dole it out, like a compensation pond, for many days, until the arrival of a fresh flood to renew it. An embankment at the outlet, to increase the storage, would protract the outflow, and postpone still further the recovery of the water from impurity.

These facts and views could only occur to one familiar with the district, or going thither to study it for a practical object. When I first went to St Mary's Loch on the 12th and 13th June last, I

had no further acquaintance with the hill structure around than that of an angler thirty years ago, when I probably looked more at what came out of the loch than at anything else concerning it. I consequently went prepossessed in its favour by the glowing account given of its extreme purity by its admirers. My surprise, therefore, was not small when my very first observation showed that its water was yellow. My visit was made in circumstances highly favourable to its condition, in splendid sunshine, being the last two days of six weeks of extraordinarily dry weather, broken only by a few light showers, sufficient to freshen the grass, and little more. But I found that my white porcelain basin became at once yellowish when dropped into the lake, acquired a lively amber hue at the depth of 3 feet, and disappeared entirely at 12 feet, while the sun shone brightly on the spot. I remembered well, however, having once distinguished small pebbles in the Dumfries-shire Esk through 16 feet of water, when spearing salmon in a still pool, and on another occasion through 21 feet in a pool below the Bracklinn Falls, near Callander. I afterwards tested the colour of the loch water on a small scale, and showed it satisfactorily to many, by comparing it with the water of Edinburgh of the same date in two narrow glass jars, 20 inches in height, with a circular disc of white porcelain at the bottom. The porcelain was of unstained whiteness as seen through the Edinburgh water, but of a lively amber tint when looked at through the water of St Mary's Loch. The difference was not less marked in the narrow 16-inch tubes. Even in dining-table water-bottles, placed on a white table-cloth, the colour of the loch water was such as to make it evident, that certainly nobody would drink it who could get the other. I may add that, when I revisited the loch on 8th September, also in bright sunshine, I found that my porcelain basin disappeared entirely in eight feet of water; and, nevertheless, there had been previously ten continuous days of absolutely dry weather.

On the 12th and 13th June, I saw in the water no want of the water-fleas, which excited so much interest and heat in the late controversy. It may create additional interest with some to be told that three months later they were decidedly bigger, busier, and altogether more deserving of their vernacular name.

Before speaking of the chemical composition of the water, let

me finish what may be said of the physical characters of the loch, by noticing one not yet adverted to. Visitors in the dry season, when the waters of the lake are somewhat shrunk, have been much struck with the beauty of its border,—its "silver strand." This is owing to a uniform beach of crowded, chiefly angular, or partially rounded, light-grey coloured stones. The colour, however, is not their own, but belongs to a generally dense covering of a dried-up matter, composed of a multitude of various diatoms entangled in the delicate lines of a finely fibrous conferva. In the fresh state this investing matter is dark greenish-brown, close, and slimy. The stones, therefore, give the loch, even in its shallows, a disagreeable, dark, deep appearance, abruptly defined by the water's edge. But all of them out of water acquire, in drying, a light grey or greyish-white hue. Every scientific visitor has observed, and some have carefully examined, these stones and their covering. But, so far as I am aware, no one has noted their full significance; of which more presently, when I come to speak of the Yarrow.

The water of the loch, though it is coloured, is a pure water,—in the sense that it contains very little solid matter in solution. It has been repeatedly analysed, and found to contain rather less than a 20,000th part of total solids. Mr Dewar, the latest analyst, I believe, found a 22,440th,—of which the inorganic salts constituted two-thirds [a 37,000th], and the organic matter one-third [a 55,500th]. The chief inorganic salts are the same as in the mica-slate streams and lochs of the Highlands, and much in the same proportion to one another. The hardness of the water was found by Mr Dewar to be 1·30 degrees by the soap-test, or nearly twice that of Loch Lomond surface water. Other chemists have found more solids, some less. My own results, with water collected on 13th June, show more saline, and rather less organic, matter; which is no more than might have been anticipated from the long antecedent very dry weather. I found the solid contents dried at about 300° F. to be a 15,000th of the water; one-fourth of this was destroyed by slow incineration at a low red heat; and the hardness was 2·0 degrees of Clark's soap-test scale,—which is about the fourth part of that of the present Edinburgh water supply. Water collected three months later, on 8th September, after ten days of complete drought, which, after a few days of showery weather,

followed the very heavy floods of 24th August, contained more colouring matter, exhibited less action with the ordinary liquid tests for the inorganic salts, and had a hardness of 1·4 degree only. I have no doubt that this water corresponded in all respects very closely with the specimen examined by Mr Dewar.

Thus, it appears, that the waters of St Mary's Loch—which, with the exception perhaps of those in the primitive districts of Kirkcudbrightshire and Wigtownshire, may be taken as a type of the lowland lochs at large—differ from the waters of the Highland lakes in containing more solid matter, a little more saline matter, and decidedly more colouring organic matter, and in being considerably harder, though really belonging to the "soft" waters too. Another difference is that they vary more with the season, the salts becoming rather more abundant in long dry weather, and the colouring matter clearly abounding more during and after floods. Finally, a remarkable difference in property, to be discussed by-and-by, is, that unlike the waters of the Highland lochs, that of St Mary's Loch does not erode lead. But first let me say a word or two about the Yarrow Water, by which this lake discharges itself.

The Yarrow, before uniting with the Ettrick, winds for 14 miles through a narrow, bare, chiefly pastoral vale, bounded by gently sloping hills. It is joined in this course by twenty-two tributaries, of which only three or four are considerable streamlets, the others being mostly rills, apt to be dried up, or nearly so, in dry weather. The waters of the chief tributaries contain in the dry season more salts than the main stream itself, but very much less colouring matter, two of them, indeed, none at all appreciable even in a 16-inch tube. The channel of the Yarrow is wide and stony, and the stream shallow, and for the most part turbulent. In the 14 miles it falls 220 feet. Its banks present very few human habitations.

These circumstances are favourable to the gradual diminution of organic impregnations, partly through the decomposing influence of fresh earthy salts added here and there by little tributaries, partly by the slow oxidation, to which Liebig gave the name of "Eremacausis,"—"quiet" or "slow burning." My attention was turned very long ago, before the publication of Liebig's views on this subject, to the rapidity with which, by natural processes,

streams rid themselves of the unnatural impurities introduced into them by sewage, and by some of the manufactures. But I am not aware that the process of clearing has been watched with care in circumstances altogether natural. It occurred to me, at anyrate, that we have in the Yarrow a most favourable opportunity for tracing this process in the case of a natural water of a remarkable kind, under the operation of natural causes alone. On the 8th of September, therefore, I examined the course of the Yarrow with some attention.

In its descent from St Mary's Loch, it is first joined by two unimportant rills, at that time nearly dried up by ten days of previous drought. A mile and a half below its outlet, it receives from the north its largest tributary, the Douglas Burn, which drains a very hilly country about five miles and a half long and four miles wide. This stream, indeed, was at the time a small rill, compared with the strong body of water in the Yarrow. But it was interesting in this respect, that its water, containing more saline matter than the main stream, and possessing the hardness of 4·90 degrees, presented no colour at all, even when examined in a 16-inch tube. This last fact is remarkable, because the Douglas Burn comes very much from peat-topped hills, so that either the peaty water of floods soon runs out in dry weather, and spring-water is alone left, or the water clears itself by eremacausis, or in its upper course in the way in which purification seems to be brought about in the Yarrow.

For, when I came to examine the Yarrow immediately above the junction of the Douglas Burn, I found to my surprise that the colour, which at the outlet was such as to render a porcelain basin invisible when sunk 8 feet only, was already so much reduced, in the course of a mile and a half, as to approach the faint hue of the waters of Loch Katrine and Loch Lomond. There was also a slight increase of salts, as shown by the ordinary liquid tests, and also by the hardness of the water having increased from 1·4 to 2·40 degrees.

A mile lower down another principal tributary, but inferior to the Douglas Burn, falls into the Yarrow on the right, the Altrieve Burn, which, however, I had not time enough to examine. Two miles further on a similar streamlet joins from the right, the

Sundhope, which, too, I could not examine. Other trifling rills, almost dried up, join between the Douglas Burn and Yarrow kirk, seven miles from the outlet of the lake. This point was a good one for studying the joint effect of atmospheric exposure through constant agitation, and of the influx of several brooks, all probably containing more salts than the main stream itself. Here I found that the soap-test indicated a further increase of hardness to 3·0 degrees, and that the yellow colour in a 16-inch tube was still further reduced, but not much.

In the next three miles and a half there are six little tributaries, all at the time of my visit insignificant, and some quite dried up, till we arrive at the Lewenshope Burn, which drains from the north a considerable stretch of the Minchmoor range, described to me as generally stony hills, without much peat. This water possessed 6·5 degrees of hardness, and so little colour that it was barely appreciable in a 16-inch tube. In the remainder of its course the Yarrow is joined by five more rills, either almost dried up when I was there, or appropriated in a great measure for the supply of mansions. Four hundred yards above its junction with the Ettrick, I found its water to possess, as at Yarrow kirk, seven miles higher up, 3·0 degrees of hardness, so that the comparatively saline water of the Lewenshope had not materially increased the salts of the Yarrow. But the colour was still more reduced, so as to be very faint indeed, equally so with the colour of the water of Loch Lomond.

Thus the principal loss of colour takes place in the first mile and a half of the river's course; but there was also a very appreciable additional improvement in the longer course below, and the final result was a nearly total removal of colour.

To what is this change owing? Does it depend entirely on the intermixture of earthy salts from the tributaries, and on eremacausis? I apprehend that these causes will scarcely account for the great change effected in the first mile and a half. There may even be a doubt whether peat-extract is particularly subject to the process of eremacausis. It is well known to be a preservative of organic matters, which it could scarcely be were it very subject to decay itself; and I find that a solution of it without any saline matter, has undergone no change in a warm room, in a half-filled

bottle, during six months. But there is a more potent agent at work in the Yarrow. The dark, green-coated stones of the loch, with all their characters unreduced, pave the entire channel of the stream as low at least as the confluence of the Douglas Burn, and, with a less abundant covering, so low at least as Yarrow kirk, seven miles from the outlet of the lake. But there is nothing of the kind in the chief tributaries. At the junction, for example, of the Douglas Burn, there is an abrupt line of demarcation between the dark green, slippery stones of the Yarrow, and the stones of the tributary, which are as naked as if they had been scrubbed clean with a brush. I do not well see how to escape the conclusion, that the confervæ and diatoms of the stones live at the cost of the peaty matter from the loch,—that peat-extract is their food and is consumed by them. This is a ready explanation of their excessive growth on the stones of the loch. The want of such food equally explains the comparative absence of them from the stony banks of Loch Lomond, and the stony channels of all the streams of the adjacent mica-slate district.* Indeed, in the opposite circumstance—in some mountain tarns of the district, resting, as they may, on peat, and surrounded by it—the slippery, dark green, stony bottom is no uncommon occurrence.

If these views be correct, it is easy to appreciate both the unfavourable significance in a lake of a dark-green bottom of stones, densely covered with confervæ and diatoms, and likewise their value in a running stream; and it may be well also not to let the imagination run away luxuriating in every "silver strand" that meets the eye.

The Temperature of the Deep Fresh-water Lakes of this country has no connection with the preceding inquiries, further than that my observations on the subject arose incidentally while I was carrying on the inquiries in question. The results I have obtained may interest the cultivator of physical geography, if I am right

* It has been said that stones covered with green confervæ and other diatoms do occur in Loch-Lomond. They do in bays and other shallows; but the covering is very thin; and the line of such stones is narrow. Where deep water is near there are none at the edge, and where they do occur the dry stones close to the edge appear quite clean.

in supposing that no prior observations of the kind have been made on our deep fresh waters. [See, however, p. 574.]

In the course of the discussion of the St Mary's Loch water-supply scheme, opposite opinions were expressed as to the relative advantage of drawing the water from the surface of the lake, or from a considerable depth; and weighty arguments, of a speculative nature, were advanced on both sides of the question. It occurred to me, therefore, to consider what becomes of the deep water. Does it escape as that of the surface must do? And if so, How? It appeared to me that during a winter of such protracted cold as that of 1870–71, the water at the bottom would probably acquire so low a temperature, that it must long remain there. For it can only rise again, either by its temperature falling below 39°·5, when its density decreases instead of continuing to increase, or by being heated by the heat of the earth beneath; and it is unlikely that the temperature of the entire water of a deep lake will fall lower than 39°·5, or indeed so low, in this latitude, and the heat derived from the earth, in our latitude at the elevation of 800 feet above the sea, must be inconsiderable. It is well known that the bottom cannot be heated by conduction from the summer heat of the atmosphere above, as in the case of a solid substance; and the effect of the penetration of the sun's rays, by which the water is heated to a certain depth, cannot descend very low in a lake, the water of which is, like that of St Mary's Loch, so coloured as to render a very white object invisible at the depth of 8 or 12 feet. The conclusion would be that the water at the bottom of the deep parts of the lake, in the absence of strong springs—of the existence of which there is neither proof nor probability—will remain at the bottom for want of a current during the whole warm season, and perhaps longer.

When I was first at St Mary's Loch on 12th and 13th June, I had no suitable thermometer for taking observation of deep temperatures. But Mr Dewar kindly undertook to make the necessary trial a few days later in the same month. With a Six's thermometer, whose graduation was subsequently tested and found correct, he ascertained that in 150 feet soundings, the temperature, being 56 at the surface, was 46° at the bottom. When I revisited St Mary's Loch on 8th September, nearly three months afterwards, the inter-

mediate weather having been generally fine, I found, with the same thermometer, in 96 feet of water, near the head of the lake, 56° at the surface and 54° at the bottom; and in 144 feet of water, in the middle of the loch, exactly opposite the 17th milestone from Selkirk, I obtained 55° at the surface and 47° at the bottom. During three of the warmest months of last warm season, the heat of the earth, or the sun's rays, had heated the water at the bottom by one degree of Fahrenheit only. I do not well see how that water can ever rise from such a depth, unless its temperature during the winter should fall below 39°·5, which is not probable.

I regret I did not take successive observations at several depths in order to fix the upper limit of the cold substratum of water. My time was short, for my main object on that occasion was the changes undergone by the river Yarrow, and I contemplated a chain of observations in more favourable circumstances at Loch Lomond. I went to Loch Lomond on four occasions for the purpose, viz., on September 14th, September 21st, October 11th and 12th, and November 18th. As accurate observations were made only on the two last occasions, I shall refer to the others only incidentally.

On 11th October, at 3 P.M., the atmospheric temperature on land being 48°, and that of the surface water everywhere over deep soundings 52°, I found in 103 fathoms of water opposite Culness, with a Six's thermometer by Casella, which, though not specially protected against high pressure, was believed to be proof against such pressures as it was to be subjected to, that a temperature of 43° was indicated at 200 feet, and 41°·8 steadily at 400, 500, and 618 feet. Next forenoon at 11, I repeated my observations about a mile lower down opposite Tarbet in 87 fathoms. The air was singularly still, the atmospheric temperature on land 44°, and that of the loch on the surface 52°, exactly as on the previous day. The following successive temperatures were obtained at various depths:—

Surface,	52°·0	150 feet,	44°·5
25 feet,	51°·5	200 „	43°·0
50 „	50°·2	300 „	42°·0
75 „	50°·0	400 „	42°·0
100 „	49°·5	518 „ bottom,	42°·0

It will be observed that these temperatures correspond almost exactly with such observations of the previous day as were made a mile and a half further north at the same depths, where the soundings were 618 fathoms. The bottom temperatures also corresponded with what I had observed with a different thermometer on September 21st, three weeks earlier. Using a cistern with proper valves, constructed by Mr Adie, for bringing up 96 ounces of water from the bottom, with a simple thermometer in it, I found that on September 21st, when the surface temperature was 54°, and also on October 11th, when it was 52°, the thermometer, on the instrument arriving at the surface, indicated 44° in the water brought up from the bottom, both in 87 and 103 fathoms of water. As the heating of the cistern in ascending must have been very nearly or altogether the same on both occasions, it follows that the corrected temperature at the bottom, as on 11th October, was 42° on 21st September.

On 18th November I found it to be also the same. Cold weather had set in for a week before. The air was frosty, the ground dry and hard, the atmosphere very clear and perfectly still. Near the lower end of the loch, where the highway first touches it, the air temperature was 33° at half-past eleven. At Tarbet at one P.M., it was on land, but at the water's edge, 37°; in the boat, in the middle of the loch, two feet above its surface, 42°; and in surface water, over 610 feet soundings, 46°. At the bottom, by a Casella's thermometer, protected against pressure, and corresponding exactly in its graduation with the unprotected one previously used, the bottom temperature was again 42°. My design to make at the same time another complete series of observations, was prevented by unexpected delays shortening my time very much, so that I had to confine myself to a single additional observation, for determining more nearly the upper limit of the cold substratum of water. At 250 feet I obtained a temperature of 42°·25, and consequently the upper limit of the water at 42° must have been as nearly as possible at 270 feet in 610 feet soundings.

Before drawing confident deductions from these observations, they require to be repeated at other seasons. But in the meanwhile it may be well to see what are likely to be the results.

It is plain, in the first place, that in a deep lake in this latitude, there is a very gradual and slight increase of cold in the warm season for the first hundred feet, viz., by 2°·5 only, then a sudden descent by 5°·0 in the next 50 feet only; next another slow descent by 2°·5 in 150 feet; and finally, below that a great substratum of 250 feet of water, and at a deeper spot of no less than 350 feet, at the uniform temperature of 42°, or a little less. Next, at Loch Lomond no change took place in the temperature of the bottom water during two months of unusual warmth for the months of September and October, and no change at 300 feet from the surface during five weeks prior to the middle of November.

It seems certain that the temperature of the great substratum of cold water cannot be raised after the middle of November, when the cold season has fairly set in. Whether it is to be lowered during winter, or whether the substratum, without becoming colder, will merely have its upper level raised, is a question to be settled by observation at an early period of next spring.

In the meanwhile, abstracting the highly improbable existence of strong springs at the great depths I have mentioned, it does not appear how this vast cold substratum could have been moved during last summer and autumn. Neither does it appear how it can be moved during the winter, unless the equally great stratum above it acquire a lower temperature than 42°, and so take its place; for the uniformity of the bottom temperature between 21st September and 18th November, when no additional cold could descend through the warmer stratum above, is sufficient proof that the influence of the heat of the earth beneath is too feeble in this latitude to make itself sensibly felt by motion of the water.

Thus there is a probability, that when water once descends to so great a depth as the bottom of our deep lakes, it cannot ascend again except under rare and extraordinary circumstances. If this view be correct, the movement of the waters of a deep lake towards its outlet for escape, must be confined very much to the warm water at its surface, or to no great depth, and, therefore, mainly to the waters which are constantly supplied on all sides by its feeding streams. This must be the case in summer and in autumn; it may be the case in winter also

[*May* 18, 1872.—Circumstances having delayed the publication of the Society's Proceedings, I take this opportunity of adding the result of recent and conclusive observations. These were made on 10th April and 6th May, as near as I could to the place of the observations described above.

April 10.—The weather on this occasion was very fine and favourable for my purpose. During the whole winter period after November 18th, the date of the last observations, the weather had been remarkably open. The mean temperature of the atmosphere for the five intervening months, as kindly calculated for me by Mr Buchan, Secretary of the Meteorological Society, from observations at Balloch Castle, at the southern end of the loch, was 1°·4 higher than the average for the same months for thirteen previous years.* Consequently, the same influence of the winter season on the temperature of deep waters cannot be expected as in ordinary winters, or in a hard winter, such as the preceding one of 1870–71.

When I made my observations, about 3 P.M. on 10th April, the temperature of the air on land was 55°; and on the water, one mile from the shore whence the wind blew, it was 53° in the boat, scarcely 2 feet above the surface of the lake. The following temperatures were obtained, at various depths in the same place:—

Surface, . .	43°·0	150 feet, . .	42°·1
50 feet, . .	42°·6	200 „ . .	42°·0
75 „ . .	42°·2	594 „ bottom,	42°·0
100 „ . .	42°·2		

These observations were made with Casella's protected thermometer. The thermometer in Adie's cistern, for bringing up water from the bottom, also stood at 42° when brought up to the surface, the temperature of the upper warmer stratum being much too low to affect the cistern in its passage.

May 6.—Between 10th April and this date the weather varied

* In the course of his calculations Mr Buchan arrived at the interesting fact that the average mean temperature of the air during the six cold months of these years, at the level of the lake's surface, was 41°·7 from November 18 to April 10, or very nearly that of the deep substratum.—*See subsequently, for his observations, the later Proceedings of the Society.*

as to warmth; but there was a large proportion of sunshine, and little rain, till three days before, when there was a heavy fall with an easterly wind. The temperature on land, within fifty yards of the water, was 55°. The following observations were made at 2 P.M.:—

Surface,	44°·5	150 feet,	42°·7
25 feet,	43°·7	175 "	42°·6
50 "	43°·5	200 "	42°·5
75 "	43°·2	250 "	42°·4
100 "	43°·1	300 "	42°·1
125 "	42°·8	574 "	42°·1

The thermometer in Adie's cistern, when brought up full of water from the bottom, but raised rather deliberately, stood at 42°·5.

It appears, from these and the preceding observations, that in the deep parts of Loch Lomond there is a substratum of water of several hundred feet, which, between the end of September last and 10th April, has been steadily of the temperature of 42°; and that during last winter no other change has taken place, in relation to temperature in or near it, than that the level of the cold substratum rose in the interval between 70 and 100 feet. A winter, materially colder than the last unusually mild one, would at least raise that level still nearer the surface. Whether it may reduce the temperature still lower than 42°, is a question which remains to be decided by future observation. It is still also a matter for observation, whether the temperature of the substratum may not rise a little during summer. For it may be reasonably said, that the unusually hard winter of 1870–71 might have lowered the temperature of the substratum in April of last year below that observed in April of this year after a very open winter, and, consequently, under 42°, which was the temperature observed in October. But the difference, if any, cannot be considerable; for it can only arise from the heating power of the earth on which the water rests.

The water of a lake is heated in summer and autumn in three ways—the heat of the atmosphere, that of the sun's rays, and that of the earth. The atmosphere will communicate its heat to so much of the superstratum only as is disturbed, more or less, by the wind; and, therefore, cannot penetrate many feet. The tempera-

ture of the earth at the bottom, from 500 to 600 feet under the sea-level, should be by theory about 60° in the deepest parts; but, considering the very low conducting power of the rocky structure of the earth, its heating power over so vast a bed of cold water must be very feeble. The sun's rays are at once the most energetic heating power, that which penetrates deepest, and that which alone can sensibly heat any part of the superstratum of water underneath the thin bed near the surface, where it is aided by the warmth of the atmosphere, and the stirring of the water by the wind. But there is a limit to the sun's penetration in such depths, when the water, as in the case of Loch Lomond, is coloured, however slightly. It has been imagined that the presence of springs at the bottom may be a fourth source of influence over the temperature. If there be any springs there, the effect must be to heat the water. But, as there are no springs in Scotland which rise above the surface, or present other proofs of owing their place to unusual sources of pressure, it seems most improbable that any are so constituted as to overcome the pressure which exists at the bottom of a very deep lake.

Every known consideration,—the great thickness of the cold substratum, its steady low temperature, and its greater colour than at the surface—contributes proof that this substratum can undergo little or no movement, unless an unusually hard winter should displace it by colder water from above.*]

The previous observations have extended to so great a length that I must postpone till another opportunity the remarks which I have prepared on the third of my promised topics—the Action of Water on Lead.

The following Gentlemen were elected Fellows of the Society:—

Alexander H. Lee, Esq., C.E.
Robert Lee, Esq., Advocate.
John Anderson, LL.D.

* While the preceding statements were passing through the press, my attention was called to similar observations in Sir John Leslie's article on Climate in the "Encyclopædia Britannica," by Saussure on the Lakes of Geneva, Thun, and Lucerne, and by the late eminent engineer, Mr James Jardine, on Loch Lomond and Loch Katrine in 1814. Their observations are not entirely concordant with those given above. I contemplate further observations which may reconcile them.

Monday, 18th December 1871.

SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Communications were read:—

1. On the Computation of the Strengths of the Parts of Skeleton or Open Structures. By Edward Sang.

The first part of the paper is devoted to the computation of the strengths of the parts of a structure destined to resist given strains, taking into account, along with those strains, the unknown weights of the parts. The results obtained by this process necessarily give the best possible arrangement of the strengths, since, if any one part were made weaker, the whole structure would be weakened; or, if a part were made stronger, the unnecessary weight thus thrown upon the other parts would also go to weaken the fabric. It is believed that this investigation has now been given for the first time.

It was pointed out that this method enables us to determine the utmost limit of magnitude of a structure having a given general configuration.

The second part concerned deficient or flexible structures; the mode of discovering the relations among the applied pressures, needed to cause the structure to assume a prescribed form, was indicated.

Thirdly, the case of redundant structures was gone into. It was observed that the absolute strains on the parts of such structures depend, not merely on their form, but also on the manner of putting them together. The changes on these strains caused by additional loads can, however, be computed by considering the compressions or distensions of the parts; and it was pointed out that the computation of these changes has been mistaken for that of the absolute strains.

Lastly, there was investigated a new general theorem, which may be stated as follows:—

When we apply a pressure to some point of a flexible system,

the yielding is not necessarily in the direction of the pressure. There is, however, always one direction of coincidence, and there may be three. When there are three, if two of these form a right angle, the third is also perpendicular to both of them.

2. On Vortex Motion. By Professor Sir William Thomson.

(*Abstract.*)

This paper is a sequel to several communications which have already appeared in the Proceedings and Transactions of the Royal Society of Edinburgh.* It commences with an investigation of the circumstances under which a portion of an incompressible frictionless liquid, supposed to extend through all space, or through space wholly or partially bounded by a rigid solid, can be projected so as to continue to move through the surrounding liquid without change of shape; and goes on to investigate vibrations executed by a portion of liquid so projected, and slightly disturbed from the condition that gives uniformity. The greatest and least quantities of energy which a finite liquid mass of any given initial shape and any given initial motion can possess, after any variations of its bounding surface ending in the initial shape, are next investigated; and the theory of the dissipation of energy in a finite or infinite frictionless liquid is deduced. A finite space, filled with incompressible liquid, traversed by a great multitude of parts of itself, each very small in comparison with the average distance of any one of the parts from its nearest neighbour, is next considered, and thus a kinetic theory of gases, without the assumption of elastic atoms, is sketched; also a realisation by vortex atoms of Le Sage's "gravific" fluid consisting of an innumerable multitude of "ultramundane corpuscles."

Towards the vortex theory of the elasticity of liquids and solids, the propagation of waves along a row of vortex columns alternately positive and negative, in a liquid contained between two rigid parallel planes, close enough to give stability to the row of columns, is next investigated.

In conclusion, it is pointed out that the difficulties of forming a complete theory of the elasticity of gases, liquids, and solids, with

* Vortex Atoms. Proceedings, February 1867; Transactions, 1868–1869.

no other ultimate properties of matter than perfect fluidity and incompressibility are noticed, and shown to be, in all probability, only dependent on the weakness of mathematics.

3. On the Ultramundane Corpuscules of Le Sage. By Professor Sir W. Thomson.

(*Abstract.*)

Le Sage, born at Geneva in 1724, devoted the last sixty-three years of a life of eighty to the investigation of a mechanical theory of gravitation. The probable existence of a gravific mechanism is admitted and the importance of the object to which Le Sage devoted his life pointed out, by Newton and Rumford* in the following statements:—

"It is inconceivable that inanimate brute matter should, without "the mediation of something else, which is not material, operate "upon, and affect other matter without mutual contact; as it must "do, if gravitation, in the sense of *Epicurus*, be essential and "inherent in it. And this is the reason why I desired you would "not ascribe innate gravity to me. That gravity should be innate, "inherent, and essential to matter, so that one body may act upon "another at a distance through a *vacuum*, without the mediation "of anything else, by and through which their action and force "may be conveyed from one to another, is to me so great an "absurdity, that I believe no man who has in philosophical "matters a competent faculty of thinking, can ever fall into it.

* On the other hand, by the middle of last century the mathematical naturalists of the Continent, after half a century of resistance to the Newtonian principles (which, both by them and by the English followers of Newton, were commonly supposed to mean the recognition of gravity as a force acting simply at a distance without mediation of intervening matter), had begun to become more "Newtonian" than Newton himself. On the 4th February 1744, Daniel Bernoulli wrote as follows to Euler, "Uebrigens glaube ich, "dass der Aether sowohl *gravis versus solem*, als die Luft versus terram "sey, und kann Ihnen nicht bergen, dass ich über diese Puncte ein völliger "Newtonianer bin, und verwundere ich mich, dass Sie den Principiis "Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht "mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbe-"greiflich ist, erschaffen, so hat er auch können eine attractionem universalem "materiæ imprimiren, wenn gleich solche attractio *supra captum* ist, da "hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

" Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."—*Newton's Third Letter to Bentley, February 25th*, 1692–3.

"Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal Newton enabled to make, merely by the investigation of the laws of its action." *

Le Sage expounds his theory of gravitation, so far as he had advanced it up to the year 1782, in a paper published in the Transactions of the Royal Berlin Academy for that year, under the title "Lucrèce Newtonien." His opening paragraph, entitled, "But de ce mémoire," is as follows:—

"Je me propose de faire voir: que si les premiers Epicuriens avoient eu; sur la Cosmographie des idées aussi saines seulement, que plusieurs de leurs contemporains, qu'ils négligeoient d'écouter; † et sur la Géométrie, une partie des connoissances qui étoient déjà communes alors: ils auroient, tres probablement, découvert sans effort; les Loix de la Gravité universelle, et sa Cause mécanique. *Loix;* dont l'invention et la démonstration, font la plus grande gloire du plus puissant génie qui ait jamais existé: et *Cause*, qui après avoir fait pendant longtems, l'ambition des plus grands Physiciens; fait à présent, le désespoir de leurs sucesseurs. De sorte que, par exemple, les fameuses Régles de *Kepler;* trouvées il y a moins de deux siecles, en partie sur des conjectures gratuites, et en partie aprés d'immenses tâtonnemens; n'auroient été que des corollaires particuliers et inévitables, des lumières générales que ces anciens Philosophes pouvoient puiser (comme en se jouant) dans le méchanisme proprement dit de la Nature. Conclusion; qu'on peut appliquer exactement aussi, aux Loix de *Galilée* sur la chûte des Graves sublunaires; dont la découverte a été plus tardive encore, et plus contestée: joint à ce que, les expériences sur lesquelles cette découverte étoit établie; laissoient dans leurs résultats (nécessairement grossiers),

* An Inquiry concerning the Source of the Heat which is excited by Friction. By Count Rumford.—*Philosophical Transactions*, 1798.

† Vobis (Epicureis) minùs notum est, quemadmodum quidque dicatur. Vestra enim solùm legitis, vestra amatis; cæteros, causâ incognitâ, condemnatis. Cicéron, De natura Deorum, ii. 29.

"une latitude, que les rendoit également compatibles avec plusieurs
"autres hypothèses; qu'aussi, l'on ne manqua pas de lui opposer:
"au lieu que, les conséquences du choc des Atoms; auroient été
"absolument univoques en faveur du seul principe véritable (des
"Accélérations égales en Tempuscules égaux)."

If Le Sage had but excepted Kepler's third law, it must be admitted that his case, as stated above, would have been thoroughly established by the arguments of his "mémoire;" for the epicurean assumption of parallelism adopted to suit the false idea of the earth being flat, prevented the discovery of the law of the inverse square of the distance, which the mathematicians of that day were quite competent to make, if the hypothesis of atoms moving in all directions through space, and rarely coming into collision with one another, had been set before them, with the problem of determining the force with which the impacts would press together two spherical bodies, such as the earth and moon were held to be by some of the contemporary philosophers to whom the epicureant "would not listen." But nothing less than direct observation, proving Kepler's third law,—Galileo's experiment on bodies falling from the tower of Pisa, Boyle's guinea and feather experiment, and Newton's experiment of the vibrations of pendulums composed of different kinds of substance—could give either the idea that gravity is proportional to mass, or prove that it is so to a high degree of accuracy for large bodies and small bodies, and for bodies of different kinds of substance. Le Sage sums up his theory in an appendix to the "Lucrèce Newtonien," part of which translated (literally, except a few sentences which I have paraphrased) is as follows:—

Constitution of Heavy Bodies.

1*st*, Their indivisible particles are cages; for example, empty cubes or octahedrons vacant of matter except along the twelve edges.

2*d*, The diameters of the bars of these cages, supposed increased each by an amount equal to the diameter of one of the gravific corpuscles, are so small relatively to the mutual distance of the parallel bars of each cage, that the terrestrial globe does not intercept even so much as a ten-thousandth part of the corpuscules which offer to traverse it.

3*d*, These diameters are all equal, or if they are unequal, their inequalities sensibly compensate one another [in averages].

Constitution of Gravific Corpuscules.

1*st*, Conformably to the second of the preceding suppositions, their diameters added to that of the bars is so small relatively to the mutual distance of parallel bars of one of the cages, that the weights of the celestial bodies do not differ sensibly from being in proportion to their masses.

2*d*, They are isolated. So that their progressive movements are necessarily rectilinear.

3*d*, They are so sparsely distributed, that is to say, their diameters are so small relatively to their mean mutual distances, that not more than one out of every hundred of them meets another corpuscule during several thousands of years. So that the uniformity of their movements is scarcely ever troubled sensibly.

4*th*, They move along several hundred thousand millions of different directions; in counting for one same direction all those which are [within a definite very small angle of being] parallel to one straight line. The distribution of these straight lines is to be conceived by imagining as many points as one wishes to consider of different directions, scattered over a globe as uniformly as possible, and therefore separated from one another by at least a second of angle; and then imagining a radius of the globe drawn to each of those points.

5*th*, Parallel, then, to each of those directions, let a current or torrent of corpuscules move; but, not to give the stream a greater breadth than is necessary, consider the transverse section of this current to have the same boundary as the orthogonal projection of the visible world on the plane of the section.

6*th*, The different parts of one such current are sensibly equidense; whether we compare, among one another, collateral portions of sensible transverse dimensions, or successive portions of such lengths that their times of passage across a given surface are sensible. And the same is to be said of the different currents compared with one another.

7*th*, The mean velocities, defined in the same manner as I have just defined the densities, are also sensibly equal.

8th, The ratios of these velocities to those of the planets are several million times greater than the ratios of the gravities of the planets towards the sun, to the greatest resistance which secular observations allow us to suppose they experience. For example, [these velocities must be] some hundredfold a greater number of times the velocity of the earth, than the ratio of 190,000* times the gravity of the earth towards the sun, to the greatest resistance which secular observations of the length of the year permit us to suppose that the earth experiences from the celestial masses.

CONCEPTION, *which facilitates the Application of Mathematics to determine the mutual Influence of these Heavy Bodies and these Corpuscules.*

1st, Decompose all heavy bodies into molecules of equal mass, so small that they may be treated as attractive points in respect to theories in which gravity is considered without reference to its cause; that is to say, each must be so small that inequalities of distance and differences of direction between its particles and those of another molecule, conceived as attracting it and being attracted by it, may be neglected. For example, suppose the diameter of the molecule considered to be a hundred thousand times smaller than the distance between two bodies of which the mutual gravitation is examined, which would make its apparent semi-diameter, as seen from the other body, about one second of angle.

2d, For the surfaces of such a molecule, accessible but impermeable to the gravific fluid, substitute one single spherical surface equal to their sum.

3d, Divide those surfaces into facets small enough to allow them to be treated as planes, without sensible error, [&c., &c.]

Remarks.

It is not necessary to be very skilful to deduce from these suppositions all the laws of gravity, both sublunary and universal (and consequently also those of Kepler, &c.), with all the accuracy which observed phenomena have proved those laws. Those laws,

* To render the sentence more easily read, I have substituted this number in place of the following words :—" le nombre de fois que le firmament contient le disque apparent du soleil."

therefore, are inevitable consequences of the supposed constitutions.

2*d*, Although I here present these constitutions crudely and without proof, as if they were gratuitous hypotheses and hazarded fictions, equitable readers will understand that on my own part I have at least some presumptions in their favour (independent of their perfect agreement with so many phenomena), but that the development of my reasons would be too long to find a place in the present statement, which may be regarded as a publication of theorems without their demonstrations.

3*d*, ... There are details upon which I have wished to enter on account of the novelty of the doctrine, and which will readily be supplied by those who study it in a favourable and attentive spirit. If the authors who write on hydro-dynamics, aerostatics, or optics, had to deal with captious readers, doubting the very existence of water, or air, or light, and therefore not adapting themselves to any tacit supposition regarding equivalencies or compensations not expressly mentioned in their treatises, they would be obliged to load their definitions with a vast number of specifications which instructed or indulgent readers do not require of them. One understands "*à demi-mot*" and "*sano sensu*" only familiar propositions towards which one is already favourably inclined.

Some of the details referred to in this concluding sentence of the appendix to his "Lucrèce Newtonien," Le Sage discusses fully in his "Traité de Physique Mécanique," edited by Pierre Prévost, and published in 1818 (Geneva and Paris).

This treatise is divided into four books.

I. "Exposition sommaire du système des corpuscules ultramondains."

II. "Discussion des objections qui peuvent s'élever contre le " système des corpuscules ultramondains."

III. "Des fluides élastiques ou expansifs."

IV. "Application des théories précédentes a certaines affinités."

It is in the first two books that gravity is explained by the impulse of ultramundane corpuscules, and I have no remarks at present to make on the third and fourth books.

From Le Sage's fundamental assumptions, given above as nearly as may be in his own words, it is, as he says himself, easy to deduce the law of the inverse square of the distance, and the law of proportionality of gravity to mass The object of the present note is not to give an exposition of Le Sage's theory, which is sufficiently set forth in the preceding extracts, and discussed in detail in the first two books of his posthumous treatise. I may merely say that inasmuch as the law of the inverse square of the distance, for every distance, however great, would be a perfectly obvious consequence of the assumptions, were the gravific corpuscules infinitely small, and therefore incapable of coming into collision with one another, it may be extended to as great distances as we please, by giving small enough dimensions to the corpuscules relatively to the mean distance of each from its nearest neighbour. The law of masses may be extended to as great masses as those for which observation proves it (for example the mass of Jupiter), by making the diameters of the bars of the supposed cage-atoms constituting heavy bodies, small enough. Thus, for example, there is nothing to prevent us from supposing that not more than one straight line of a million drawn at random towards Jupiter and continued through it, should touch one of the bars. Lastly, as Le Sage proves, the resistance of his gravific fluid to the motion of one of the planets through it, is proportional to the product of the velocity of the planet into the average velocity of the gravific corpuscules; and hence by making the velocities of the corpuscules great enough, and giving them suitably small masses, they may produce the actual forces of gravitation, and not more than the amount of resistance which observation allows us to suppose that the planets experience. It will be a very interesting subject to examine minutely Le Sage's details on these points, and to judge whether or not the additional knowledge gained by observation since his time requires any modification to be made in the estimate which he has given of the possible degrees of permeability of the sun and planets, of the possible proportions of diameters of corpuscules to interstices between them in the "gravific fluid," and of the possible velocities of its component corpuscules. This much is certain, that if hard indivisible atoms are granted at all, his principles are unassailable; and nothing can be said against the probability

of his assumptions. The only imperfection of his theory is tha which is inherent to every supposition of hard, indivisible atoms. They must be perfectly elastic or imperfectly elastic, or perfectly inelastic. Even Newton seems to have admitted as a probable reality hard, indivisible, unalterable atoms, each perfectly inelastic.

Nicolas Fatio is quoted by Le Sage and Prévost, as a friend of Newton, who in 1689 or 1690 had invented a theory of gravity perfectly similar to that of Le Sage, except certain essential points; had described it in a Latin poem not yet printed; and had written, on the 30th March 1694, a letter regarding it, which is to be found in the third volume of the works of Leibnitz, having been communicated for publication to the editor of those works by Le Sage. Redeker, a German physician, is quoted by Le Sage as having expounded a theory of gravity of the same general character, in a Latin dissertation published in 1736, referring to which Prévost says, "Où l'on trouve l'exposé d'un système fort semblable à celui " de Le Sage dans ses traits principaux, mais dépourvu de cette " analyse exacte des phénomènes qui fait le principal mérite de toute " espèce de théorie." Fatio supposed the corpuscules to be elastic, and seems to have shown no reason why their return velocities after collision with mundane matter should be less than their previous velocities, and therefore not to have explained gravity at all. Redeker, we are told by Prévost, had very limited ideas of the permeabilities of great bodies, and therefore failed to explain the law of the proportionality of gravity to mass; "he enunciated this law " very correctly in section 15 of his dissertation; but the manner " in which he explains it shows that he had but little reflected upon " it. Notwithstanding these imperfections, one cannot but recog- " nise in this work an ingenious conception which ought to have " provoked examination on the part of naturalists, of whom many " at that time occupied themselves with the same investigation. " Indeed, there exists a dissertation by Segner on this subject.* " But science took another course, and works of this nature gradu- " ally lost appreciation. Le Sage has never failed on any occasion " to call attention to the system of Redeker,as also to that of Fatio."†

* De Causa gravitatis Redekeriana.

† Le Sage was remarkably scrupulous in giving full information regarding ll who preceded him in the development of any part of his theory.

Le Sage shows that to produce gravitation those of the ultramundane corpuscules which strike the cage-bars of heavy bodies must either stick there or go away with diminished velocities. He supposed the corpuscules to be inelastic (*durs*), and points out that we ought not to suppose them to be permanently lodged in the heavy body (*entassés*), that we must rather suppose them to slip off; but that being inelastic, their average velocities after collision must be less than that which they had before collision.*

That these suppositions imply a gradual diminution of gravity from age to age was carefully pointed out by Le Sage, and referred to as an objection to his theory. Thus he says, ". . . Donc, la durée " de la gravité seroit *finie* aussi, et par conséquent la durée du " monde.

"*Réponse.* Concedo; mais pourvu que cet obstacle ne contribue " pas à faire finir le monde plus promptement qu'il n'auroit fini sans " lui, il doit être considéré comme nul."†

Two suppositions may be made on the general basis of Le Sage's doctrine:—

1*st*, (Which seems to have been Le Sage's belief.) Suppose the whole of mundane matter to be contained within a finite space, and the infinite space round it to be traversed by ultramundane corpuscules; and a small proportion of the corpuscules coming from ultramundane space to suffer collisions with mundane matter, and get away with diminished gravific energy to ultramundane space again. They would never return to the world were it not for collision among themselves and other corpuscules. Le Sage held that such collisions are extremely rare; that each collision, even between the ultramundane corpuscules themselves, destroys some energy;‡ that at a not infinitely remote past time they were set in motion for the purpose of keeping gravitation throughout the world in action for a limited period of time; and that

* Le Sage estimated the velocity after collision to be two-thirds of the velocity before collision.

† Posthumous. "Traité de Physique Mécanique," edited by Pierre Prévost. Geneva and Paris, 1818.

‡ Newton (Optics, Query, 30 Edn. 1721, p. 373) held that two equal and similar atoms, moving with equal velocities in contrary directions, come to rest when they strike one another. Le Sage held the same; and it seems that writers of last century understood this without qualification when they called atoms hard.

both by their mutual collisions, and by collisions with mundane atoms, the whole stock of gravific energy is being gradually reduced, and therefore the intensity of gravity gradually diminishing from age to age.

Or, 2*d*, suppose mundane matter to be spread through all space, but to be much denser within each of an infinitely great number of finite volumes (such as the volume of the earth) than elsewhere. On this supposition, even were there no collisions between the corpuscules themselves, there would be a gradual diminution in their gravific energy through the repeated collisions with mundane matter which each one must in the course of time suffer. The secular diminution of gravity would be more rapid according to this supposition than according to the former, but still might be made as slow as we please by pushing far enough the fundamental assumptions of very small diameters for the cage-bars of the mundane atoms, very great density for their substance, and very small volume and mass, and very great velocity for the ultramundane corpuscules.

The object of the present note is to remark that (even although we were to admit a gradual fading away of gravity, if slow enough), we are forbidden by the modern physical theory of the conservation of energy to assume inelasticity, or anything short of perfect elasticity, in the ultimate molecules, whether of ultramundane or of mundane matter; and, at the same time, to point out that the assumption of diminished exit velocity of ultramundane corpuscules, essential to Le Sage's theory, may be explained for perfectly elastic atoms, consistently both with modern thermodynamics, and with perennial gravity.

If the gravific corpuscules leave the earth or Jupiter with less energy than they had before collision, their effect must be to continually elevate the temperature throughout the whole mass. The energy which must be attributed to the gravific corpuscules is so enormously great, that this elevation of temperature would be sufficient to melt and evaporate any solid, great or small, in a fraction of a second of time. Hence, though outward-bound corpuscules must travel with less velocity, they must carry away the same energy with them as they brought. Suppose, now, the whole energy of the corpuscules approaching a planet to consist of trans-

latory motion: a portion of the energy of each corpuscule which has suffered collision must be supposed to be converted by the collision into vibrations, or vibrations and rotations. To simplify ideas, suppose for a moment the particles to be perfectly smooth elastic globules. Then collision could not generate any rotatory motion; but if the cage-atoms constituting mundane matter be each of them, as we must suppose it to be, of enormously great mass in comparison with one of the ultramundane globules, and if the substance of the latter, though perfectly elastic, be much less rigid than that of the former, each globule that strikes one of the cage-bars must (Thomson & Tait's "Natural Philosophy, § 301), come away with diminished velocity of translation, but with the corresponding deficiency of energy altogether converted into vibration of its own mass. Thus the condition required by Le Sage's theory is fulfilled without violating modern thermo-dynamics; and, according to Le Sage, we might be satisfied not to inquire what becomes of those ultramundane corpuscules which have been in collision either with the cage-bars of mundane matter or with one another; for at present, and during ages to come, these would be merely an inconsiderable minority, the great majority being still fresh with original gravific energy unimpaired by collision. Without entering on the purely metaphysical question,—Is any such supposition satisfactory? I wish to point out how gravific energy may be naturally restored to corpuscules in which it has been impaired by collision.

Clausius has introduced into the kinetic theory of gases the very important consideration of vibrational and rotational energy. He has shown that a multitude of elastic corpuscules moving through void, and occasionally striking one another, must, on the average, have a constant proportion of their whole energy in the form of vibrations and rotations, the other part being purely translational. Even for the simplest case,—that, namely, of smooth elastic globes,—no one has yet calculated by abstract dynamics the ultimate average ratio of the vibrational and rotational, to the translational energy. But Clausius has shown how to deduce it for the corpuscules of any particular gas from the experimental determination of the ratio of its specific heat pressure constant, to its specific heat volume constant.* He found that

* Maxwell's "Elementary Treatise on Heat," chap. xxii. Longman, 1871.

$$\beta = \frac{2}{3}\frac{1}{\gamma - 1},$$

if γ be the ratio of the specific heats, and β the ratio of the whole energy to the translational part of it. For air, the value of γ found by experiment, is 1·408, which makes $\beta = 1·634$. For steam, Maxwell says, on the authority of Rankine, β "may be as much as 2·19, but this is very uncertain." If the molecules of gases are admitted to be elastic corpuscules, the validity of Clausius' principle is undeniable; and it is obvious that the value of the ratio β must depend upon the shape of each molecule, and on the distribution of elastic rigidity through it, if its substance is not homogeneous. Farther, it is clear that the value of β for a set of equal and similar corpuscules will not be the same after collision with molecules different from them in form or in elastic rigidity, as after collision with molecules only of their own kind. All that is necessary to complete Le Sage's theory of gravity in accordance with modern science, is to assume that the ratio of the whole energy of the corpuscules to the translational part of their energy is greater, on the average, after collisions with mundane matter than after inter-collisions of only ultramundane corpuscules. This supposition is neither more nor less questionable than that of Clausius for gases which is now admitted as one of the generally recognised truths of science. The corpuscular theory of gravity is no more difficult in allowance of its fundamental assumptions than the kinetic theory of gases as at present received; and it is more complete, inasmuch as, from fundamental assumptions of an extremely simple character, it explains all the known phenomena of its subject, which cannot be said of the kinetic theory of gases so far as it has hitherto advanced.

Postscript, April 1872.

In the preceding statement I inadvertently omitted to remark that if the constituent atoms are aeolotropic in respect to permeability, crystals would generally have different permeabilities in different directions, and would therefore have different weights according to the direction of their axes relatively to the direction of gravity. No such difference has been discovered, and it is

certain that if there is any it is extremely small. Hence, the constituent atoms, if aeolotropic as to permeability, must be so, but to an exceedingly small degree. Le Sage's second fundamental assumption given above, under the title "*Constitution of* "*Heavy Bodies*," implies sensibly equal permeability in all directions, even in an aeolotropic structure, unless much greater than Jupiter, provided that the atoms are isotropic as to permeability.

A body having different permeabilities in different directions would, if of manageable dimensions, give us a means for drawing energy from the inexhaustible store laid up in the ultramundane corpuscles, thus:—First, turn the body into a position of minimum weight; Secondly, lift it through any height; Thirdly, turn it into a position of maximum weight; Fourthly, let it down to its primitive level. It is easily seen that the first and third of those operations are performed without the expenditure of work; and, on the whole, work is done by gravity in operations 2 and 4. In the corresponding set of operations performed upon a moveable body in the neighbourhood of a fixed magnet, as much work is required for operations 1 and 3 as is gained in operations 2 and 4; the magnetisation of the moveable body being either intrinsic or inductive, or partly intrinsic and partly inductive, and the part of its aeolotropy (if any), which depends on inductive magnetisation, being due either to magne-crystallic quality of its substance, or to its shape.*

4. Note on Spherical Harmonics. By Professor Tait.

While engaged in some quaternion researches with reference to Spherical Harmonics, which I hope soon to lay before the Society, I was led to imagine that some of my results might produce a simplification of the ordinary modes of treating the subject. The following is the result of the attempt. It seems to make the cal-

* "Theory of magnetic induction in crystalline and non-crystalline sub- "stances."—*Phil. Mag.*, March 1851. "Forces experienced by inductively "magnetised ferro-magnetic and dia-magnetic non-crystalline substances." —*Phil. Mag.*, Oct. 1850. "Reciprocal action of dia-magnetic particles."— *Phil. Mag.*, Dec. 1855; all to be found in a collection of reprinted and newly written papers on electrostatics and magnetism, nearly ready for publication, (Macmillan, 1872).

culus somewhat more intelligible to the beginner than the methods employed by O'Brien and Murphy, whose works on the subject are usually read in this country. As I am not writing a treatise, but merely sketching a method, I shall run over the principal elementary propositions only.

1. Let

$$\frac{1}{\rho} = \frac{1}{(1-2h\mu+h^2)^{\frac{1}{2}}} = \Sigma_0^\infty h^i Q_i .$$

This is possible, if h be always taken less than 1; and, as μ is never beyond the limits ± 1, $1, Q_i, -1$ are in order of magnitude, and the series is always convergent.

Hence we may differentiate, and we thus obtain

$$\frac{d}{d\mu}\frac{1}{\rho} = \frac{h}{\rho^3} = \Sigma_0^\infty h^i \frac{dQ_i}{d\mu},$$

and

$$\frac{d}{d\mu}\left((1-\mu^2)\frac{d\frac{1}{\rho}}{d\mu}\right) = \frac{d}{d\mu}\frac{(1-\mu^2)h}{\rho^3} = \rho^{-5}\left\{-2\mu h\rho^2 + 3(1-\mu^2)h^2\right\}$$

$$= \Sigma h^i \frac{d}{d\mu}\left((1-\mu^2)\frac{dQ_i}{d\mu}\right) \quad . \quad . \quad . \quad (1).$$

Also

$$h^2\frac{d}{dh}\frac{1}{\rho} = \frac{\mu h^2 - h^3}{\rho^3} = \Sigma . ih^{i+1} Q_i,$$

and

$$\frac{d}{dh}\left(h^2\frac{d}{dh}\frac{1}{\rho}\right) = \rho^{-5}\left\{(2\mu h - 3h^2)\rho^2 + 3(\mu-h)^2 h^2\right\}$$

$$= \Sigma . i(i+1)h^i Q_i \quad . \quad . \quad . \quad (2).$$

The sum of the multipliers of ρ^{-5} in (1) and (2) is obviously zero. Thus we have the equation for Q_i

$$i(i+1)Q_i + \frac{d}{d\mu}\left((1-\mu^2)\frac{dQ_i}{d\mu}\right) = 0 \quad . \quad (3).$$

2. From this equation, by differentiation $s-1$ times with respect to μ, we have

$$i(i+1)\frac{d^{s-1}Q_i}{d\mu^{s-1}} + (1-\mu^2)\frac{d^{s+1}Q_i}{d\mu^{s+1}} - 2s\mu\frac{d^sQ_i}{d\mu^s} - s(s-1)\frac{d^{s-1}Q_i}{d\mu^{s-1}} = 0,$$

or,

$$\Big(i(i+1)-s(s-1)\Big)(1-\mu^2)^{s-1}\frac{d^{s-1}Q_i}{d\mu^{s-1}}+\frac{d}{d\mu}\left((1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\right)=0 \quad (4).$$

3. Let Q_j be any one of the values of Q above defined, then

$$\int(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^sQ_j}{d\mu^s}d\mu=(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^{s-1}Q_j}{d\mu^{s-1}}-\int d\mu\frac{d^{s-1}Q_j}{d\mu^{s-1}}\frac{d}{d\mu}\left((1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\right)$$

$$=(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^{s-1}Q_j}{d\mu^{s-1}}+(i+s)(i-s+1)\int(1-\mu^2)^{s-1}\frac{d^{s-1}Q_i}{d\mu^{s-1}}\frac{d^{s-1}Q_j}{d\mu^{s-1}}d\mu.$$

Hence, integrating between the limits ∓ 1 of μ, we have

$$\int_{+1}^{-1}(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^sQ_j}{d\mu^s}d\mu=(i+s)(i-s+1)\int_{+1}^{-1}(1-\mu^2)^{s-1}\frac{d^{s-1}Q_i}{d\mu^{s-1}}\frac{d^{s-1}Q_j}{d\mu^{s-1}}d\mu \quad (5).$$

Applying the reduction s times, we evidently obtain

$$\int_{+1}^{-1}(1-\mu^2)^s\frac{d^sQ_i}{d\mu^s}\frac{d^sQ_j}{d\mu^s}d\mu = \frac{\lfloor i+s}{\lfloor i-s}\int_{+1}^{-1}Q_iQ_j\,d\mu \qquad (6).$$

4. To find the value of the integral on the right, note that Q_iQ_j is the co-efficient of $h^ih'^j$ in the expansion of

$$\frac{1}{(1-2\mu h+h^2)^{\frac{1}{2}}(1-2\mu h'+h'^2)^{\frac{1}{2}}}$$

Now

$$\int_{+1}^{-1}\frac{d\mu}{\sqrt{(1+h^2-2h\mu)(1+h'^2-2h'\mu)}}$$

$$=\frac{1}{\sqrt{hh'}}\log.\frac{\sqrt{\frac{1+h^2}{2h}-1}+\sqrt{\frac{1+h'^2}{2h'}-1}}{\sqrt{\frac{1+h^2}{2h}+1}+\sqrt{\frac{1+h'^2}{2h'}+1}}$$

$$=\frac{1}{\sqrt{hh'}}\log.\frac{\sqrt{h'}(1-h)+\sqrt{h}(1-h')}{\sqrt{h'}(1+h)+\sqrt{h}(1+h')}$$

$$=\frac{1}{\sqrt{hh'}}\log.\frac{1-\sqrt{hh'}}{1+\sqrt{hh'}}$$

$$= -2\,\Sigma_0^\infty \frac{(hh')^i}{2i+1}.$$

In this there is no term in which the powers of h and h' are different, hence we have

$$\int_{+1}^{-1} Q_i\, Q_j\, d\mu = 0 \quad . \quad . \quad . \quad (7).$$

in all cases unless $j=i$. In this special case we have

$$\int_{+1}^{-1} Q_i^2\, d\mu = \frac{2}{2i+1} \quad . \quad . \quad . \quad (8.)$$

Hence the left hand member of (6) vanishes unless $j = i$, and in that case we have

$$\int_{+1}^{-1} (1-\mu^2)^s \left(\frac{d^sQ_i}{d\mu^s}\right)^2 d\mu = \frac{2}{2i+1}\,\frac{\lfloor i+s}{\lfloor i-s}. \quad . \quad (9).$$

We might have proved (7) from (6) by exchanging i and j, and showing that unless $i = j$, we cannot have

$$\frac{\lfloor i+s}{\lfloor i-s} = \frac{\lfloor j+s}{\lfloor j-s}.$$

5. The equation (3), which is satisfied by Q_i, is a mere particular case of the general equation of surface harmonics—

$$i(i+1)\,S_i + \frac{1}{1-\mu^2}\,\frac{d^2S_i}{d\phi^2} + \frac{d}{d\mu}\left((1-\mu^2)\,\frac{dS_i}{d\mu}\right) = 0 \quad (10).$$

which may be obtained by putting $V_i = S_i$ in the ordinary equation of Laplace—

$$r\,\frac{d^2(rV_i)}{dr^2} + \frac{1}{1-\mu^2}\,\frac{d^2V_i}{d\phi^2} + \frac{d}{d\mu}\left((1-\mu^2)\frac{dV_i}{d\mu}\right) = 0,$$

after differentiating the first term. That differentiation gives, in fact,

$$r\,\frac{d}{dr}\left(V_i + r\,\frac{dV_i}{dr}\right) = (i+1)\,r\,\frac{dV_i}{dr} = i(i+1)\,V_i.$$

From equation (10) we may prove, as usual, by multiplying by S_j and integrating over the unit sphere, that

$$i(i+1)\int d\sigma S_i S_j = j(j+1)\int d\sigma S_i S_j,$$

the expression for either being symmetrical in i and j, so that the integral vanishes unless $i=j$: or, if negative values be admitted, unless $i+j+1=0$.

6. We must now express S_i in terms of ϕ and Q_i. Let, then,

$$S_i = \Sigma_0 A_s \cos.(s\phi + a_s)\Theta_i^{(s)} \quad . \quad . \quad . \quad (11).$$

where A_s, a_s are virtually $2i+1$ arbitrary constants. Substituting this value in (10), and supposing all the coefficients A to vanish except A_s, we have

$$\left(i(i+1) - \frac{s^2}{1-\mu^2}\right)\Theta_i^{(s)} + \frac{d}{d\mu}\left((1-\mu^2)\frac{d\Theta_i^{(s)}}{d\mu}\right) = 0 \quad . \quad (12).$$

This equation is materially simplified by assuming (as is suggested by (6) and (9))

$$\Theta_i^{(s)} = (1-\mu^2)^{\frac{s}{2}}\theta_s \quad . \quad . \quad . \quad . \quad (13),$$

for with this substitution it becomes, by a process the same as that of section 2 above,

$$\left(i(i+1) - s(s+1)\right)(1-\mu^2)^s\theta_s + \frac{d}{d\mu}\left((1-\mu^2)^{s+1}\frac{d\theta_s}{d\mu}\right) = 0.$$

But, by (4), putting $s+1$ for s,

$$\left(i(i+1) - s(s+1)\right)(1-\mu^2)^s\frac{d^sQ_i}{d\mu} + \frac{d}{d\mu}\left((1-\mu^2)^{s+1}\frac{d^{s+1}Q_i}{d\mu^{s+1}}\right) = 0.$$

Comparing these equations, and remembering that all the permissible arbitrary constants have already been introduced into the solution of (10), we have

$$\theta_s = \frac{d^sQ_i}{d\mu^s}.$$

Hence, finally,

$$S_i = \Sigma_0^i A_s \cos.(s\phi + a_s)(1-\mu^2)^{\frac{s}{2}}\frac{d^sQ_i}{d\mu^s} \quad . \quad . \quad (14.)$$

7. We may now easily find the value of

$$\int S_i S_j \, d\sigma$$

taken over the whole spherical surface. For

$$\int (\quad) \, d\sigma = \int_0^{2\pi} \int_{+1}^{-1} (\quad) d\varphi \, d\mu :$$

and

$$\int_0^{2\pi} d\varphi \cos.(s\varphi + a_s) \cos.(s'\varphi + a_{s'})$$

vanishes unless s and s' be equal, in which case its value is π. Hence, attending to § 4, and to (14),

$$\left. \begin{aligned} \int S S \, d\sigma &= 0 , \\ \text{and} \qquad \int S_i^2 \, d\sigma &= \frac{2\pi}{2i+1} \Sigma_0^i A_s^2 \frac{|\underline{i+s}}{|\underline{i-s}} \end{aligned} \right\} \quad . \quad . \quad . \quad (15).$$

8. Another curious expression for $\Theta_i^{(s)}$ is given by (4). For that equation gives

$$(1-\mu^2)_s \frac{d^s Q_i}{d\mu^s} = - \left(i(i+1) - s(s-1) \right) \int (1-\mu^2)^{s-1} \frac{d^{s-1} Q_i}{d\mu^{s-1}} d\mu$$

$$= + \{ i(i+1) - s(s-1) \} \{ i(i+1) - (s-1)(s-2) \} \iint (1-\mu^2)^{s-2} \frac{d^{s-2} Q_i}{d\mu^{s-2}} d\mu$$

$$= (-)^s \frac{|\underline{i+s}}{|\underline{i-s}} \int^{(s)} Q_i \, d\mu^s \quad . \quad . \quad . \quad . \quad (16).$$

Hence

$$\Theta_i^{(s)} = (-)^s \frac{|\underline{i+s}}{|\underline{i-s}} (1-\mu^2)^{-\frac{s}{2}} \left(\int d\mu \right)^s Q_i \quad . \quad (17).$$

10. Let

$$\sqrt{1+2\mu h+h^2} = 1 + hy \quad . \quad . \quad . \quad (19),$$

where y is a function of h and μ, never beyond the limits $+1$ and -1.

Then

$$\frac{h}{\sqrt{1+2\mu h+h^2}} = h \frac{dy}{d\mu} .$$

But the first equation gives, at sight,

$$y = \mu + h \frac{1-y^2}{2} \quad . \quad . \quad . \quad (20).$$

whence,

$$y = \mu + h\,\frac{1-\mu^2}{2} + \frac{h^2}{1\cdot 2}\,\frac{d}{d\mu}\left(\frac{1-\mu^2}{2}\right)^2 + \text{\&c.},$$

and therefore,

$$\frac{1}{\sqrt{1+2\mu h+h^2}} = \frac{dy}{d\mu} = 1 + h\,\frac{d}{d\mu}\left(\frac{1-\mu^2}{2}\right) + \frac{h^2}{1\cdot 2}\,\frac{d^2}{d\mu^2}\left(\frac{1-\mu^2}{2}\right)^2 + \text{\&c.},$$

which shows that

$$Q_i = (-)^i\left(\frac{d}{d\mu}\right)^i\left(\frac{1-\mu^2}{2}\right)^i \quad . \quad . \quad (21),$$

and suggests obvious simplifications of preceding results, *e.g.*,

$$\theta_s = (-)^i\left(\frac{d}{d\mu}\right)^{i+s}\left(\frac{1-\mu^2}{2}\right)^i = (\text{by § 8})\,(-)^{i+s}(1-\mu^2)^{-s}\,\frac{|\underline{i+s}}{|\underline{i-s}}\left(\frac{d}{d\mu}\right)^{i-s}\left(\frac{1-\mu^2}{2}\right)^i,$$

&c., &c.,

11. The complete integral of

$$i(i+1)Q_i + \frac{d}{d\mu}\left((1-\mu^2)\frac{dQ_i}{d\mu}\right) = 0 \quad . \quad (3)$$

may easily be found, since a particular integral is known. Let it be MQ_i, where M is a function of μ. Then (3) gives at once

$$\left(-2\mu Q_i + 2\,(1-\mu^2)\frac{dQ_i}{d\mu}\right)\frac{dM}{d\mu} + (1-\mu^2)\,Q_i\,\frac{d^2M}{d\mu^2} = 0,$$

or,

$$\frac{-2\mu}{1-\mu^2} + \frac{2}{Q_i}\frac{dQ_i}{d\mu} + \frac{1}{\frac{dM}{d\mu}}\,\frac{d^2M}{d\mu^2} = 0,$$

whence

$$\frac{dM}{d\mu} = \frac{C}{(1-\mu^2)Q_i^2}.$$

Thus the complete integral is

$$CQ_i\int\frac{d\mu}{(1-\mu^2)Q_i^2} \quad . \quad . \quad . \quad (22).$$

12. Let us now suppose

$$S_i = P_iQ_i \quad . \quad . \quad . \quad (23),$$

where Q_i is as in § 1, and P_i is a function of μ and ϕ. The equation (10) becomes successively

$$\frac{d}{d\mu}\left((1-\mu^2)\frac{d\,(P_iQ_i)}{d\mu}\right)+\frac{1}{1-\mu^2}\frac{d^2(P_iQ_i)}{d\varphi^2}+i(i+1)P_iQ_i=0,$$

$$-2\mu Q_i\frac{dP_i}{d\mu}+(1-\mu^2)\left(2\frac{dP_i}{d\mu}\frac{dQ_i}{d\mu}+Q_i\frac{d^2P}{d\mu^2}\right)+\frac{Q_i}{1-\mu^2}\frac{d^2P_i}{d\varphi^2}=0,$$

$$(1-\mu^2)Q_i\frac{dP_i}{d\mu}\left(-\frac{2\mu}{1-\mu^2}+\frac{2}{Q_i}\frac{dQ_i}{d\mu}+\frac{1}{\frac{dP_i}{d\mu}}\frac{d^2P_i}{d\mu^2}\right)+\frac{Q_i}{1-\mu^2}\frac{d^2P_i}{d\varphi^2}=0,$$

$$(1-\mu^2)Q_i\frac{dP_i}{d\mu}\frac{\frac{d}{d\mu}\left((1-\mu^2)Q_i^2\frac{dP_i}{d\mu}\right)}{(1-\mu^2)Q_i^2\frac{dP_i}{d\mu}}+\frac{Q_i}{1-\mu^2}\frac{d^2P_i}{d\varphi^2}=0,$$

and, finally,

$$(1-\mu^2)Q_i^2\frac{d}{d\mu}\left((1-\mu^2)Q_i^2\frac{dP_i}{d\mu}\right)+Q_i^4\frac{d^2P_i}{d\varphi^2}=0.$$

If we put, for a moment,

$$\frac{d\mu}{(1-\mu^2)Q_i^2}=d\nu \quad \text{(which has a real meaning, see § 11),}$$

and suppose Q_i to be expressed in terms of ν instead of μ, calling it q_i, the equation may be written

$$\frac{d^2P_i}{d\nu^2}+q_i^4\frac{d^2P_i}{d\varphi^2}=0 \quad . \quad . \quad . \quad (24).$$

Hence it appears at once that P_i cannot contain φ except in the form of factors, such as cos. $s\varphi$, sin. $s\varphi$, in the several terms of which (as an integral of a linear equation) it must be composed. Hence, as before,

$$P_i = \Sigma_0^i A_s \overset{(s)}{\Theta_i} \cos.(s\varphi+a),$$

and, keeping to one value of s,

$$\frac{d^2\Theta_i^{(s)}}{d\nu^2} - s^2q_i^4 = 0.$$

5. Laboratory Notes: On Thermo-Electricity. By Professor Tait.

For some time back I have been endeavouring to prove, by experiment, through great ranges of temperature, the result announced by me in December last, viz., that the electro-motive force of a thermo-electric circuit is in general, unless the temperature be very high, a parabolic function of the absolute temperature of either junction, that of the other being maintained constant.

For moderate ranges of temperature the experiment presents little difficulty; but, when mercurial thermometers cannot be employed, a modification of the experimental method must be made. I have employed in succession several such modifications, of which the following are the chief:—

The simplest of all is to dispense altogether with thermometers, and to employ two thermo-electric circuits, whose hot and whose cold junctions are immersed in the same vessels; and to plot the curve whose abscissæ and ordinates are simultaneous readings of the electro-motive forces in the two circuits. In every case I have tried the curve thus obtained is almost accurately a parabola, most of the few deviations yet observed being in the case of silver and other metals at temperatures not very much below their melting points—under circumstances, in fact, in which we should naturally expect that the law would no longer hold. There are, also, cases in which the whole electro-motive force is so small, even for very large differences of temperature, that very much more delicate apparatus would be required for their proper investigation. And there are cases in which the neutral point is so far off that for moderate ranges of temperature the curves obtained are sensibly straight lines. I intend to examine these cases with care—the former by using more delicate galvanometers; the latter, by employing metals which are practically infusible. The difficulty of obtaining wires of such metals has been the chief one I have had to face.

If we assume the experimental curve to be a parabola, then it is easily seen (*Proc.* May 29, 1871) that in each circuit the electro-motive force must be a parabolic function of some function of the absolute temperatures of the junctions. And, as in the iron-silver,

iron-zinc, iron copper, iron-cadmium, &c., circuits, this function has been proved to be simply the absolute temperature itself (at least within the range of mercury thermometers), it is probable that such is the general law, at least for ranges of temperature short of those which materially alter the molecular structure of the metals employed.

The second method consisted in employing two pairs of circuits, all four hot junctions being in the same heated substance, and all four cold junctions kept at a common temperature. The members of each pair acted on a differential galvanometer (as explained in *Proc.* Dec. 19, 1870) in such a way as to eliminate the term containing the square of the absolute temperature. In this case the readings of the galvanometers should be simply proportional to one another, and likewise to the differences of absolute temperature of the junctions. The method is exact in theory, but by no means easy in practice, especially with the very limited number of metals capable of resisting a high temperature which I could manage to obtain. That a very exact and useful thermometric arrangement can be made on this principle admits of no doubt, when we examine the results of the experiments.

The third method consisted in assuming the parabolic law, and the following consequence of it which follows directly by the use of Thomson's general formulæ. These may easily be reproduced as follows:—Suppose a sliding ring or clip to be passed round the wires, so as to press together points of the wires which are at the same temperature, t. Its effects are known by experiment to be nil, whatever be its material. Let it be slid along so that the temperature of what is now effectively the hot junction becomes $t + \delta t$, then the two laws of thermodynamics give, respectively,

$$\delta E = J\left(\delta\Pi + (\sigma_1 - \sigma_2)\,\delta t\right),$$

and

$$0 = \delta\frac{\Pi}{t} + \frac{\sigma_1 - \sigma_2}{t}\,\delta t\,.$$

Here E is the electromotive force, Π the Peltier effect at a junction at temperature t, and σ_1, σ_2, are the specific heats of electricity in the two metals.

Hence

$$\delta E = J\left(\delta\Pi - t\delta\frac{\Pi}{t}\right) = J\frac{\Pi}{t}\delta t .$$

Introducing the hypothesis, obtained from considerations of Dissipation of Energy, (*Proc.* Dec. 19, 1870) that

$$\sigma_1 = k_a t, \quad \sigma_2 = k_b t,$$

we have

$$J\frac{\Pi}{t} = \frac{dE}{dt} = (k_a - k_b)\ (T_{ab} - t)\,,$$

where T_{ab} is the well-known "neutral point."

Also

$$E = (k_a - k_b)\ (t - t_1)\left(T_{ab} - \frac{t + t_1}{2}\right),$$

since it vanishes for $t = t_1$, the temperature of the cold junction. Now, if the neutral point be between such limits as 0° C. and 300° C., the exact determination of it is an easy matter; and this exact knowledge of it greatly facilitates the determination of $\frac{dE}{dt}$, which cannot be *very* accurately found by drawing a tangent to the plotted curve. For if one junction be at t, the other at T_{ab}, we have

$$E_T = \tfrac{1}{2}(k_a - k_b)\ (T_{ab} - t)^2$$

E_T and $T_{ab} - t$ are easily measured on the experimental curve, and thus $k_a - k_b$ is found. The following values have thus been (roughly) calculated from observations. Where the neutral point was not reached, it is put in brackets. The unit for $k_a - k_b$ is 3 or 4 *per cent.* less than $\frac{2}{10^5}$ of the electromotive force of a good Grove's cell.

	T	$k_a - k_b$		T	$k_a - k_b$
Fe – Cu (bad)	265 C.	– 0·00147	Fe – Al	(387) C.	– 0·00105
,, – Cu (good)	260	– ·00145	,, – Arg.	(1357)	– ·00045
,, – Cd	159	– ·00209	Cu (bad) – Cd	– (23)	– ·00081
,, – Zn	199	– ·00189	,, – Zn	– (146)	– ·00048
,, – Ag	235	– ·00151	,, – Ag	– (687)	– ·00006
,, – Pb	(357)	– ·00112	,, (good) – Pb	– (213)	+ ·00016
,, – Brass	(318)	– ·00127	Pb – Cd	– (74)	– ·00096
,, – Pt	(519)	– ·00063	,, – Pd	– (188)	+ ·00080
,, – Sn	(416)	– ·00094	,, – Zn	– (78)	– ·00060
,, – Pd	(1908)	– ·00029	,, – Ag	– (262)	– ·00026

Now, it is an immediate consequence of the second law of thermodynamics that, as Peltier effects are reversible with the direction of the current, and are the *only* sensible thermal effects when a very feeble current passes through a thermo-electric circuit, all of whose parts are at one temperature, we must have

$$\Sigma \frac{\Pi}{t} = 0 ,$$

or, assuming the parabolic law,

$$\Sigma . (k_a - k_b) (T_{ab} - t) = 0 .$$

This holds for any number of separate materials in the conductor. As t is the same throughout, the terms involving it evidently vanish identically; but there remains the equation

$$\Sigma . (k_a - k_b) T_{ab} = 0,$$

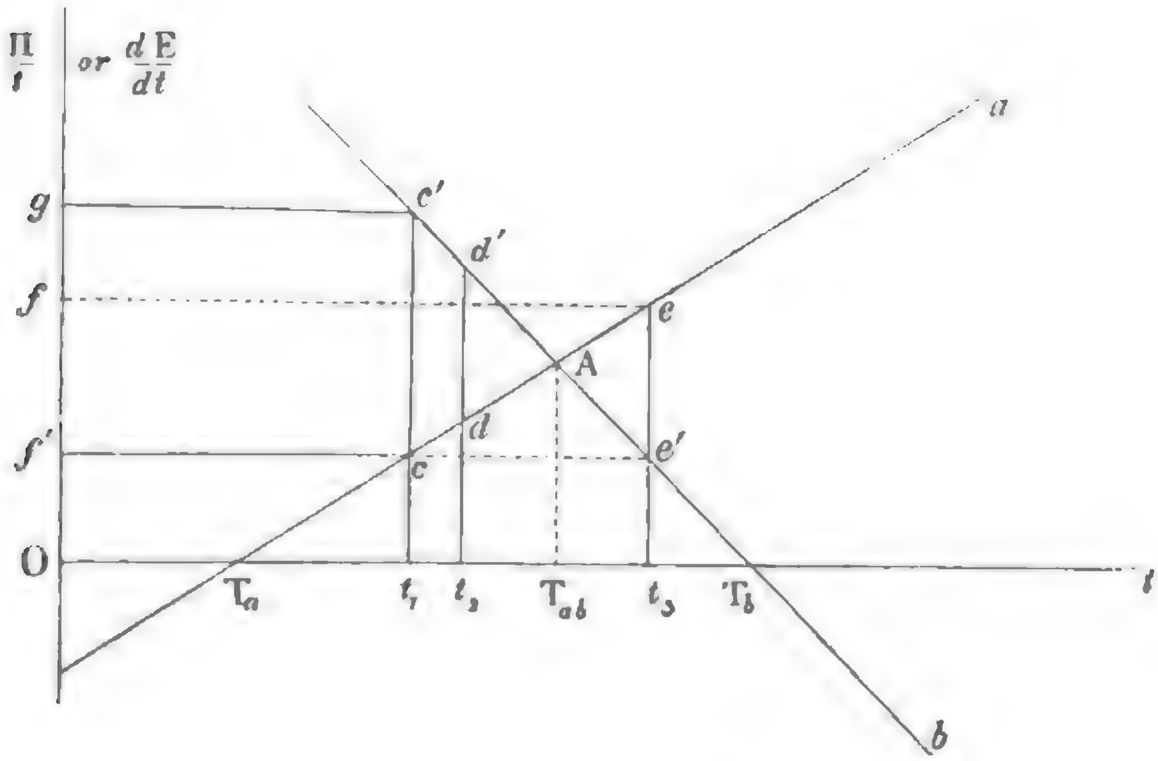
establishing a relation between the specific heats of electricity in a number of metals and the absolute temperatures of the neutral points of each junction of two of them. Other relations may be obtained by altering the order of the metals if there be more than three—but they are all virtually contained in the formula for three, which we write at full length,

$$(k_a - k_b) T_{ab} + (k_b - k_c) T_{bc} + (k_c - k_a) T_{ca} = 0.$$

From the direct experiments of Le Roux on "l'Effet Thomson," as he calls it, it appears that k is null in lead.* At all events, since Thomson showed that it has opposite signs in iron and copper, we may imagine a substance for which $k = 0$. We may now construct an improved "*Thermo-electric diagram*" to represent these relations numerically, employing the line for this substance as our axis of absolute temperatures; while the ordinates perpendicular to it give, for this substance employed with any other in a circuit of two metals, the values of $\frac{\Pi}{t}$, or $\frac{dE}{dt}$, or (what comes to the same thing) the electro-motive force of a circuit whose junctions are both very nearly at t, but have a small constant temperature difference. This quantity corresponds with what has been called the *thermo-electric power* of the circuit.

* Annales de Chimie, 1867, vol. x. p. 277.

The two oblique *straight* lines in the diagram belong to the metals a, b, respectively. The tangents of their inclination to the horizontal axis (the line of the supposed metal for which $k=0$) are k_a, k_b—and they cut it at the points T_a, T_b, where they are neutral to it; cutting one another at a point A whose abscissa is their own neutral point T_{ab}. The only change which would be introduced, by taking



as horizontal axis the line corresponding to a metal for which k does not vanish, would be a dislocation of the diagram, by a simple shear. This follows at once from the equation of one of the lines—

$$y = k_a (x - T_a).$$

The diagram gives the Peltier effect at the junction of a and b for any temperature t_1, by drawing the ordinate at t_1, and completing a rectangle $cc'gf'$ on the part intercepted, its opposite end being at absolute zero. The area of this rectangle is to be taken positively or negatively according as the corner corresponding to a is nearer to, or further from, the horizontal axis than that corresponding to b, the current being supposed to pass from a to b.

The electro-motive force in a circuit of the two metals, a and b, with its junctions at t_1 and t_2 respectively, is found by drawing ordinates at these temperatures, so as to cut off triangular spaces Acc', Add', whose vertices are at the neutral point. The difference

of the areas of these spaces, *cdd'c'*, is proportional to the electromotive force. When the higher temperature, t_3, is above the neutral point, the electromotive force is the difference of the areas Acc', Aee'. The case above mentioned, in which, by a differential galvanometer, we get rid of the terms in t^2, is obviously a process for making the curves of two separate complex arrangements into parallel straight lines.

In conclusion, I may give a few instances of the comparison of results of calculation of the neutral point of two metals from their observed neutral points, and differences of k, as regards iron, with calculation of the same neutral point from the portion of the curve (assumed to be a parabola) which expresses their electro-motive force within ranges of temperature where mercurial thermometers can be applied.

Thus with Fe, Cd, Pb, we have from the iron circuits 0·00112 − 0·00209 = − 0·00097, while the direct experiment with Cd, Pb gave − 0·00096.

The neutral point, as calculated from the data for the iron circuits is − 69° C., while the calculation from direct experiment gives − 74° C.

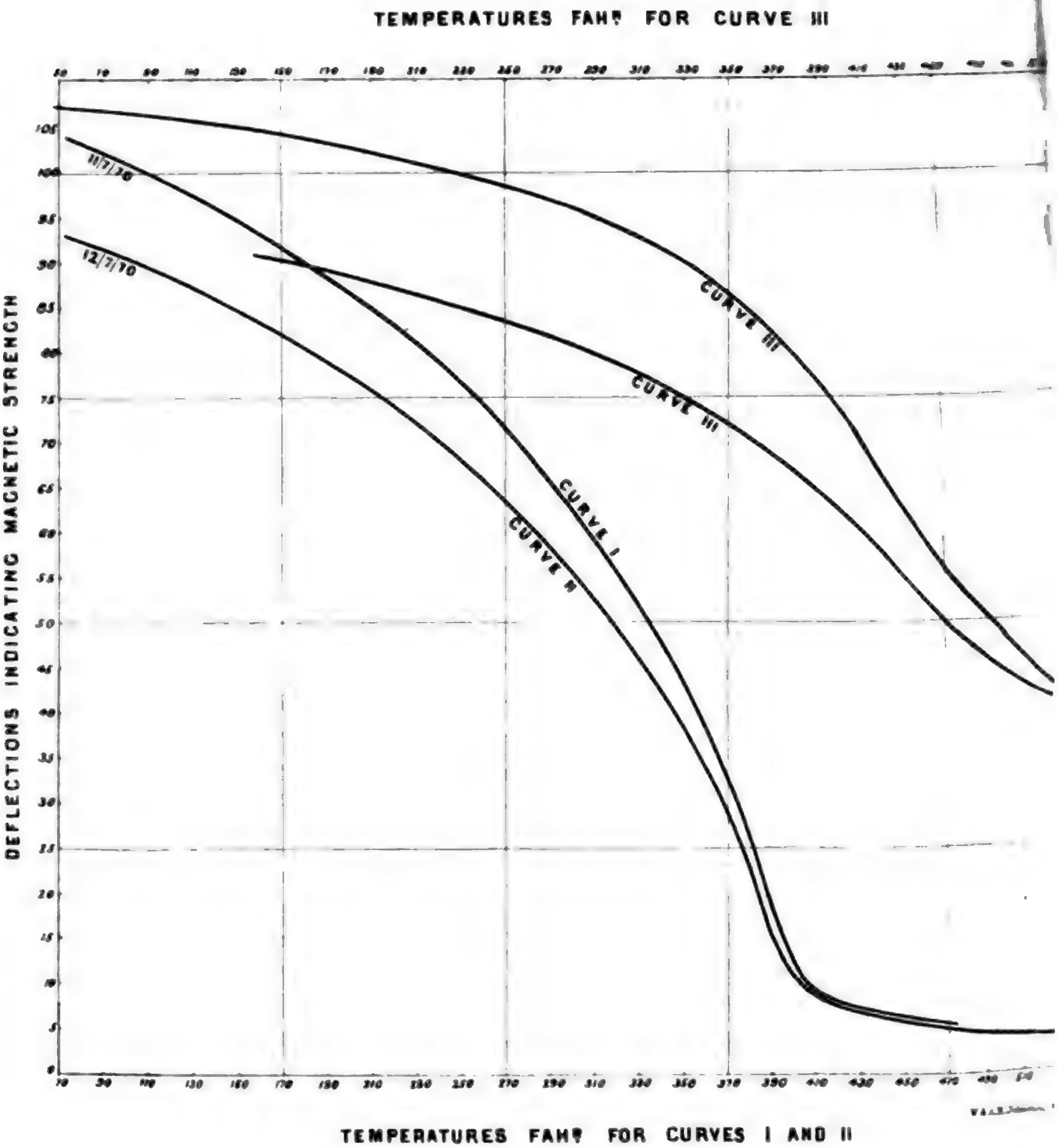
When the quantities to be found are very small, as for instance in the case Ag − Cu, we cannot expect to get a good approximation by introducing a third metal. In fact, introducing Fe we find indirectly 0·00147 − 0·00151 = − 0·00004, while the direct determination gives − 0·00006.

Again with Zn and Cu, indirectly we get

	− 0·00042 and − 144° C.
Directly	− 0·00048 and − 146° C.

Several of the other groups give results as closely agreeing with one another as these, others are considerably out.

The numerical determinations above are founded entirely on a series of experiments made for me by Messrs J. Murray and R. M. Morrison. Mr W. Durham is at present engaged in determining the electromotive force of contact of wires of the same metal at different temperatures, with the view of inquiring into its relation to ordinary thermo-electric phenomena which appears to be suggested by some of the formulæ above given.


TEMPERATURES FAHR. FOR CURVE III
DEFLECTIONS INDICATING MAGNETIC STRENGTH
11/7/70
12/7/70
CURVE III
CURVE III
CURVE I
CURVE II
TEMPERATURES FAHR. FOR CURVES I AND II

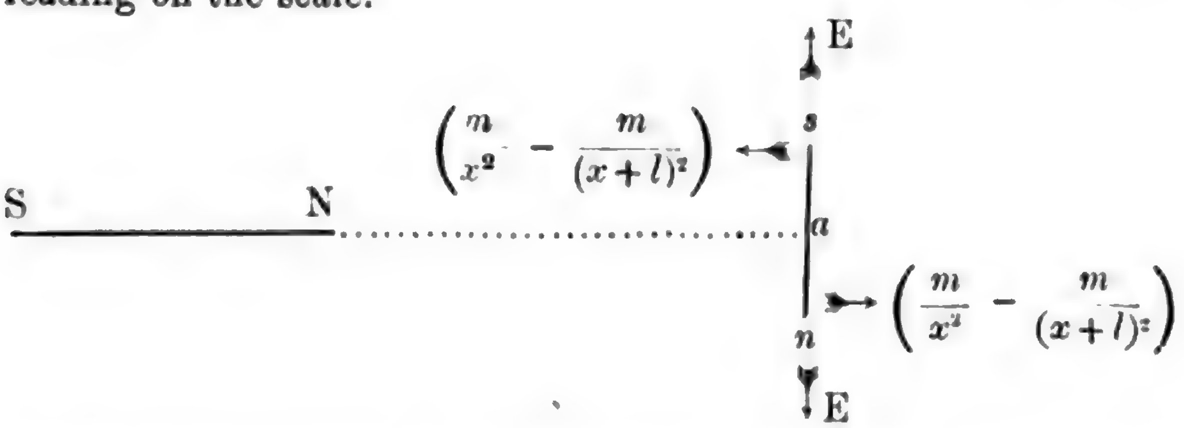
Monday, 15th January 1872.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Relation of Magnetism to Temperature. (With a Plate.) By D. H. Marshall, Esq., M.A., Assistant to the Professor of Natural Philosophy. Communicated by Professor Tait.

The following was the arrangement adopted in these experiments:—A large magnet was put into a copper pot containing oil, which was heated up by a brass Bunsen, and its temperature determined by a mercurial thermometer immersed in it. The magnet was set magnetically east and west, and placed so as to act with equal force on the poles of a small magnet, whose centre was in the prolongation of its axis. This small magnet was cemented to the back of a small concave mirror, suspended by a single silk fibre, and placed in a glass case to guard it against currents of air. The deflections of the small magnet were measured exactly as in the reflecting galvanometer, and since from the nature of the arrangement, the absolute magnetism in the large magnet is directly as the tangent of the angle of deflection of the small one, its amount for any temperature was immediately measured by the reading on the scale.



N S, the poles of the fixed magnet, m its absolute magnetism. N $a=x$, S N $=l$. The couples indicated are those produced by the large magnet, and the earth's magnetism, E, on the small magnet.

For any deflection θ, if the length of the small magnet be negligible compared with x, we have

$$E \sin. \theta = m \left(\frac{1}{x^2} - \frac{1}{(x+l)^2}\right) \cos. \theta \quad \therefore m \propto \tan. \theta .$$

[This simple formula holds, of course, however complex be the distribution of magnetism in the large magnet, provided the *relative* intensities of magnetization at different parts, and their directions, remain unchanged by heating.]

Disturbances were experienced in the form of thermo-electric currents in the pot and brass ring supporting it (these acted against one another), but their effects were rendered insignificant by removing the flame, and allowing the whole to come to a uniform temperature before reading. The direction of these currents, and therefore that of the disturbance to which they gave rise, could be reversed by changing the position of the flame relatively to the pot; but a smaller disturbance of a more unaccountable nature presented itself during the heating of the pot, which did not depend on the position of the flame, and could not be got rid of. This latter disturbance, which increased with the temperature, resulted in a gradual alteration of zero, and in consequence the deflections, corresponding at least to the higher temperatures in the curves and all the ordinates of the lower part of curve III., are somewhat less than they ought strictly to be.

Curves I., II., and the upper part of curve III., show how the absolute magnetism diminishes as the temperature of the magnet increases; the lower part of curve III. shows how the magnet regains its power when the temperature again falls, and it is seen at once from it that, when the magnet is allowed to cool after being heated, the deflection corresponding to a given temperature is less than that obtained at the same temperature when the magnet is being heated, thus indicating a loss of magnetic power, and the difference of the two deflections is greater the lower the temperature. It is principally on this account also that the curves I. and II. do not coincide, for the experiments were performed on successive days, and it was found that that magnet took about two days after such heating to acquire its original power. The magnet used

in the experiments represented by curves I. and II. was not the same as the one used in that represented by curve III.; the latter was a thin, very hard steel magnet, the former thicker and softer, and it may be seen from the curves that the hard steel parted with its magnetism less readily than the soft.

From these experiments it follows also that $\frac{dm}{dt}$, or the rate of change of magnetism with temperature, is not constant for each temperature, but depends in some way or other upon the state of the magnet.

When the above experiment was repeated with an electro-magnet in the copper pot instead of a permanent magnet, it was found that while at a temperature of 500° F. the power of the permanent magnet is very much lessened, that of the electro-magnet, provided the intensity of the current remain constant, is unaltered.

2. Note on a Singular Property of the Retina. By Professor Tait.

While suffering some of the annoyances seemingly inseparable from re-vaccination at too advanced an age, I was led to the curious observation presently to be described. I was unable to sleep, except in "short and far between" dozes, from which I woke with a sudden start, my eyelids opening fully. I found by trial that this state of things became somewhat less intolerable when I lay on my back, with my head considerably elevated. In this position I directly faced a gas jet, burning not very brightly, placed close to a whitish wall, and surrounded by a ground glass shade, through which the flame could be prominently perceived. The portions of the wall surrounding the burner were moderately illuminated, and hyperbolic portions above and below somewhat more strongly. I observed, on waking, that the gas flame seemed for a second or two to be surrounded by a dark crimson ground, though itself apparently unchanged in colour. Gradually, after the lapse of, at the very utmost, a couple of seconds, everything resumed its normal appearance. As this phenomenon appeared not only to be worthy of observation in itself, but to furnish me with something definite to reflect upon, which is far the best alleviation of annoy-

ances similar to those from which I was suffering, I determined to watch it, transitory as it was, feeling assured that I should have many opportunities of observing it. After two nights' practice, I found myself getting dangerously skilful in reproducing it, and decided, somewhat reluctantly, that I must give it up. What I observed, however, has already been almost completely described as having been seen on the very first occasion. I endeavoured to prepare myself to note any possible difference of colour in the crimson field, as distinguished from mere difference of intensity of illumination, and I could perceive none. I also endeavoured to ascertain the nature of the transition from this state to the normal one, but this was so exceedingly rapid that I could form no conclusion, and I found that under the necessary circumstances of the observation, viz., as it could be made only at the instant of awakening, it was impossible for me to estimate, even approximately, the duration of the crimson appearance.

Several possible modes of explaining the phenomenon at once occurred to me. Of these, however, I shall mention but three, and give reasons for rejecting two of them, while not pretending to specify them in the order in which they occurred to me. It cannot be ascribed to any visual defects in my eyes, which are normal as to colour sensations, and very perfect optically. 1*st*, I imagined it might be due to light passing through the almost closed eyelid, or through a portion of the eyeball temporarily filled with blood. Besides feeling certain that my eyes were fully open, I had the additional argument against this explanation, that I could not reproduce the phenomenon by carefully and gradually closing them, and that I am not aware that an effusion of blood in any part of the eye could possibly disappear so rapidly. 2*d*, It might be due to diffraction either by my eyelashes or by small particles, whether on the cornea or in the transparent substances of the eye, coarse enough to produce nearly the same tint for some distance round the flame. This is negatived by several considerations, among which (in addition to those urged against the preceding explanation) it is only necessary to mention again the facts, that the colour is not one which can be produced by diffraction under such circumstances, and that it appeared to be the same on the more illuminated, as well as on the darker part of the field.

3*d*, I suggest, as a possible explanation, but one which is more specially in the province of the physiologist than of the natural philosopher, that the retina (or the nerve cells connected with it?) partakes of sleep with the other nerve cells, by which that phenomenon has been accounted for, and that on a sudden awakening, the portions connected with the lowest of the primary forms of colour are the first to come into action, the others coming into play somewhat later, and almost simultaneously. This would completely account for the peculiar crimson colour, and for its uniformity of tint over the whole field, excepting the gas flame itself, the comparative intensity of whose light may easily be supposed to have simultaneously aroused all the three sensations in the small portion of the retina on which it fell, though it is just possible that it also may have appeared crimson for an exceedingly short period. I am not aware of any experiments or observations having been made with reference to the subject of this note, and I hope to have no further opportunities of making them, at least in the way in which these were made, but the point is a curious one, and worthy of the careful attention of all who may be forced to consider it. Professor Clerk-Maxwell informs me that he and others have observed that the lowest of the three colour sensations is the first to evanesce with faintness of light, and that it has been asserted to be the most sluggish in responding to the sudden appearance of light. This, however, is not necessarily antagonistic to my explanation, but will rather, if my explanation be correct, tend to show a greater interval between the awakening of the red, and that of the other colour sensations than that above hinted at.

3. On the Operator $\phi(\nabla)$. By Professor Tait.

(*Abstract.*)

By combining, as above, Hamilton's linear and vector-function with his celebrated vector square-root of the negative of Laplace's operator, an operator of great use in physical applications of mathematics is obtained. With the notation employed in the author's paper "On Green's and other Allied Theorems," *Trans. R.S.E.*

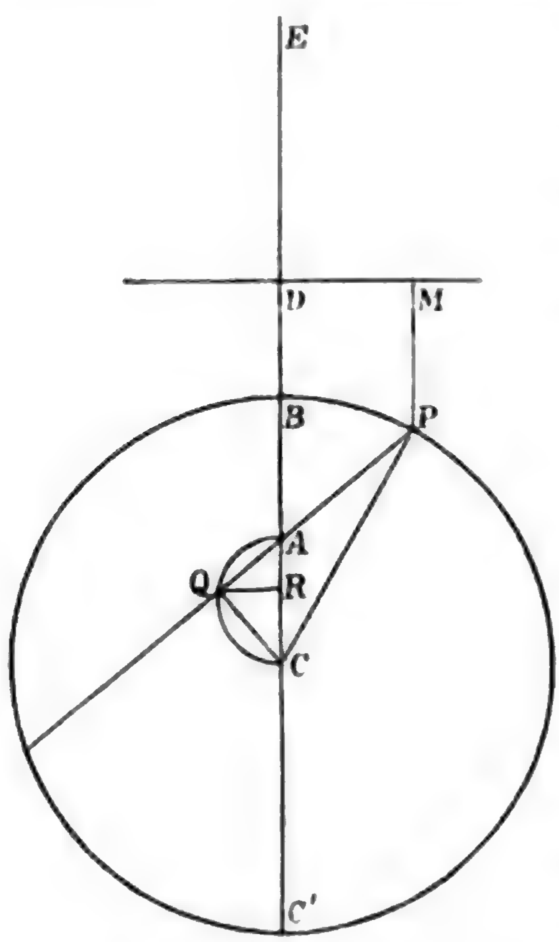
1870, § 17, it is shown to be generally expressible in the form of

$$\alpha_1 d_\alpha + \beta_1 d_\beta + \gamma_1 d_\gamma,$$

where α, β, γ, are any three unit vectors (not necessarily rectangular), and $\alpha_1, \beta_1, \gamma_1$, any three vectors whatever. The scalar and vector parts of the result of its operation on a vector-function, σ, of ρ are first considered—with various interpretations, especially as to distortions, condensations, &c., in a group of points—then it is exhibited in its applications to various questions; especially to Physical Strain, to Heat, and to Electricity. By making the constituents of ϕ variable, we have a means of Deformation specially applicable to problems such as that of Orthogonal Isothermal Surfaces.

4. Note on Pendulum Motion. By Professor Tait.

Mr Sang's papers in recent parts of the Transactions of the Society have reminded me of some geometrical constructions which are to a certain extent indicated in *Tait and Steele's Dynamics of a Particle* (1856). Some of these were suggested to me by a beautiful construction given (I believe by Clerk-Maxwell) in the *Cambridge and Dublin Math. Journal*, Feb. 1854, the others by a very simple process which occurred to me for the treatment of oscillations in cycloidal arcs. The former enables us easily to divide the arc of oscillation of a pendulum, or the whole circumference if the motion be continuous, into two, four, eight, &c., parts, which are described in equal times; also to solve by simple geometrical constructions problems such as the following:—Given any three points in a circle, find how it must be placed that a heavy particle, starting from rest at one of them, may take twice as long



to pass from the second to the third as it takes to pass from the first to the second. It suggested to me the following theorem, which really involves Mr Sang's results, but which appears to be considerably simpler in treatment, this being my sole reason for bringing it before the Society.

Let DM be a horizontal line, and let DA be taken equal to the tangent from D to the circle BPC′, whose centre C is vertically under D. Also let PAQ be any line through A, cutting in Q the semicircle on AC. Also make E the image of A in DM. Then if P move with velocity due to DM, Q moves with velocity due to the level of E; so that we have the means of comparing, arc for arc, two different continuous forms of pendulum motion, in one of which the rotation takes place in half the time of that in the other.

Let ω be a small increment of the circular measure of BAP, then

$$\text{arc at Q} = \text{CA} . \omega \, , \text{ arc at P} = \frac{\text{AP} . \text{PC}}{\text{PQ}} . \omega \, .$$

Also,

$$\text{velocity at P} = \sqrt{2g . \text{PM}} = \sqrt{\frac{g}{\text{AC}}} . \text{AP} \, .$$

Hence,

$$\text{velocity at Q} = \frac{\text{CA} . \text{PQ}}{\text{AP} . \text{PC}} \sqrt{\frac{g}{\text{AC}}} . \text{AP} = \frac{\sqrt{g . \text{AC}}}{\text{PC}} . \text{PQ} \, .$$

But

$$\text{PQ} = \sqrt{\text{CP}^2 - \text{CQ}^2}$$

$$= \sqrt{\text{CP}^2 - \text{CR} . \text{CA}} \text{ (where QR is horizontal)}$$

$$= \sqrt{\text{CA}} \sqrt{\frac{\text{CP}^2 - \text{CA}^2}{\text{CA}} + \text{AR}} = \sqrt{\text{CA} . \text{ER}} \, .$$

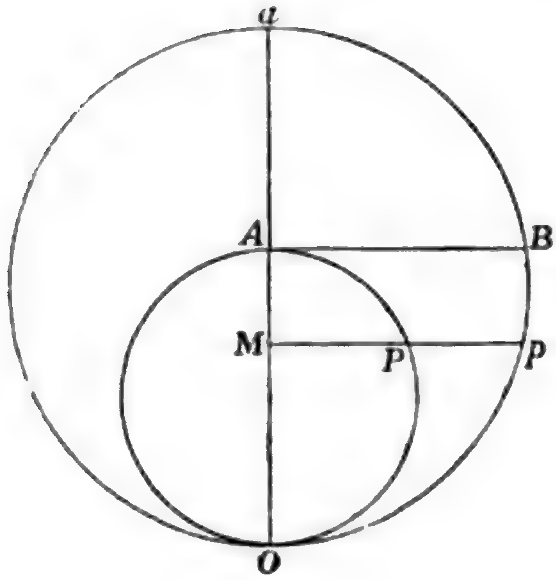
Hence,

$$\text{velocity at Q} = \frac{\text{AC}}{\text{PC}} \sqrt{g . \text{ER}} \, .$$

Thus Q moves with velocity due to the level of E, and constant acceleration

$$\frac{\text{AC}^2}{2\text{PC}^2} . g \, .$$

The second process referred to above gives at once the means of comparing continuous rotation with oscillation, as follows—



Let two circles touch one another at their lowest points—compare the arcual motions of points P and p, which are always in the same horizontal line. Draw the horizontal tangent AB. Then, if the line MPp be slightly displaced,

$$\frac{\text{Arc at P}}{\text{Arc at } p} = \frac{\text{AO}}{\text{MP}} \cdot \frac{\text{M}p}{a\text{O}} = \frac{\text{AO}}{a\text{O}}\sqrt{\frac{a\text{M}\,.\,\text{MO}}{\text{AM}\,.\,\text{MO}}} = \frac{\text{AO}}{a\text{O}}\sqrt{\frac{a\text{M}}{\text{AM}}}\,.$$

Thus, if P move, with velocity due to g and level a, continuously in its circle, p oscillates with velocity due to

$$g \cdot \frac{a\text{O}^2}{\text{AO}^2} \text{ and level AB}\,.$$

Combining the two propositions, we are enabled to compare the times of oscillation in two different arcs of the same or of different circles.

Professor Cayley has pointed out to me that results of this kind depend upon one of the well-known fundamental transformations of elliptic functions. In fact, it is obvious that the squares of the sines of the quarter arcs of vibration which the combination of the above processes leads us to compare are (in the first figure),

$$\frac{\text{CA}}{\text{CE}} \text{ and } \frac{\text{C'B}}{\text{C'D}} \text{ respectively.}$$

Calling them $\dfrac{1}{k^2}$ and $\dfrac{1}{k_1^{\,2}}$, and putting DA $= a$, AC $= e$,

we have

$$\frac{1}{k^2} = \frac{e}{2a + e}\,,\; \frac{1}{k_1^{\,2}} = \frac{2\sqrt{2ae + e^2}}{a + e + \sqrt{2ae + e^2}}\,.$$

Hence

$$\frac{1}{k_1^2} = \frac{\frac{4}{k}}{1 + \frac{1}{k^2} + \frac{2}{k}},$$

or

$$\frac{1}{k_1} = \frac{2\sqrt{k}}{1+k}.$$

Lagrange's transformation is equivalent to

$$\sin.\,\varphi = \frac{2\sqrt{k}\sin.\,\theta}{1 + k\sin.^2\theta},$$

which, for limits 0 and $\sin.^{-1}\frac{1}{k}$ for θ, gives 0 and $\sin.^{-1}\frac{1}{k_1}$ for φ; and we thus have

$$\int_0^{\sin.^{-1}\frac{1}{k_1}} \frac{d\varphi}{\sqrt{\frac{1}{k_1^2} - \sin.^2\varphi}} = \frac{2k_1}{\sqrt{k}}\int_0^{\sin.^{-1}\frac{1}{k}} \frac{d\theta}{\sqrt{\frac{1}{k^2} - \sin.^2\theta}}.$$

whose application to the pendulum problem is obvious.

5. On the Decomposition of Forces externally applied to an Elastic Solid. By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edin.

(*Abstract.*)

The principles set forth in this paper, though now (with the exception of the first theorem) published for the first time, were communicated to the French Academy of Sciences fifteen years ago, in a memoir entitled "de l'Equilibre intérieur d'un Corps solide, élastique, et homogène," and marked with the motto, "Obvia conspicimus, nubem pellente Mathesi," the receipt of which is acknowledged in the Comptes Rendus of the 6th April 1857 (vol. xliv. p. 706.)

The author quotes a theorem discovered by him, and previously published in the Philosophical Magazine for December 1855, called "the Principle of Isorrhopic Axes," viz., "Every self-

balanced system of forces applied to a connected system of points, is capable of resolution into three rectangular systems of parallel self-balanced forces applied to the same points."

Let X, &c., be the forces resolved parallel to any three orthogonal axes; find the six sums or integrals, ΣXx, ΣYy, ΣZz, $\Sigma Yz = \Sigma Zy$, $\Sigma Zx = \Sigma Xz$, $\Sigma Xy = \Sigma Yx$; these are called the "rhopimetric coefficients." Conceive the ellipsoid of whose equation these are the coefficients; then for the three axes of that ellipsoid (called the "isorrhopic axes") each of the last three coefficients is null; and the three systems of forces parallel respectively to those three axes are separately self-balanced.

The theorem may be extended to systems of moving masses by putting $X - m\frac{d^2x}{dt^2}$, &c., instead of X, &c. If for any system of forces, the last three rhopimetric coefficients are null, and the first three equal to each other, every direction has the properties of an isorrhopic axis. This, of course, includes the case in which all the coefficients are null; and in that case the system of forces is said to be "Arrhopic." The author shows that the six rhopimetric coefficients of a system of forces externally applied to an elastic solid, being divided by the volume of the solid, give the mean values throughout the solid of the six elementary stresses. Those are called the "Homalotatic stresses."

If we calculate from them the corresponding externally applied pressures, these may be called the "Homalotatic pressures."

Take away the homalotatic pressures from the actual system of externally applied pressures, and the residual pressures will be arrhopic; that is to say, their components parallel to any direction whatsoever will be separately self-balanced, and may have their straining effects on the solid separately determined; and hence, the axes to which those residual pressures are reduced may be arbitrarily chosen, with a view to convenience in the solution of problems.

The author then demonstrates that those problems respecting the distribution of stress in an elastic solid, in which the stresses are expressed by constants and by linear functions of the co-ordinates, are all capable of solution independently of the coefficients of elasticity of the substance.

6. On Geometric Mean Distance. By Professor Clerk Maxwell.

7. On a Singular Case of Rectification in Lines of the Fourth Order. By Edward Sang, Esq.

The class of curves resulting from the formula

$$x = a \,.\, \sin \theta \;,\; y = b \,.\, \sin 2\,\theta$$

are of considerable interest as occurring in various mechanical inquiries. When a straight wire, whose effective breadth and thickness are as two to one, is fixed at one end and made to vibrate, its free end describes a curve of which the general equation is

$$x = a \,.\, \sin (\theta + k) \;,\; y = b \,.\, \sin 2\,\theta \,,$$

in which k is constant for the particular variety of curve. When $k = \frac{1}{2}\pi$ the curve becomes a parabola, and when $k = o$, it takes the form above mentioned; these phases were exhibited by me in 1832. Again, when a system of toothed wheels is deduced from a straight rack, having a curve of sines for its outline, the points of contact describe a curve of this class, as is shown in my treatise on the teeth of wheels.

In attempting the rectification of these curves, we have to integrate an expression of the general form

$$d\,l = \{\, a^2 .\, \cos \theta^2 + 4\, b^2 .\, (\cos 2\,\theta)^2 \}^{\frac{1}{2}} d\,\theta \,,$$

and for this purpose have to expand the root in an interminate series, and then integrate each term, the result being unmanageable from its complexity. In one particular phase of the curve, however, the integration can be easily effected. The above general expression may be written

$$d\,l = \{\, 16\,b^2 .\, \cos \theta^4 + (a^2 - 16\,b^2) \cos \theta^2 + 4\,b^2 \}^{\frac{1}{2}}\, d\,\theta \,,$$

and we readily observe that if $a^2 = 32\,b^2$, that is, if $a = 4\,b\sqrt{2}$,

the quantity under the radical sign becomes a square, and in this case

$$d\,l = \{\,4\,b\,.\cos\theta^2 + 2\,b\,\}\,d\,\theta$$
$$= \quad 2\,b\,\{\,\cos 2\,\theta + 2\,\}\,d\,\theta\,,$$

whence, on integrating, we at once obtain

$$l = b\,\{\,\sin ?\,\theta + 4\,\theta\,\} = y\,4\,b\,\theta\,.$$

The expression for the radius of curvature also takes a very simple form, it is

$$r = \frac{b}{\sqrt{2}}\,\frac{(\cos 2\,\theta + 2)^2}{\sin\theta}\,.$$

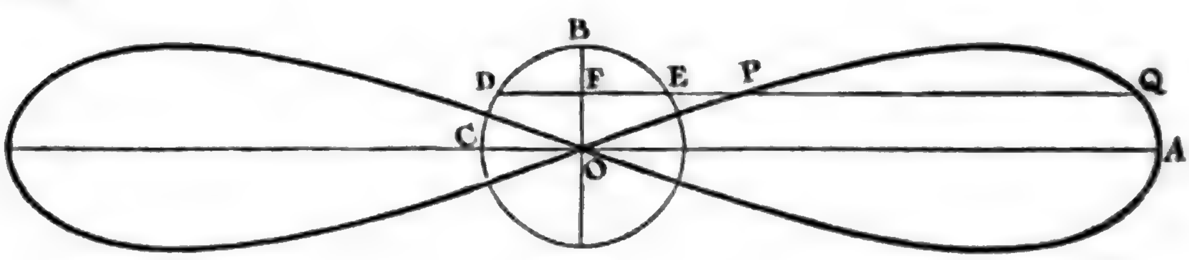
No other curve of this class, nor indeed any belonging to the more general formula

$$x = a\,.\sin(p\,\theta + k)\,,\; y = b.\sin(q\,\theta)\,,$$

seems to be susceptible of easy rectification.

These results may be exhibited geometrically thus:—Having drawn OA, OB in the directions of the length and breadth of the curve, and described round O a circle with the radius OB = OC = b, let OA be made equal to four times CB, and an hour-glass curve be constructed in the usual manner. Then, having assumed any arc CD to represent $b\,.\,2\,\theta$ and drawn DFQ parallel to OA, if FP be laid off equal to $a\,.\sin\theta$, P is a point in the curve, and the length from O to P is equal to the sum of OF, and twice the arc CD.

Hence it follows that the portion PQ of the curve, cut off by the line DQ, is just double of the circular arc DBE, cut off by the same line.



Hence it appears that the length of the quadrant OPQA of the curve is just equal to the circumference of the circle, or that the whole curve is equal in length to four times the circumference of the circle described with the radius OB.

The following Gentlemen were admitted Fellows of the Society:—

David Maclagan, Esq., C.A.
Major Rickard.
Dr John Sibbald.
Dr J. G. Fleming.
Rev. Andrew Tait, LL.D.
David Grieve, Esq.
The Right Rev. Bishop Cotterill.
George Barclay, Esq.

Monday, 29th January 1872.

The Hon. LORD NEAVES, Vice-President, in the Chair.

The following Communications were read:—

1. On the Wheeling of Birds. By Professor Fleeming Jenkin.

2. Notice of a New Family of the Echinodermata. By Professor Wyville Thomson, LL.D., F.R.SS.L. and E., F.L.S., F.G.S.

During the deep sea dredging expedition of H.M. ships 'Lightning' and 'Porcupine,' in the summers of 1868–69 and 1870, two or three nearly perfect specimens, and a number of fragments were procured of three species of regular echinideans, which were referred by the author to a new family, the Echinothuridæ, intermediate in their more essential characters between the Cidaridæ and the Diadematidæ.

In these urchins the test is circular and greatly depressed. The plates of the perisom are long and strap-shaped, and the inter-ambulacral plates overlap one another regularly from the apical towards the oral poll, while the ambulacral plates overlap in a similar way in the opposite direction. The test is thus flexible. The plates of the ambulacral areæ are essentially within the inter-ambulacral plates which over-lie them along their outer edges. The ambulacral pores are tri-geminal, arranged in wide arcs; the

two pairs of pores of each arc which are nearest the centre of the ambulacral area, pierce two small accessory plates intercalated between the ambulacral plates, while the outer pair passes through the ambulacral plate itself near its outer extremity. The tube-feet on the oral surface of the body are provided with terminal suckers, supported by calcareous rosettes, while those on the apical surface are conical and simple. The tube-feet on both surfaces have their walls supported by wide cribriform calcareous plates.

The peristome and the periproct are unusually large. The edge of the peristome is entire, without branchial notches, and the peristomial membrane is uniformly plated with twenty rows of imbricating scales, corresponding with the rows of plates of the corona, and the rows of ambulacral tube-feet are continued as in the Cidaridæ, over the peristome up to the edge of the mouth. The ovarial plates are unusually large; in some of the species they are broken up into several calcareous pieces. The ovarial apertures are very large, and are partly filled up with membrane.

The dental pyramid is wide and strong, but somewhat low on account of the depressed form of the test. The epiphyses of the tooth-sockets do not form closed arches as in the Echinidæ, and in this respect resemble those of *Cidaris* and *Diadema*. The teeth are simply grooved as in *Cidaris*. The spines are hollow and comparatively small, and the larger spines show a tendency to the spiral arrangement of projecting teeth which is so characteristic of the Diadematidæ. The Pedicellariæ are very remarkable in form, more nearly related, however, to those of the Diadematidæ than to any others. A strong fenestrated fascia traverses the body cavity vertically on either side of each ambulacral area, thus nearly cutting off the ambulacral from the inter-ambulacral region, and allowing only a small space for the coils of the intestine.

For this family, distinguished by the depressed corona of imbricated plates, the peristome covered with scales through which the rows of ambulacral double-pores are continued to the mouth, the absence of branchial notches in the peristomial border, the peculiar arrangement of the ambulacral pores, the heterogeneity of the tube-feet on the oral and apical surfaces, the absence of closed arches uniting the pairs of tooth-sockets, and the absence of longitudinal ridges within the simple grooved teeth, the term

Echinothuridæ was proposed, the fossil-genus *Echinothuria*, sagaciously described by the late S. P. Woodward, from an imperfect specimen from the upper chalk being taken as the type. The specimens procured were referred to two genera and three species.

In the genus *Phormosoma* the plates of the perisom only slightly overlap, and fit so closely as to form a complete calcareous casing without any membranous fenestræ. Although constructed essentially on the same plan, the apical and oral surfaces of the test differ from one another singularly in character, the oral surface being almost uniformly covered with large areolar depressions surrounding spine tubercles.

One species, *Phormosoma placenta*, n. sp., was dredged in deep water off the Butt of the Lews, and some fragments were met with in gravel from the Rockall Channel.

In the genus *Calveria*, the plates of both the ambulacral and inter-ambulacral areas form large expansions towards the middle line of the area, while the outer portions of the plates are narrow and strap-shaped, leaving large fenestræ filled up with membrane between plate and plate. The oral surface of the body does not differ markedly in character from the apical.

Two species of this genus were taken, *Calveria hystrix*, n. sp., with a strong perisom, of a nearly uniform rich claret colour, from deep water off the Butt of the Lews; and *Calveria fenestrata*, n. sp., more delicate, with wider spaces between the plates, the body of a greyish colour, rayed from the apical pole with bright chocolate.

It is very possible that the genus *Asthenosoma*, described by Professor Grube, may belong to this group, but the description of that form hitherto given is not sufficient for identification, as the points of structure on which the families of the Echinidea are distinguished from one another are not noticed. With this exception, the form which most nearly resembles them is *Astropyga*, which, however, is in every respect, except in habit, a true *Diadema*, with the peristomial margin deeply notched for external branchiæ, and all the other characters of the family.

3. On the Principles which regulate the Incidence of Taxes. By Professor Fleeming Jenkin.

It is well known that many taxes do not fall ultimately on the person from whom they are in the first instance levied. The merchant advances the duties imposed on goods, but the tax ultimately falls on the consumer. The problem of discovering the ultimate or true incidence of each tax is one of great importance, and of considerable complexity. The following paper contains an investigation of the methods by which this incidence may in some cases be experimentally determined, and of the principles regulating the incidence in all cases, these principles being stated in a mathematical form.

The author, in a paper published in Recess Studies, expressed the law of supply and demand by representing what may be termed the demand and supply functions, as curves. The ordinates parallel to the axis OX, fig. 1, were prices—the coordinates parallel to the axis OY were the supplies at each price, and the demand at each price for the respective curves—the market price is then indicated by the ordinate X of the point at which the curves intersect, this being the only price at which buyers and sellers are agreed as to the quantity to be transferred.

We might write the law algebraically as follows, calling y the quantity of goods in the market, at each price x, we have $y = \varphi x$; and calling y_1 the quantity of goods demanded at each price, we have $y_1 = \varphi_1 x$; the market price is determined by the equation $y = y_1$. There is, however, little or no advantage in adopting this algebraic form, because we cannot suppose that in any instance φx or $\varphi_1 x$ will be any tolerably simple algebraic function, whereas the curve for given goods might be determined experimentally by observing from year to year variations of quantities bought or quantities supplied at various prices.

Professor Jevons has since given a much more complex algebraic representation of the same law, which, however, reduces itself to the above simple form.

The graphic method may also be employed to indicate the advantage gained by each party in trade, and to show how it may be estimated in money. Let the two curves indicate the demand

and supply at each price for a certain kind of goods. If all sellers were of one mind, and were willing to supply all their goods at a given price x, and were quite determined to sell no goods below that price, the supply curve would be a mere straight line parallel to OX, and ending abruptly at the ordinate raised at x. Similarly, if all buyers were of one mind, and would only buy below a given price x, but were willing to buy all they want at that price, and no more at any lower price, the demand curve would be a line parallel to OX ending abruptly at the ordinate raised at x, and the price would be quite indeterminate. If the two lines overlapped, transactions might take place at any price between that at which the

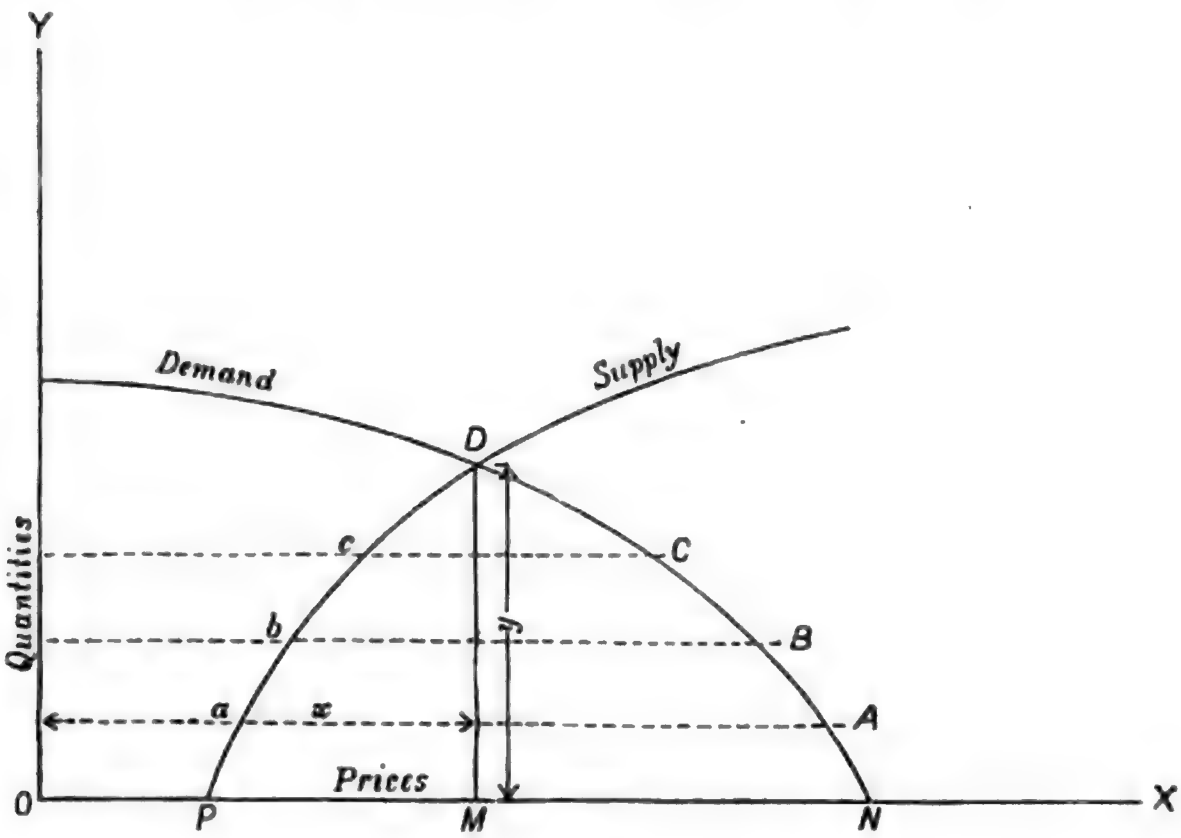


Fig. 1.

sellers were willing to sell and the buyers willing to buy; there would in this case be no market price. This case does not represent the true state of either buyers' or sellers' minds in any real large market. There are always a few holders who would only sell if the price were much higher than the market price,—these are the people who expect prices to rise; there are some who are just willing to sell at the market price, but who will not sell a penny below; and there are others, weak holders, who expect prices to fall, and these would really, if pushed to extremity, sell below the market price. This condition of things is represented by the supply curve in fig. 1.

Similarly, there are a few buyers who, if pushed to extremity, would buy some goods above market price; some also will just buy at market price; some will not buy unless the price is below market price. This is represented by the demand curve.

Now, I contend that when the market price is fixed, those traders who are perfectly indifferent whether they buy or sell at that price reap no benefit by the trade; but these will be few in number.

Looking at the demand curve, the ordinate X_A from the axis OY to A represents the value set on some of the goods by some buyers, but these buyers have got the goods for the sum represented by the ordinate x = OM; the difference between these two ordinates $X_A - x$ is the difference in price between what was given and what might have been given for a certain small quantity Δy of goods. Δy is therefore the benefit reaped by buyers from the purchase of the quantity Δy; and integrating the benefits derived from the sale of each successive quantity, we find the area MDCBAN represents the whole gain to buyers by the purchase of the quantity y of goods. Similarly, it is easy to show that the area MD*cba*P represents the gain to sellers by the same transaction; these areas represent the gain in money. Each product $\Delta y(x - X_A)$ being the product of a quantity by the gain in money per unit of quantity.

Thus the whole benefit to the two leading communities is represented by the sum of the two above named areas, and the repartition of the benefit between the two communities is perfectly definite.

Professor Jevons has used curves to integrate what he terms the utility gained by exchange in a manner analogous to the above; but utility, as he defines it, admits of no practical measurement, and he bases his curve, not on the varying estimates of value set by different individuals each on what he has or what he wants, but on the varying utility to each individual of each increment of goods. The above estimate of the gain due to trade, deduced from the demand and supply curves as originally drawn in my Recess Studies' article is, I believe, novel, and gives a numerical estimate in money of the value of any given trade, which might be approximately determined by observing the effect of a change of prices on the trade; the curves throughout their whole lengths could cer-

tainly not, in most cases, be determined by experiment, but statistics gathered through a few years would show approximately the steepness of each curve near the market price, and this is the most important information.

A steep supply curve and a horizontal demand curve indicate that the buyers reap the chief benefit of the *trade*. The sellers, if producers, may, however, be making important profits as capitalists and labourers.

A steep demand curve and a level supply curve indicate that the suppliers are chiefly benefited by the trade; the community or body which is most ready to abandon the trade if the price increases a little, benefits least by the trade.

When the traders are producers and consumers, the benefits estimated in this way as due to the *trade* are not the only benefits reaped by the community from the manufacture.

In this case, what is termed the supply curve depends on the cost of production of the article, including that interest on capital and that remuneration for skilled superintendence which is necessary to induce the producer to employ his capital and skill in that way. The cost of production increases generally with the quantity of the article produced, otherwise the supply curve would be a straight vertical line; but as a matter of fact, to produce an increase of production a rise of price is necessary, indicating that only a few men with little capital are content with a small rate of interest and small remuneration for their skill, but that to induce many men and much capital to be employed in the particular manufacture, a large rate of interest and considerable remuneration are required, hence the supply curve will be such as shown in fig. 2, where the price OP is that price or cost of production which is just sufficient to tempt a few producers to produce a little of the article.

Then if OP′ is the actual cost out of pocket required to produce a small quantity of an article, and if OP is the lowest cost at which any manufacturer can afford to produce it, the area P′D′DM represents the whole profit to the producing capitalist when the price is OM. The line D′P′ is not necessarily parallel to DP, nor vertical, the bare cost of production of the article generally increases as the quantity increases; and in that case D′P′ is not vertical. Again, the rate of interest required to tempt additional capital

into a particular field is not constant, but increases, hence P′D′ is steeper than PD. I see at present no means of experimentally ascertaining the gain reaped by producers represented by the area PDD′P′; it can be approximately estimated by considering the prevailing rate of interest in the producing community and the amount of capital required for the production of the unit of the article.

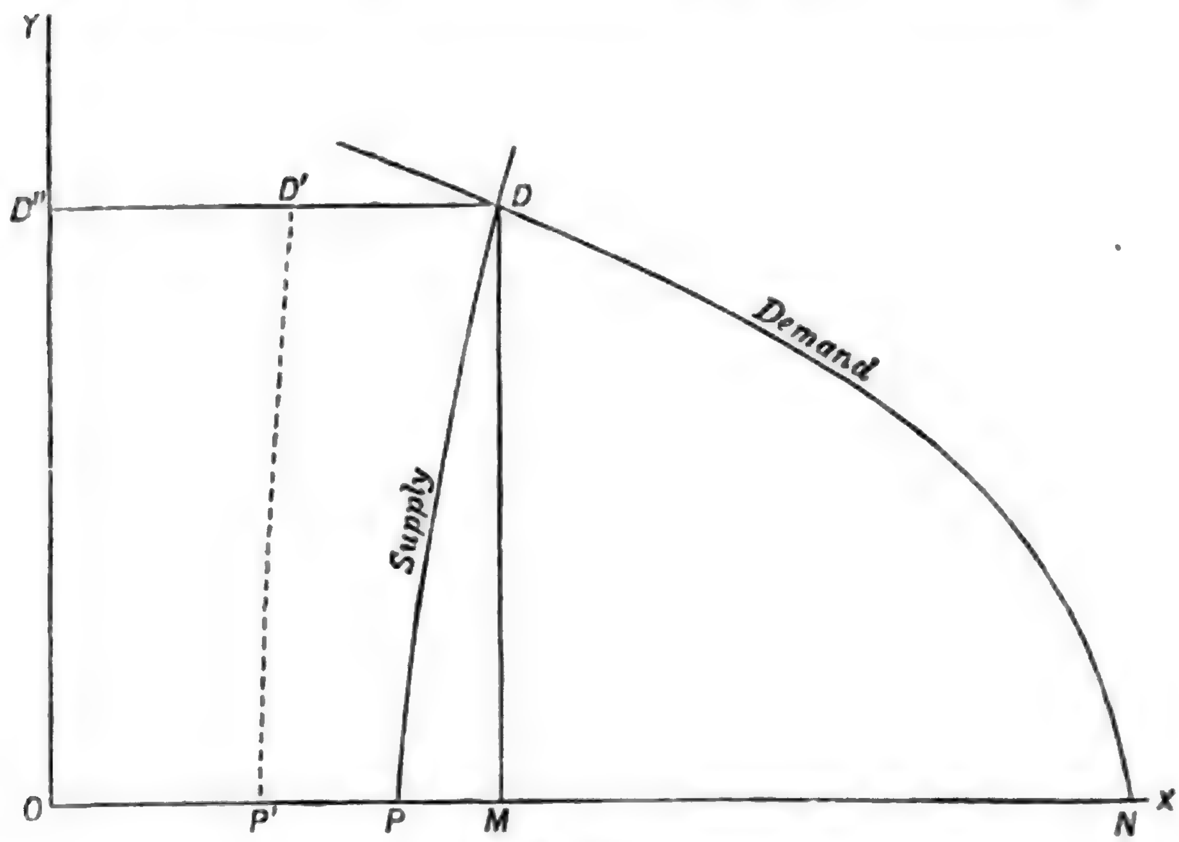


Fig. 2.

We see that the gain of a manufacturing capitalist may be divided into two parts—the profit as a trader, and the interest as a capitalist.

In safe trades, where there are few fluctuations in price, the former gain may perhaps be the most important; in more speculative trades the latter.

There is yet a third source of gain to the manufacturing community: the labourer who produces the goods earns his wages by the manufacture, and this is an advantage to him. In the diagram, the area OP′D′D″ represents the wages paid for labour alone. The length of the lines between OY and P′D′ represent the wages of labour per unit of goods, increasing as the quantity of goods required increases. This is lost to the community if the manufacture is stopped. Thus the whole sum paid by the consumer is the area OMDD″; and this is made up of three parts, one of which

is the profit to the trader, one the interest to the capitalist, and one the wages of the labourer; all these advantages are lost if the manufacture ceases.

The gain of the labourer does not resemble the profit of the trader, or the interest of the capitalist. The profit of the trader is the difference between his valuation of the goods and what he gets for them. If he does not sell his goods he still has his goods, he only loses the profit. Similarly, if the capitalist does not sell his capital, he still has his capital. Now, the area P′PDD′ represents the profit made by the capitalist on the particular employment of his capital, and this is all that he loses if unable to sell that capital; but the area OP′D′D″ represents the whole sum received by the labourers, not their profit. The profit of the labourer may perhaps be considered as the excess of wages which he earns in a particular trade, over that which would just tempt him to work rather than starve or go into the workhouse.

If the consumer purchases the article for simple unproductive consumption, then the loss to him is only represented by the area DMN. If, however, a community purchases goods, and consumes them productively, then, by the cessation of the trade, they in their turn lose the interest on the capital they employ, and the labourers of the community lose their wages; so that, in that case, the loss to the buyer, who cannot be classed as an immediate consumer, is made up of three parts, similar to those enumerated in the case of the seller.

Taxes on Trade.

Having distinguished between the three distinct advantages given by trade, I will now consider the incidence of a tax on trade, levied as a fixed sum per unit of goods, as one pound per ton, or one shilling per gross.

The effect of such a tax is to produce a constant difference between the price paid by the buyer and the price received by the seller. The market prices are determined in the diagram of the supply and demand curves, by the points between which a line parallel to OX, and equal in length to the tax, can be filled between the two curves.

Thus, if in figure 3, FN be the demand curve, and PE the supply curve, and if the length of the line CC′ be the amount of

the tax per unit of goods, then OM is the market price to the supplier, OM′ the market price to the buyer and the difference MM′ is equal to the tax.

The total amount raised by the tax from the transactions represented in the diagram, is measured by the area MCC′M′. The

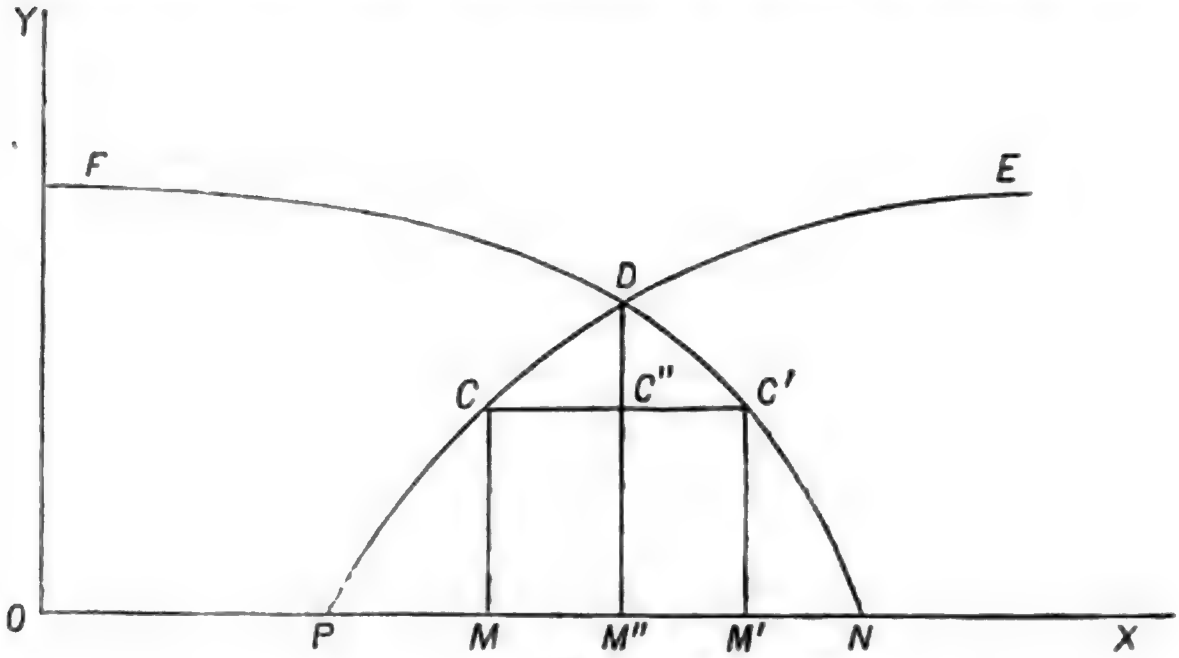


Fig. 3.

portion paid by the seller is measured by the area CC″M″M. The portion paid by the buyer is measured by the area C″C′M′M″. The whole loss entailed by the tax on the two communities is measured by the area MCDC′M′; the loss to the sellers is measured by the area CDM″M; the loss to the buyers by the area M″DC′M′; both buyers and sellers suffer a loss beyond the tax they pay. This excess of loss is represented by the area CC″D for the sellers, and C′C″D for the buyers.

If the tax be large, the line CC′ will approach the axis OX, the tax will be unproductive, and the area CC′D representing the excess of injury to the buyers and sellers will be large, compared with the produce of the tax. This fact is one justification of free trade.

There is a certain magnitude of tax which will produce the maximum revenue or value for the area MCC′M′. The ratio in which the tax falls, in one sense, on sellers and buyers is simply the ratio of the diminution of price obtained by the sellers to the increase of price paid by the buyers.

It is absolutely clear that this is the proportion in which the tax is actually *paid* by the two parties, although this may by no means

correspond to the relative suffering inflicted on the two parties, nor is it even the proportion in which the two parties lose by the loss of trade profit. The whole loss of either party is, as the diagram shows, always greater than the tax they pay. The relative total losses of the two communities as traders, are in proportion to the areas MCDM″ and M′C′DM″; and these areas might approximately, at least, be ascertained by experiments for this purpose, treating CD and C_1D as straight lines, we only require to know the quantity and price of the goods before the imposition of the tax, and the quantity and price afterwards.

Thus, if a tax of 2d. per pound were imposed on the trade in cotton between ourselves and America, if before the tax we imported 500 million lbs. at one shilling, and after the tax 300 million lbs. for which we paid 13½d., and the Americans received 11½d., the total loss to the two communities as traders would be $600 + 200 =$ 800 million pennies, the produce of the tax 600 million pennies.

England would pay of the tax 450 million pennies. England's total loss would be 600 million pennies. America would pay of the tax 150 million pennies. America's total loss would be 200 million pennies. The incidence would be the same whichever government levied the tax.

It follows from the above principles, that if a holder sells unreservedly, trusting to the competition between the buyers to produce the market, the whole tax falls on the seller; the supply curve becomes a vertical straight line. If a buyer buys unreservedly, the whole tax falls on him; in this case the demand curve becomes a vertical straight line.

Thus, if sales by auction were subject to a tax *ad valorem* or otherwise, and if sales were quite unreserved, the number of transactions not being altered, the prices would be unaltered, but the sellers would only get the prices minus the tax.

This case does not practically arise, because, if auctions were really so taxed, although in each auction that occurred the sale might be unreserved, auctions would, as a whole, be checked; fewer people would put up their goods for sale in that way,—the prices would rise, the number of transactions would be diminished, and the tax would really be borne in part by the buyers and part by the sellers.

If the trade between two countries really consists in the exchange of goods, effected by the agency of money as a unit for expressing value, but not involving the actual transfer of coin, the above principles show the whole gain by the exchange to be the sum of two gains which each party would make by each trade if it alone existed.

If by duties one portion of the trade be extinguished or much diminished, both parties lose, but if the other portion of the trade remain uninjured, then, although there may be no exchange of commodities other than of goods for actual money, nevertheless the full gain on that which is untaxed remains intact. Thus, although the French may tax our goods, and so inflict a loss on themselves and on us, this is no reason for our inflicting an additional loss on the two communities by taxing the import of their goods.

House Rent.

I will next consider the effect of a tax on house rent.

Landlords are here the sellers, and tenants the buyers of what may be termed a commodity; not the house, but the loan of a house for a term of years—the tenant buys what might be called, by the extension of a suggestion of Professor Jevons, a *house-year* from his landlord.

The difference between the house and other commodities such as food or dress is, that the house remains, whereas they are consumed. The house-year is consumed year by year, but it is reproduced year by year without material fresh expenditure on the part of the landlord. This permanency alters the incidence of taxation.

If the demand falls off the landlord cannot remove his house—he cannot cease to produce his house-year, which therefore he must dispose of. Hence, in a stationary or declining community, where no new houses are being built, but where year after year a sensible proportion remains unoccupied, the landlord must sell his house-year unreservedly, and any tax imposed on house rent would fall on him alone; that is to say, he would receive a rent diminished by the full amount of the tax, and the tenant would pay no more rent for a house of a given class than if no tax were imposed. The supply curve becomes a straight horizontal line, and is unaffected by the tax; the demand curve is equally unaffected by

the tax; the number of houses let is unaltered by the tax, but the landlords lose as rent the whole amount raised by taxation.

This reasoning is based on the assumption, that the supply curve has become a straight horizontal line unaffected by the tax. This condition is altered in any prosperous or growing community. There, new houses must be built, and a considerable number of houses are always unlet, not because they are not required by the community, but because the speculative builders are holding out for higher terms. This produces a supply curve of the kind common to all other kinds of goods. At higher prices more goods are forthcoming. A newly imposed tax will then be distributed between sellers and buyers, landlords and tenants in a manner depending on the form of these curves. A sensible check will be given to the letting of houses, tenants will be content with somewhat less good houses, and landlords with rather smaller rents. This is the immediate effect of the tax—the greater portion would probably fall on the landlords at first, at least in the new houses where fresh contracts are being made. But after a few years the conditions would have altered. New houses are only built because the builders obtain the usual trade profit and interest on their capital—the check to letting consequent on the imposition of the tax will therefore diminish the supply of new houses until, owing to diminution in supply, rents have risen to their old average. Then builders resume their operations. The whole tax by that time will be borne by the tenants; that is to say, if there were no tax they would get their houses cheaper by the precise amount of the tax, because rents so diminished would suffice to induce speculative builders to supply them. The rents through the whole town are ruled by those of the new districts. There is a certain relative value between every house in the town, and if the rents of new houses are dearer the rents of the old houses are increased in due proportion. In fact, when new houses need to be supplied year by year, houses are commodities which are being produced, and the tax falls on the consumers.

The above principles determine the incidence of a tax, whether nominally levied on the landlord or tenant, but in their application account must be taken of the mental inertia of both landlords and tenants, as well as of the fact that many contracts for houses are

not immediately terminable. These two conditions will for the first few years after the imposition of any new tax cause it to fall on the party from whom it is nominally levied.

Precisely as a tax on trade not only falls on the traders, but injures capitalists and labourers, a tax on house rents injures the capitalists who build houses and the labourers they employ—not that the capitalist pays the tax, but he is prevented from finding a useful investment for his money owing to the diminution in the number or quality of houses required.

Taxes on Land.

The question of the incidence of taxes on land is peculiarly interesting. Land differs from all other commodities, inasmuch as the quantity of it does not depend on the will of any producer. The number of houses in a flourishing community does depend on the will of speculative builders; but land can only be increased in quantity by such processes as enclosing commons, or breaking up private pleasure grounds. We will neglect these small disturbing influences, and assume that all the land in a country is available for cultivation, where such cultivation is profitable; and that the absence of profit is the only reason for neglecting to cultivate any portion of it.

It is well known that the rent of each acre of land is simply the excess of annual value of that acre over the annual value of the poorest land which tenants think it worth while to cultivate. We may classify all land according to the total return which it will yield per acre upon capital invested in its cultivation; and we may draw a supply curve of land such that the ordinates will be the total quantities of land which will return each successive percentage on the capital required to cultivate it. The supply diminishes as the rate of percentage increases, that is to say, there is less land which will return ten per cent. on the capital than will return five per cent., and still less land which will return twenty or thirty per cent.

If, therefore, tenants as a body, considered as capitalists, will not cultivate any land which does not yield twenty per cent., there will be far less land in the market than if they will be just satisfied with ten per cent.

Again, all tenants are not of one mind, and we may construct a demand curve in which the ordinates are the total quantities of

land which would be let, if the land paying no rent be fixed at each successive percentage. The actual quantity of land let will be determined by the intersection of the two curves, and is represented by the height MD, fig. 4.

If we now build a solid on the base OD′DN, such that its height all along each ordinate x is the number of hundreds of pounds of capital per acre required to give the percentage corresponding to the length x, then we shall have a volume standing on (OD′DN), the contents of which will measure the total annual returns from all the land cultivated.* The rent is the volume standing on MDN, the profit received by the farmers is the volume standing on OD′DM, and this is in excess of what would have just tempted them to cultivate by the volume MDP. We may, therefore, considering the farmer as a capitalist and a trader, call the volume on MDP his trade profit, and the volume on OD′D the interest on his capital.

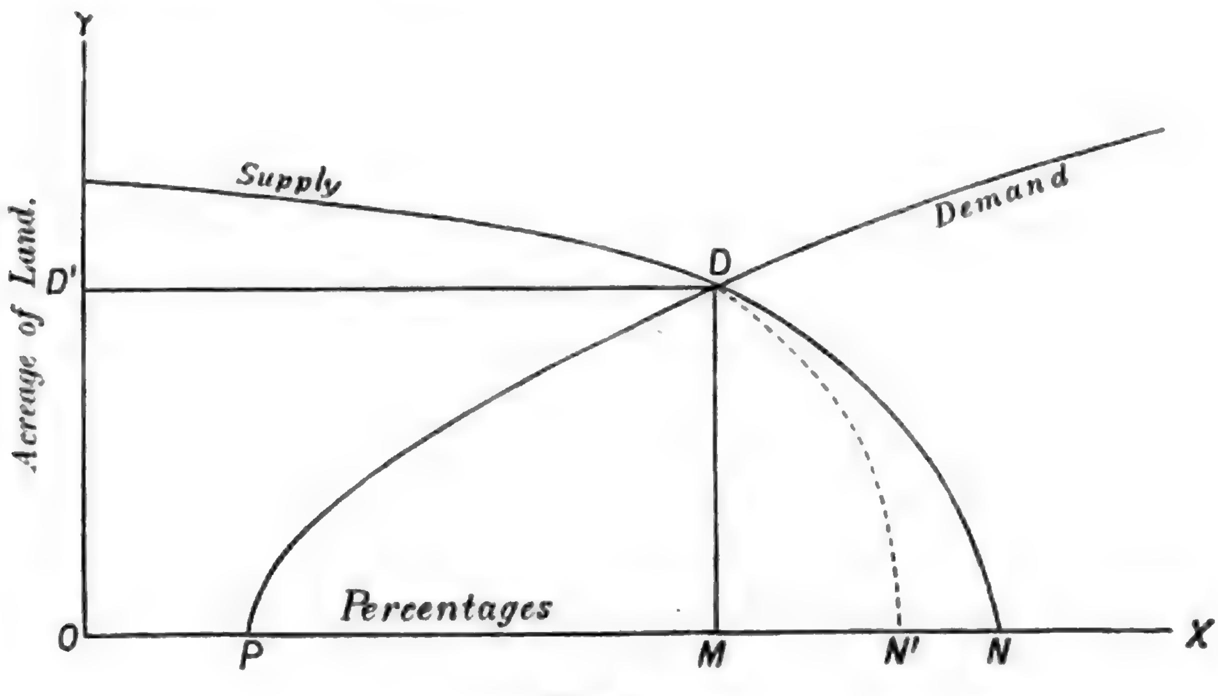


Fig. 4.

The effect of any tax on the land is to reduce the interest which each class of land is capable of returning on the capital employed. This it will do in very different ways according to the manner in which the tax is levied.

* If L.150 per acre are required to give the percentage x of any one class of goods, the height of the ordinate perpendicular to the plane of OD′DN will be 1·5.

If the tax be an *ad valorem* duty on rent, it will modify the supply curve only between D and N. There will remain just as much land as before capable of paying rates of interest less than OM, but the quantity of land capable of paying the higher rates will be diminished; in other words, the rate of interest which the poorest land worth cultivating pays will not be affected, for this land pays no rent and remains untaxed—hence no land will be thrown out of cultivation, but the supply curve will be altered from DN to DN′, diminishing the volume representing rent, but leaving the other quantities untouched; hence any tax assessed on rent is paid wholly by the landlord. The amount of the tax is the volume standing on DNN′. It is curious to remark that this tax in no way falls on the consumer. The tax on rent simply diminishes the excess of value which some land has over others; no land is thrown out of cultivation, and no less capital employed in production than before; no one suffers but the landlord. If, instead of being assessed on the rent, the tax is assessed on the produce of the cultivation, the incidence of the tax will be greatly modified. The cultivation of land will no longer be so profitable; *i.e.*, the returns from capital employed on the land will be less; in other words, the whole supply curve of the land will be modified, falling everywhere if the produce taxed be that which is produced on all qualities of land. Some land will fall out of cultivation, and only part of the tax will be borne by the landlord; part will fall in the first instance on the tenant, but he, like any other manufacturer, will recover almost the whole of his portion from the consumer. Tenants will be injured by the limitation of the number of transactions, and labourers by the diminution in the amount of work required. This is the effect of an octroi duty.

Sometimes a tax is assessed not on the rent, but on an assumed value per acre. Such a tax can never be raised on land which pays no rent, for the owner would rather abandon possession of the land than pay the tax. It might, however, lead to the abandonment of the cultivation of poorer soils; it would then injure tenants and consumers, although they would not pay one penny of the tax; for taxes cannot be paid out of lands which lie waste; assuming that the tax is always less than the rent, as it certainly always should

be, it will be paid wholly by the landlords. The tax in this case does not diminish the supply of land.

A cognate question of great interest is, Who reaps the benefit of any improvements in agriculture, making land return more than it previously did? This improvement may require, and probably will require, increased investment of capital. The whole supply curve will be raised; assuming the demand to remain the same, fig. 5, M″D″ will be the new increased number of acres in cultivation, but land will be left uncultivated which would have returned the interest OM on capital. The volume standing on D′D″N″ will be much greater than that on D′DN, for the third dimension will also have increased; the average rate of interest and the trade profit of the tenant will have increased, and it is highly probable that the volume standing on D″M″N″ may be greater than that which stood on DNM; but this is by no means certain. It might at first be actually smaller. In all probability, however, the demand curve is very nearly vertical, a small increase of profit tempting a

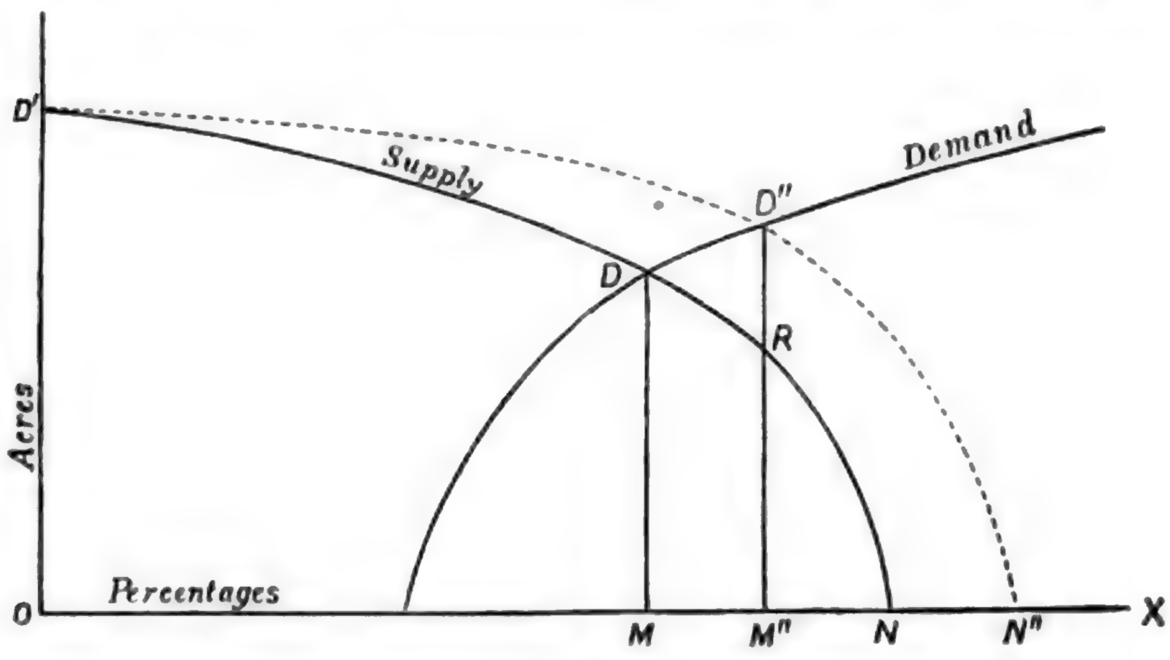


Fig. 5.

largely increased investment of capital in farming. If this be so, then the landlord also reaps considerable benefit from the improvement, for if the farmers were contented with nearly the same rate of interest as before, the solid standing on DRNN″D″ which he gains would be larger than the solid on DRM″M which he loses; moreover, the volume on RNM″, which he retains, is increased. Labourers and consumers also gain.

4. Additional Notes on the Occurrence of the Sperm-Whale in the Scottish Seas. By Professor Turner.

In a communication made to this Society on the 6th February, 1871, I noted the capture of a sperm-whale at Oban in May, 1829, and I collected from various sources records of the stranding of seven additional specimens on the Scottish coasts.

Since that communication was published, a large sperm-whale has come ashore on the west coast of the Isle of Skye, some particulars concerning which I propose to relate in this communication.

Tourists in Skye, during the past autumn, who visited Loch Corruisk by boat from Torrin, as they sailed up Loch Scavaig, became conscious, by another sense than that of sight, that a large animal in a state of putrefaction was in their immediate vicinity.

A correspondent of the "Glasgow Herald," writing in July last, states that a great whale entered Loch Scavaig about the middle of that month, and after floundering about, bellowing like a bull amongst the rocks, amidst which it had become entangled, it died after a lapse of two or three days. Large quantities of blubber were removed from the carcase without loss of time by the neighbouring fishermen, but enough of the external form remained to enable the correspondent to give the following description: Skin black, thick and corrugated. Head enormous, square, ending in a flat snout some eight or ten feet across, looking like a peat stack. Eye small, surrounded with lashes, some 16 feet from the snout. Blower covered with a flap a foot long. Under jaw slender, shorter than the upper, in it were thirty-six teeth shaped like the ends of ducks' eggs. No teeth were visible in the upper jaw. The whale could not be short of 60 feet in length.

My attention having been directed by Sir Robert Christison to the newspaper report, I at once recognised from the form of the head, jaw, and teeth, that the characters were those of the sperm-whale (*Physeter macrocephalus*), and I determined, if possible, to obtain a portion, if not the whole of its skeleton. The distance, however, of the spot, where the carcase was lying, from human habitations, and the want of proper appliances for lifting heavy objects, have proved hindrances to the removal of the huge cranium of the animal, but the two halves of the lower jaw, and a number

of the smaller bones of the skeleton, are now in my possession.

From the examination of these bones an estimate may be formed of the age, size, and, I believe, also the sex of the animal.

The state of ossification of the bones proved that the animal had reached its full period of growth, for the epiphysial plates were anchylosed to the bodies of the vertebræ, the lower jaw had attained a great length, the radius and ulna were anchylosed together, both at their upper and lower ends, and the various subdivisions of the sternum were welded into one massive bone.

As some estimate may be formed of the size of the animal from the dimensions of its lower jaw, it may be useful to give the measurements of this bone, and at the same time to compare it with the dimensions of some other specimens.

In the Natural History department of the Edinburgh Museum of Science and Art is a magnificent lower jaw, which was presented many years ago by Captain William Hardie. It possesses twenty-five teeth on one side, but only twenty-four on the other. On the outer face of the right mandible there has been engraved a large picture of the boats of the ship "Woodlark" of London, Captain William Hardie, engaged in the capture of the sperm-whale, in a school of sperm-whales, off the Banda Islands, April 7th, 1843. On the other half, a figure, 43 inches long, of a sperm-whale has been engraved. As authentic drawings of this animal are by no means common, and as this figure has been executed with a considerable amount of artistic skill, and in all probability by one well acquainted with the form and proportions of this animal, I produce on the following page a reduced copy. In the Anatomical Museum of the University of Edinburgh is the mandible of a young male, presented two years ago by my pupil, Mr F. B. Archer of Barbadoes. The animal was captured in the North Atlantic Ocean, in the latitude of the Azores.

Professor Flower has also carefully recorded * the dimensions of three specimens from Tasmania, in the Museum of the London College of Surgeons, one of which is stated to be "unique on account of its great size," and in measuring the Edinburgh specimens I have followed his plan of taking the length from the apex

* Trans. Zool. Soc. 1868.

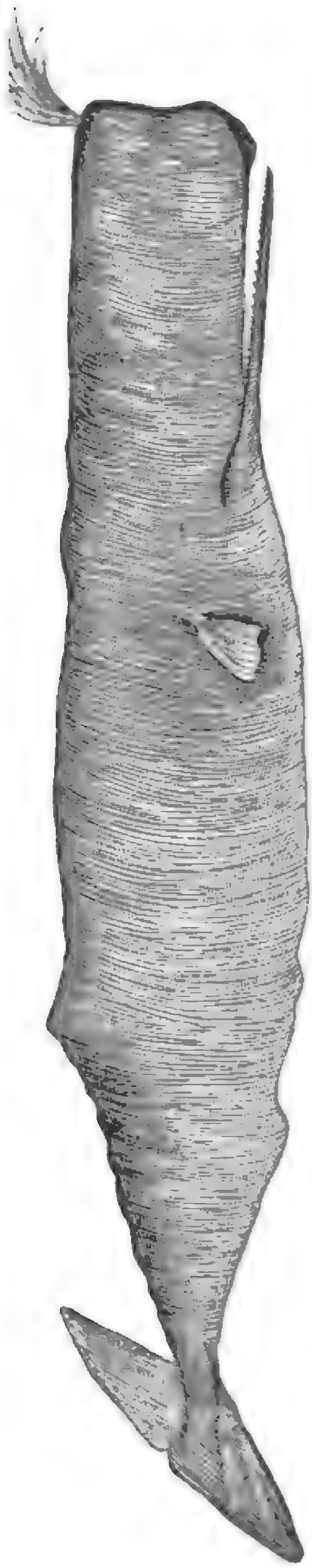


Fig. 1.

Reduced copy of the figure of a Sperm Whale, engraved on the mandible in the Museum of Science and Art, Edinburgh.

of the mandible to the middle of a line drawn across the posterior ends of the rami.

	Entire Length.	Length of Symphysis.	Greatest Girth Behind.
Mandible from Isle of Skye,	190½″	116″	56″
Proportion, ...	100	61	29
Mandible in Natural History Museum,	196	120	54
Proportion, ...	100	60	27
Mandible in Anatomical Museum,	80	38½	29
Proportion, ...	100	48	36
			Width Behind.
Mandible, young skull, Tasmania,	49	21	31
Proportion, ...	100	43	63
Mandible, Tasmanian Skeleton,	174	102	72
Proportion, ...	100	59	41
Largest Tasmanian Mandible,	194	124	75
Proportion, ...	100	64	38

The specimens in the Edinburgh Museums corroborate the conclusions arrived at by Mr Flower, that a gradual increase in the length of the symphysis, compared with that of the entire jaw, takes place as age advances, and it is obvious also that the girth behind diminishes in proportion to the increase in the length of the jaw. This increase is without doubt co-ordinated with the development and growth of the teeth.

Although the teeth had been removed by the fishermen, and sold to tourists before the mandible of the Skye sperm-whale came into my possession, yet the sockets were entire, and twenty-four on each side could be counted, so that the animal had attained its complete dentition. Seven loose teeth were, however, sent, all of which, with one exception, were worn on the surface and sides of the crown. In all, the pulp cavity was completely closed at the extremity of the fang, and, in several, irregular outgrowths from the surface of the fang were present. Two of the teeth, though worn at the crown, closely corresponded in general form with the one not so affected, and were much more slender and tapering than the remaining four, the roots of which were much more bulky. The unworn tooth was five inches long, and the greatest circumference of its root 4¼ inches.

The sternum was a massive, plate-like, triangular-shaped bone, greatly expanded anteriorly in its transverse diameter, and gradually tapering backwards to a rounded apex posteriorly. Inferior surface, convex; superior, concave; anterior border, convex; lateral borders varied in thickness, but were from four to five inches in diameter at the thickest part. Four well-marked costal articular surfaces on each lateral border. An oval hole, 6½ inches long, was in the middle of the manubrial element of the bone, and 4½ inches further back a much smaller foramen pierced the entire thickness of the bone. From this smaller hole a mesial and two lateral grooves passed for some inches backwards along the inferior surface of the bone. On the inferior surface there was no indication of the original transverse segmentation; on the superior surface, 19 inches in front of the posterior end, a deep transverse fissure passed across the bone through the middle of the third pair of costal articular facets, but there was no trace of the original division between the first and second segments.

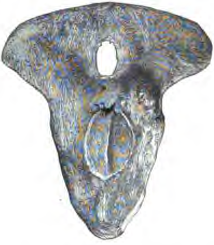


Fig. 2.

Inferior surface of the sternum of the Skye sperm-whale.

Extreme length of sternum, 50 inches; transverse diameter at

first pair of costal facets, 40 inches; at second pair, 22 inches; at third pair, 18 inches; at fourth pair, 14 inches. This bone had attained a more complete stage of ossification than had previously been described or figured in the sternum of this cetacean.

The length of the third transverse segment of the sternum being 19 inches, I examined it carefully to see if any evidence of a subdivision into smaller segments could be detected, but without success. Moreover, I find that Professor Flower has met with

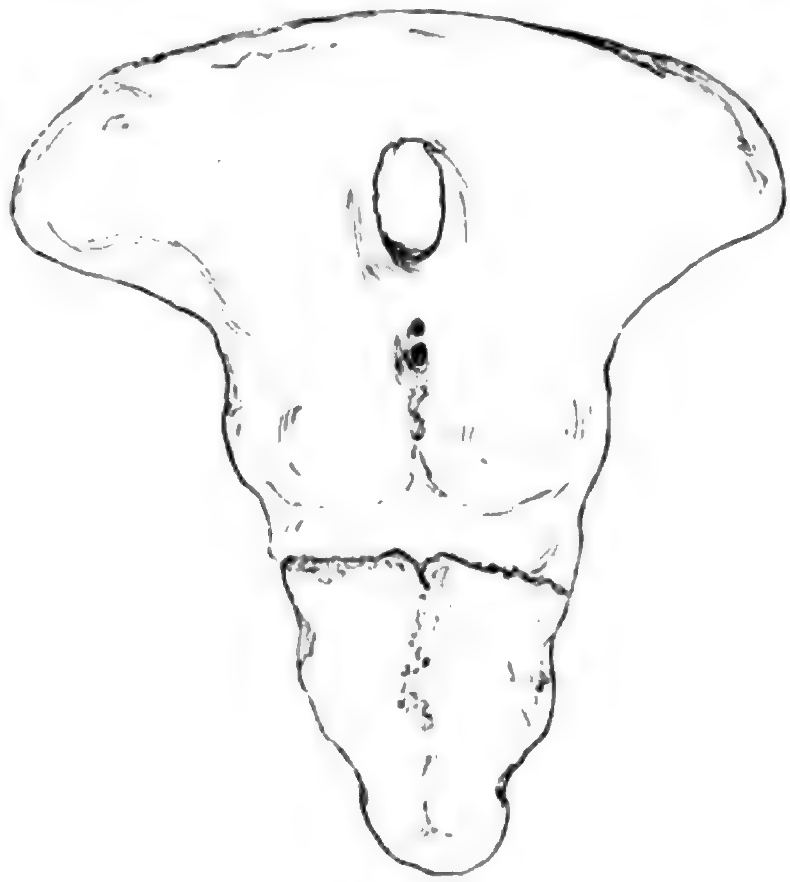


Fig. 3.

Outline sketch of the superior surface of the sternum of the Skye sperm-whale.

great differences in the length of the terminal segment of this bone in the specimens which he has examined. In one from Tasmania the length was 14¾ inches, whilst in the Caithness Cachalot the hinder piece is represented by a median spheroidal nodule of bone, 4 inches in diameter, imbedded in dried cartilage. The terminal piece of the sternum is therefore variable in its dimensions, and the greater length in the Skye specimen is without doubt due to the age of the animal having rendered possible complete ossification of the terminal cartilage.

That the animal had reached its full growth and attained the

adult period of life is evident from the completed ossification and the dimensions of its bones. There can be, I think, little doubt but that it was of the male sex. For although little has been done in the descriptions of the sperm-whale to discriminate the sexual characters of the skeleton, yet those who have had opportunities of observing the habits of this cetacean, agree in ascribing to the male a much greater magnitude than is acquired by the female. That excellent naturalist, Mr F. D. Bennett, for example,* states that the adult female does not exceed the length of thirty, or at most thirty-five feet.

We may now pass from the most recent specimen to the consideration of, I believe, the most ancient relic of the sperm-whale which has yet been found in the British Islands.

In August 1871, Mr George Petrie of Kirkwall presented to the Royal Scottish Society of Antiquaries a tooth recently obtained from a "brough" near the Howe of Hoxa, in the Isle of Sh. Ronaldsay, on a promontory opposite the Bay of Scapa. This tooth had obviously been buried in the earth for a lengthened period, and in all probability was co-eval with the early occupation of the "brough," and may have belonged to one of its early Norse, or even still more ancient inhabitants. This tooth has been carefully examined by Professor Duns, Dr John Alexander Smith, and myself, and we all agree in regarding it as the tooth of a sperm-whale. A part of the alveolar end of the tooth, more especially on one side, has been broken away, so that the conical-shaped pulp-cavity is fully exposed. The free end of the crown is smooth and rounded, such as one sees in specimens of well-worn teeth of this animal. The length of the tooth is 5¾ inches, but, owing to a part being broken off, this does not give its full length; the greatest girth is 6¾ inches.

Mr Petrie has most courteously sent me an account of the locality in which he discovered the tooth. He says:—"I was glad to find that the tooth was of some interest. I was led to its discovery by a request of my friend, Mr James Fergusson, the author of the 'Handbook of Architecture,' to make some excavations in the vicinity of the Howe of Hoxa, with the view of discovering, if

* Whaling Voyage, vol. ii. p. 155.

possible, the tomb of the celebrated Orkneyan Jarl, Thorfinnr who was, according to the 'Orkneyinga Saga,' buried at Haug seið, now known as the Howe of Hoxa. The Howe is apparently a long-shaped natural mound of considerable height, on which artificial mounds were probably made, as traces of them can still be seen, as well as of a massive stone wall encircling a great portion of the top of the mound. On the north end of the mound are the ruins of a large circular structure, which, on being excavated between twenty and thirty years ago, was found to be the remains of a brough or round tower. On proceeding to the spot last summer, and carefully examining the mound, I found that it would involve much time, labour, and expense to make a satisfactory examination. I determined, therefore, to excavate a smaller mound, evidently wholly artificial, at a short distance from the Howe of Hoxa, but connected at one time with it, as traces of an avenue of stones leading from the one to the other were still to be seen. I expected to find a chambered tomb, but to my surprise a structure resembling the ordinary brough, but far less symmetrical than such buildings usually are, was revealed. I am inclined to think that it was sepulchral in character, although of a type unique, so far as my experience goes. The passages or galleries were still roofed in many parts by flagstones laid across from wall to wall. The excavations did not produce many relics, but amongst these were bits of dark pottery and several vertebræ of whale much scorched by fire. One of the vertebræ, about 1 foot in diameter at the broadest part, and 9½ inches in height, had been fashioned into a rude vessel by scooping out the central or more porous part of the bone, as is often the case. It was found about two feet beneath the surface of the mound at A, on what appeared to be the floor of the interior of the structure, and it and the other vertebræ were buried beneath the ruins, which seemed to have fallen upon them. The tooth was found at B, and not far off a piece of freestone, convex on one side and slightly concave on the other. The concave side was tolerably smooth, apparently due to friction of a freestone rubber passing frequently over its surface. Similar stones were found in the brough of Hoxa, when it was cleared out some years ago. They much resemble the slightly hollowed stones found at New Grange, in

Ireland. I do not remember any case of a brough which has been explored in Orkney in which bones of the whale have not been found."

"I hesitate very much to attempt even to assign a date to the

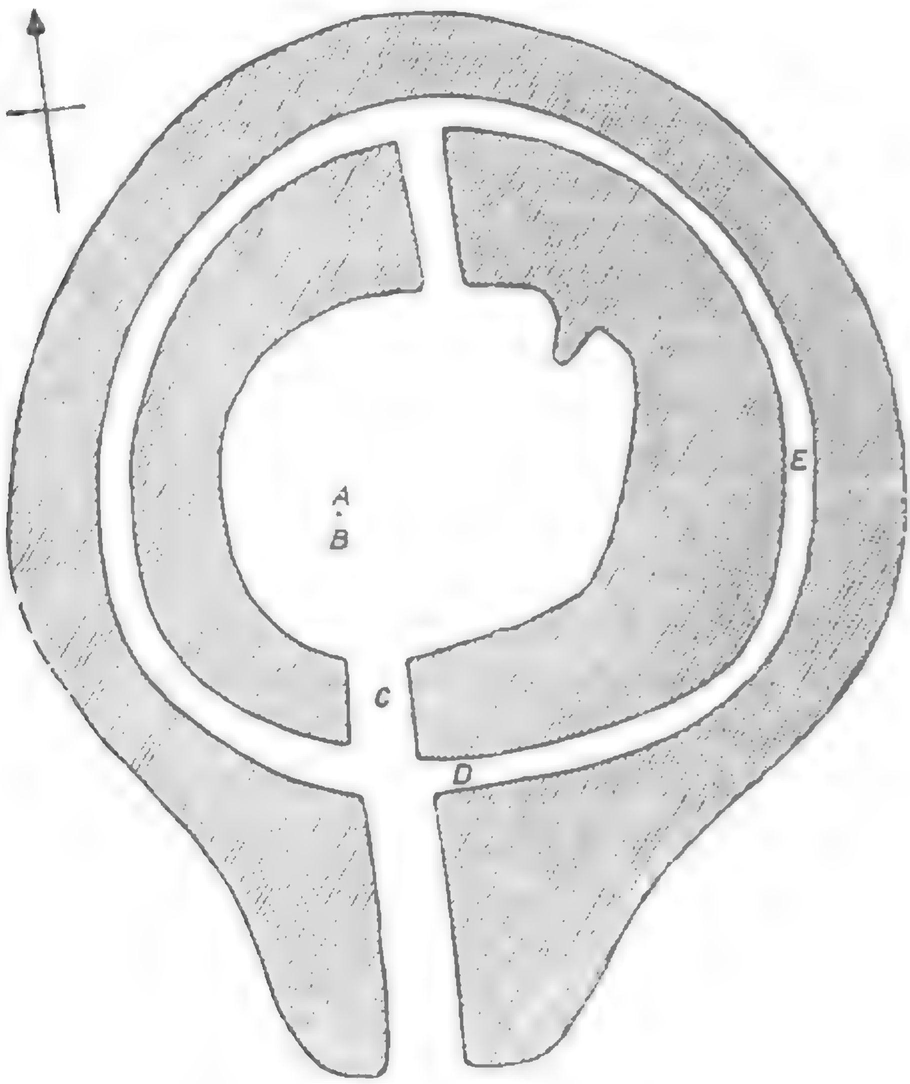


Fig. 4.

Ground Plan of structure near seashore at Hoxay, about 110 yards westward of Howe of Hoxay, or Brough of Hoxay. Ruins excavated and planned by George Petrie, Esq., Kirkwall, in summer, 1871. Scale, $\frac{1}{16}$th inch to 1 foot. A, the place where the broken vessel made out of the vertebra of a whale was found. B, the situation of the tooth of the sperm-whale. C, entrance doorway, which was roofed over with stones. D, passage, also roofed over. E, passage where stone roof was destroyed.

structure in which the tooth was found. It may belong to the period when the Celtic or Pictish population by whom the islands were occupied prior to their invasion by the Scandinavians, but I do not think, from the general appearance of the ruins and the character of the remains found in them, that the tooth belonged

to a whale captured or driven ashore later than the Scandinavian-Pagan period in Orkney, or say the ninth or tenth century."

As bearing on the early history of the sperm-whale in the British islands, I may next refer to a passage in a memoir by the eminent Norwegian archæologist, Professor P. A. Munch, to which my attention has been directed by Mr Joseph Anderson, the curator of the Antiquarian Museum. The memoir is entitled "Geographical Elucidations of the Scottish and Irish Local Names occurring in the Sagas,"* and on pp. 128, 129, Professor Munch, in his account of the Shetland Isles, says:—"The island of Yell is nearly divided into two halves by the deep fiords which penetrate on each side, Whalefirth (Hvalfjörðr) on the west, and Reafirth (Reyðarfjörðr) on the east. In a deed dated May 19, 1307, which speaks of the pledging of the estate Kollavâgr, now Cullavoe, one of the witnesses is a Högni i Reyðarfirði. This Reyðarfjörðr is clearly the above Reafirth, early contracted, or rather corrupted, even by Norse speakers, to Rafjörd." Further, Professor Munch states, it is very suitable that the two opposite fiords should be called, the one Hvalfjörðr and the other Reyðarfjörðr, for Reyðr (now called Röðr or Rör, in Norway), is also a kind of whale, the *Physeter macrocephalus*, black-headed spermaceti whale.

If we are to accept this interpretation by Professor Munch, that the old Norse term Reyðar was equivalent to our sperm-whale, then we should have to assume that this cetacean was so well known to the ancient Norsemen that they had coined a word to designate it. And it is indeed not improbable that, considering their roving habits, they may have sailed in the seas which it most usually frequents, and perhaps have chased it for the sake of its valuable oil.

But from the association of this name with a particular firth in the Shetland group of islands, it would, granting the accuracy of Munch's interpretation, seem as if, in the early years of the Norse settlement, the sperm-whale had not unfrequently entered this firth, and perhaps been captured there—a circumstance which would show that this animal was then a much more frequent visitor of

* Mémoires de la Soc. Royale des Antiquaries du Nord, 1850-1860, Copenhague.

the Scottish seas than we know it to be at the present day, or indeed to have been for some centuries past.

But I think it very questionable if the interpretation given by Professor Munch of the old word Reyðar can be regarded as zoologically correct. Torfæus, the historian of Greenland, in his account of the cetacea which frequent the Greenland and Iceland seas,* uses the term Reidr three times in his description of these whales. One he terms Hrafnreidr, white in colour, of a length of fourteen or sixteen cubits, "branchiis etiam præditus," and tastes well. A second, called Hafreidr, a whale of sixty cubits, or a little more, which carries a small horn, and is most pleasant to eat. The third is named Reidr, or most usually Steipireidr, which, he says, surpasses all others in sweetness, is gentle, and not to be feared by ships. The largest which has been caught by the Northmen equals 130 cubits, is very fat, "branchiis gaudet," but wants teeth. This description by Torfæus is much wanting in precision, and the statement that the Hrafnreidr and Reidr possess branchiæ would lead one to say, if this term were understood by him in the sense in which it is now employed, that these animals were not whales, but fishes. It is probable, however, that the so-called branchiæ in Hrafnreidr and Steipireidr may be the plates of whalebone which depend from the roof of the mouth of the baleen whales, and which have a laminar arrangement not unlike the gills of a fish, and might readily be mistaken for such by an inexperienced observer. The absence of teeth, however, conclusively shows that these could not be sperm whales.

Otho Fabricius, in his "Fauna Groenlandica,"† identifies the Hrafnreidr of Torfæus with the fin-whale named by Linnæus *Balæna boops;* and the Reidr or Steipereidur with the *Balæna musculus* of the same naturalist. By Otho F. Müller,‡ the term Reider or Reydur is applied to two species of Baleen whales. Mohr also, in his Natural History of Iceland,§ adopts the classification of Fabricius; and Erik Jonsson, in his Dictionary of old Norse terms,|| accepts the definition of the above naturalists. Further,

* Gronlandia Antiqua, pp. 90, 96. Havniæ, 1706.

† Hafniæ, 1780, p 36, *et seq.*

‡ Zoologicæ Danicæ prodromus. Hafniæ, 1776.

§ Forsog til en Islandsk Naturhistorie. Copenhagen, 1786.

|| Oldnordisk Ordbog. Copenhagen, 1863.

both the lexicographer and the naturalists agree in giving as the Norse equivalent for our term sperm-whale, not Reyðar, but Búrhvalr. Munch himself, also, by putting the Norwegian term Röhr or Rör as equivalent to the older word Reyðar, supplies me with an additional argument against the latter word being regarded as signifying sperm-whale, for Rör or Rörhval is merely our term Rorqual, *i.e.*, a whale with folds and sulci extending longitudinally along the belly, such as one sees in the Balænopteridæ or Finner whales, but which do not exist in the sperm-whale.

Hence we cannot regard Reafirth in Yell as having received its name from having once been a place of resort for the sperm-whale, or as affording any evidence that our seas were at one time more largely frequented by these huge cetaceans than at the present day.

But though this name loses its interest in connection with the natural history of the sperm-whale, it acquires importance in reference to the natural history of the rorquals. Of this group of whales, two, viz., the common Finner, and the species of Fin whale, of which we had recently so fine a specimen stranded at Longniddry, attain a length of upwards of 60 feet, and are not uncommon in our seas. By modern zoologists, the common Finner is usually called *Balænoptera musculus* (*Physalus antiquorum*), and may be identical with the Hrafnreidr of Torfæus. The other, the *Balænoptera Sibbaldi*, has been identified by Professor Reinhardt and myself * as identical with the Rorqual, to which the Icelanders even at the present day apply the name of Steypireythr. In all probability the firth on the east side of Yell, now known as Reafirth, was frequented by these Rorquals, and was named by the ancient Norse settlers, Reyðarfjörðr, from this circumstance, whilst the deep inlet of the sea on the west side of the island, now known as Whale-firth, may have obtained its Norse name of Hvalfjördr from having been the resort of the "caaing" whale, which in large herds still frequents the Orkney and Shetland seas, and is killed in great numbers by the islanders.

For convenience of reference, I may append a tabular statement, compiled from the cases referred to in this and my former essay,

* See my Memoir in Trans. of this Society, p. 247, 1870.

of the well-authenticated instances in which the sperm-whale has been met with on the Scottish coasts.

Locality.	Date.	Authority.
Hoxay, Orkney, ...	9th or 10th cent.?	George Petrie.
Limekilns, ...	Feb. 1689	Sir R. Sibbald.
Cramond, ...	1701	James Paterson.
Monifieth,*	Feb. 1703	Sir R. Sibbald.
Ross-shire, ...	1756	Sir W. Jardine.
Cramond, ...	1769	James Robertson.
Hoy Sound, Orkney, ...	About 1800	George Low.
Oban, ...	May, 1829	William Turner.
Thurso, ...	July, 1863	J. E. Gray, and W. H. Flower.
Loch Scavaig, Skye, . .	July, 1871	William Turner.

Monday, 5th February 1872.

SIR WILLIAM THOMSON, Vice-President, in the Chair.

At the request of the Council Professor Tait gave an Address on Thermo-Electricity.

The following Communication was read:—

1. Note on Cystine. By James Dewar, F.R.S.E.

The following observations on Cystine are a continuation of those formerly communicated to the Society by Dr Arthur Gamgee and myself, during the course of the Session 1869-70, and reprinted with addition in the "Journal of Anatomy and Physiology" for that year; and although really little of a novel nature to present to the Society, still it is necessary to show some additional facts have

* In connection with this animal, I may refer to an essay in the "Scottish Naturalist," dated November 1871, by Mr Robert Walker, of St Andrews, in which he describes and figures the vertebra of a whale, in the University Library of that city, which he believes to be the tenth dorsal of a youngish Cachalot. He believes it to be a relic of a whale stranded there, from which Mr Foster, a former Regent in the University of St Andrews, obtained a parasite which he sent to Sibbald, who figured it. He thinks that the whale figured on the same plate, though stated to be stranded at Monifieth, may have been this animal.

been observed tending towards the synthesis of this interesting substance.

The most important fact ascertained with regard to the chemical relation of cystine in memoir referred to was the production of pyruvic acid, when it was treated with nitrous acid. In this reaction the amido residue was not alone eliminated, the sulphur also separating as sulphuric acid, however carefully the experiment was performed. The fear of allowing the action to proceed too far, on the necessarily small quantity of substance operated upon, prevented us from purifying the product thoroughly, and, consequently, the analysis differed slightly from that of pure pyruvic acid. We had no hesitation in saying, however, the acid agreed better with the chemical characters of the syrupy modification of pyruvic acid than with that of Wischelhaus's carbacet oxylic acid, that we had anticipated would be produced, and that in all probability cystine would be found to be an amido-sulpho pyruvic.

If cystine is directly related to pyruvic acid, it must contain five instead of seven hydrogen atoms (and this supposition agrees well with the published analysis). The formula of the compound will then be, $C_3H_5NO_2S$. On this supposition, we may derive from pyruvic acid at least five isomers, that will all have the general characters of cystine, although there are many other possible constitutional formulæ.

Pyruvic Acid.	1.	2.
CH_3	CH_2NH_2	CH_2NH_2
CO	CO	CO
$CO.OH$	$CO.SH$	$CSOH$
3.	4.	5.
$CH_2\,(NH_2)$	$CH\begin{matrix}NH_2\\SH\end{matrix}$	CHS
CS	CO	$C\begin{matrix}NH_2\\H\end{matrix}$
$CO.OH$	$CO.OH$	$CO.OH$

Of the five possible cystines formulated, it is impossible to select that of the natural substance, because of our ignorance of the intermediate sulpho-acid. All attempts to replace the amido group alone by the action of nitrous acid having failed, I have tried several experiments, with the object of replacing the sulphur alone, with the small quantity of cystine at my disposal.

If cystine is one of the above five substances, the replacement

of the sulphur by hydrogen will generate very different bodies. Theory enables us to predict that, in the case of bodies having the constitutional formulæ of No. (5), we ought to obtain alanine. In that of (3) (β) alanine, and in that of (4) amido-lactic acid (serine), and in that of (2) amido-glycerine; whereas it is difficult to imagine the sulphur in (1) being replaced. A successful experiment in this direction ought to restrict the selection to two possible constitutional formulæ in the worst case, and synthetical processes might then be attempted. It was formerly observed that nascent hydrogen generated in an acid solution, readily liberated sulphuretted hydrogen, and might be used as a test for this substance. The action goes on, however, very slowly, and it was found extremely difficult to get anything like the theoretical quantity of sulphur evolved. With this experience, sodium amalgam suggested itself as being more powerful, and equally likely to act. When cystine is dissolved in caustic soda, and sodium amalgam added, in a few minutes it is easy to detect the presence of a sulphide by the nitro-prusside test. The action was allowed to proceed for several days, being occasionally rendered acid by the addition of hydrochloric acid; and the amalgam renewed. Ultimately the alkaline solution, after being neutralised with hydrochloric acid, was evaporated and treated with boiling alcohol to separate the chloride of sodium, and to dissolve any hydrochlorate of alanine that might be formed. After the filtrate was evaporated, the residue still contained sulphur, from the presence of hydrochlorate of cystine. This was separated by treating with water, and the filtrate was boiled with oxide of lead, treated afterwards with sulphuretted hydrogen to precipitate the dissolved lead, and evaporated. The residue was then heated to 200 C. in a tube, with the object of subliming the alanine. No crystalline sublimate was observed; it is probable, therefore, that substances of the constitutional formulæ of 5 do not express the constitution of normal cystine. This result is subject to a certain amount of reservation, from the difficulty of separating a small quantity of substance from a very large amount of secondary material accumulated in the course of the experiment. The battery is far better adapted to give a supply of nascent hydrogen in this case: and an experiment made in this way looks promising, if sufficient material was to be had.

The small quantity of substance left I have employed for the purpose of corroborating the production of pyruvic acid, when it is treated with hydrate of baryta.

Took a decigramme of cystine, treated it in a tube with a solution of hydrate of baryta, and heated it all night to a temperature of 130° C., opened it, and transferred contents to a beaker, boiled to expel the ammonia produced, then added an exactly equivalent quantity of sulphuric acid, filtered from the sulphate of baryta; after boiling to expel the sulphuretted hydrogen, the filtrate evaporated contained a yellow syrupy acid, which contained a few crystals under the microscope, having the appearance of Finck's uvitic acid. Ammonia was added, and gave a yellow solution, which was evaporated on the water-bath; it was dissolved in water, and gave a white precipitate, with nitrate of silver, which was not distinctly crystalline; it also gave a white precipitate with subnitrate of mercury, and a red colour with a crystal of sulphate of iron, and no precipitate with sulphate of copper. The barium salt was also found to be non-crystalline, the acid lost the power of giving a red colour with Ferric salts after treatment with sodium amalgam, and the composition of the silver salt agreed better with pyruvic acid then formerly.

Considerable progress has been made in an examination of the chemical characters and relations of the thio-pyruvic acids. Normal thio-pyruvic acid has been obtained from the di-chlorpropionic ether. When this ether is treated with excess of alcoholic sulphide of potassium, we obtain at once a precipitate of chloride of potassium, and a solution of the potash salt of the new acid. When this is diluted with water, acidulated with sulphuric acid, and shaken up with ether, the acid is obtained in yellow crystalline plates, part of it seems to remain a viscid fluid. The lead and silver salts are white and insoluble, blacken when heated. It precipitates mercurous salts black from the first. The calcium, barium, iron, cadmium, and copper salts are all soluble. The potassium and sodium salts are intensely yellow, and decompose slightly on exposure to the air. When treated with tin and sulphuric acid, they evolve sulphuretted hydrogen.

The thio-carboxyl pyruvic acid has not yet been obtained in a pure state. When pyruvic acid tr eated with pentasulphide of phosphorus, a violent action takes place, associated with much

frothing; and when the product is distilled, a large mass of carbon is left in the retort, and a very small quantity of distillate is obtained. It is probable that chloro-pyruvil, when treated with sulphide of potassium, will give a more satisfactory yield. It is the author's intention to make a careful comparison of these two acids, and to transform them into amido-acids, with the object of making an artificial cystine; and the results arrived at will shortly be communicated to the Society.

The author's stock of cystine being now exhausted, he will feel extremely indebted to any one who would spare him a small quantity for experimental purposes.

The following Gentlemen were elected Fellows of the Society:—

George Forbes, Esq., B.A., St Catherine's College, Cambridge.
J. Lindsay Stewart, M.D., Conservator of Forests, Punjab.
Rev. Charles R. Teape, M.A.

Monday, 19th February 1872.

Principal Sir ALEXANDER GRANT, Bart., Vice-President, in the Chair.

The following Communications were read:—

1. Remarks on Contact-Electricity. By Sir William Thomson.

2. On the Curves of the Genital Passage as regulating the movements of the Fœtus under the influence of the Resultant of the Forces of Parturition. By Dr J. Matthews Duncan.

The observer of the current literature of Midwifery finds nothing more characteristic of it than the number of papers on the mechanism of natural parturition. These papers indicate for the most part an enlightened zeal, for they are engaged with a most important branch of this mechanism, namely, the mode of action of the force of labour upon the fœtus and upon the passages, and the explanation thereby obtained of the changes which take place in these as natural labour advances.

For these inquiries great additional value would accrue, were the amount of power exerted by the combined forces of parturition

well known; but they can be carried on to a great degree of advancement, even while the amount of power exerted by the machine is unknown, or at least unsettled.

Some of these inquiries as to the action of the force of labour upon the fœtus and passage are very easily solved, and have been long in this condition. But the most, and by far the most, important are questions only recently raised; and of which it may be said that few are familiar to the profession even as questions, and still fewer can be regarded as settled. These inquiries form the natural sequel to the most recent developments of our knowledge of natural parturition. These have been chiefly engaged in describing how the fœtus and the passages actually behave during the process, while the new inquiries are destined to explain why they so behave. These new inquiries will introduce us far more deeply into the subject of the mechanism of labour than those which have preceded them. They are specially difficult because of the varying conditions of the force of labour and of the correlated parts, the fœtus and the passage. The former has the relations of its parts extensively changed while the process of labour proceeds, and the latter is only produced at the time by what is called the development of parts, as the fœtus advances.

The subject to which I wish at present to direct attention is the curves of the genital passage, and their influence on the phenomena of parturition.

I. The first curve to which I direct attention is said to be at the brim of the pelvis, and to have its convexity directed downwards and forwards. I do not admit that the curve exists, but it is of the utmost importance to decide the point, because, without doing so, we cannot possibly determine the primary direction of the driving force of labour. Hitherto and now, the axis of the gravid uterus has been and is generally regarded as coincident with the axis of the brim of the pelvis, and to indicate the direction of the resultant of the forces of parturition. But an elaborate attempt has been recently made by Schatz and Schultze, especially by the former of these authors, to demonstrate that the axis of the uterus at rest and in action is inclined to the axis of the brim of the pelvis, at a small angle opening forwards and upwards, and of about ten degrees. I have just said that the axis of the uterus has been generally considered to indicate the primary direction of the driv-

ing power; but it is evident that this can only be the case if a variety of conditions be satisfied. Of these the following are probably principal:—the assistant driving force, which is auxiliary to the proper uterine force, must be also directed in the axis of the brim of the pelvis, being supposed to be uniformly applied to the uterus by the circumjacent viscera and parts, acting like a fluid, exerting pressure equally in all directions: the uterus must be distended with a fluid which is copious enough to prevent any part of the walls being specially pressed upon or indented by the fœtus; or, it must have its tendency to become spheroidal superiorly unrestrained. Now Schatz, in addition to giving the proper uterine driving force a posterior inclination to the axis of the brim by ascribing to the uterine axis such an inclination, still further increases the inclination of the whole driving force, by describing the special direction of the auxiliary bearing-down driving force as still more inclined than the direction of the uterine axis. The resultant of the combined or whole driving forces will of course, according to Schatz, have a direction somewhere intermediate between that of the uterine and that of the auxiliary driving forces.

Smellie's authority is much relied upon in support of the existence of this curve. In his plates he gives the uterus this inclination to the axis of the brim of the pelvis, both in natural cases and in cases of deformity; but this is not satisfactory evidence as to what he believed, for it is probable that in preparing his plates he did not pay particular attention to the point. Those of them to which reference is here made (as xii. and xiv.) are not in the proper sense drawings or pictures, but mere plans, and might very well have been arranged as they are, merely because in other respects the works looked well. Dr Barnes, in his recent work on obstetric operations, while adhering to the generally entertained view as to the coincidence of the axis of the uterus and of the brim of the pelvis, implies, by his descriptions and drawings, a belief that, in most if not all cases of antero-posterior contraction of the brim of the pelvis, the uterine axis is inclined to the axis of the contracted brim, as Schatz believes it to be in cases generally. This is not the place for any full criticism of what Barnes very aptly calls the curve of the false promontory, because I confine myself to ordinary or natural conditions. I shall merely say that this important and practically valuable doctrine of Barnes regarding the curve of the false promon-

tory is made too general. It can be true and applicable only where the posterior uterine obliquity is present, and it is not demonstrated, nor is it probable that this always is so, in cases of deformity.

It is extremely desirable that means should be devised for ascertaining the direction of the resultant of the combined forces of parturition, and especially of the axis of the uterus in action. The means adopted by Schatz with this object in view are not satisfactory; they merely go the length of showing how carefully he entered upon the question. But it may be permitted me to state reasons which tend to establish the ordinary opinion, and to discountenance that of Schatz.

If the uterine axis is inclined to the brim of the pelvis posteriorly to its axis, we should expect to find the child's head at the commencement of labour, while yet above the brim, to be in a position which has never, so far as I know, been ascribed to it in natural cases. Smellie, in his plate xii., gives this position consistently, but not truly. He could not avoid doing so, unless he represented the child at rest as having a left lateral flexion of the head, which would be ridiculous. His mode of drawing the uterus with this posterior obliquity created an exigency for him, which he could get over only by what must be regarded as misplacement of the head. One error thus led him into another. The erroneous posterior uterine obliquity forced him to represent the left side as presenting in the very commencement of labour in an ordinary case of first cranial position with the occiput looking to the left. I do not see how the difficulty, Smellie's yielding to which gave rise to error, can be avoided, except by assuming that the ordinary view as to the axis of the pregnant uterus is correct.

At the same point where Smellie stumbled, Nægele also fell into error, but in an opposite direction. In his classical essay on the mechanism of birth, describing the first position of the fœtal head, he represents it as presenting at the brim of the pelvis, which it has not yet fully entered, more obliquely than when it has entered it, or as having at the earliest stage its perpendicular axis more inclined anteriorly to the axis of the brim; and in this way he accounts for his allegation that the right ear can generally be felt at this time without difficulty behind the pubic bone.* Here a

* See the work of H. F. Nægele, "Die Lehre vom Mechanismus der Geburt." Mainz, 1838, S. 12.

remark may be made similar to that applied to Smellie's drawing; namely, that the head could not be so placed unless the uterus had an anterior obliquity, an obliquity opposite in direction to that figured by Smellie and described by Schatz; an obliquity quite incompatible with Nægele's own description in his work on the female pelvis;* or unless the child maintained an unnatural and undescribed left lateral flexion of its head.

The now generally entertained views, that the axis of the uterus coincides with the axis of the brim of the pelvis, and that the fœtal head presents at the brim directly,† have at least the merit of evading such obvious and adverse criticism as the figure of Smellie, and the expressed opinions of Schultze, Schatz, and of Nægele, are liable to be subjected to.

The great authority of Nægele was long sufficient to give currency to his statement that the head of the fœtus, as it passed through the brim of the pelvis, had its vertical axis in a position of anterior obliquity to the plane of the brim, an obliquity which is appropriately designated the Nægele obliquity, in order to distinguish it from other obliquities at the same situation. The great argument against this view, and the only one having a final character, is, that it is not an accurate description of what takes place; but in addition, it has been argued against it that it is impossible to find a mechanism to account for it. Stoltz's attempt to explain its occurrence by mere lateral flexibility of the neck of the child is insufficient, because it affords no explanation why the lateral flexion is towards the posterior shoulder; but the now alleged posterior obliquity of the uterus, as regards the axis of the brim, affords a solution which Nægele did not foresee when he described this obliquity as present and increasing with the increasing height of the head in or above the true pelvis. If, adopting the kind of nomenclature introduced by Barnes, we describe a curve of the natural promontory, produced at the brim of the pelvis by the posterior obliquity of the uterus, then this curve, representing a deflection of the axis to the extent of about ten degrees, can be easily made to account for the alleged Nægele obliquity during the first half of the passage of the child's head through the ligament-

* F. C. Nægele. "Das Weibliche Becken." Carlsruhe, 1825.
† See my "Researches in Obstetrics," p. 334, &c.

ous pelvis. For, if we suppose with Schatz that the whole power of labour acts in an oblique line nearly corresponding to that of the axis of the uterus, or inclined still more posteriorly, then there will always be a tendency of the anterior half of the head, or of that which is nearer the concavity of the curvature of the passage, to descend first, and so produce the Nægele obliquity, if there be uniform resistance to the advance of all parts of the head. But, as the occurrence of Nægele's obliquity is now very generally denied, any mechanism which accounts for it derives little or no support of its own accuracy from the circumstance of its doing so.

Still another difficulty in the way of admitting the presence of the curve of the natural promontory as the natural or ordinary condition is worthy of consideration. It is justly held that in natural labour the advance of the head through the brim of the pelvis is impeded only by friction and imperfect dilatation or dilatability of the soft parts; but, if this curve of the natural promontory exists, a new and considerable difficulty is introduced, namely, the difference between driving a body through a curved and a straight passage—a new difficulty which it appears to me unreasonable to admit. And this is not all; for this addition of difficulty is not overcome and passed when the child's head has traversed the curve, but lasts during most of the process of the birth of the child. If this curve exists, the axis of the genital passage, regarded in the antero-posterior vertical plane, has the shape of a Roman S; its first or upper curve, the curve of the natural promontory, having its concavity looking backwards; its second and universally recognised curve having its concavity looking forwards. I believe we are nearer the truth when adopting the view at present generally entertained, that, in the antero-posterior vertical plane, the genital passage has ordinarily only one curve, having the concavity of its axis looking forwards.

Direct therapeutical bearings of this matter are evident and important both in natural and morbid parturition. Certain attitudes of the body, by increasing or diminishing the flexion of the iliac beams upon the sacrum, a movement which I have elsewhere described as nutation of the sacrum,* may alter not only the dimensions of certain parts, but also the relations of the axis of the

* Researches in Obstetrics, p. 148.

pelvic brim to the axis of the uterus, or to the direction of the resultant of the forces of labour. In an elaborate paper Schultze* has attempted to show that similar results may be produced by flexion and extension of the spine. This author assumes that the lower lumbar vertebræ govern the uterine axis, and that the latter is normally inclined posteriorly to the plane of the pelvic brim. He therefore recommends that when difficulty arises at the brim, the spine should be flexed so as to bring the axes of the uterus and of the brim, if possible, into coincidence; and if we admit his assumptions, there can be no doubt as to the justice of his conclusion. For practical application, however, the proper treatment may be stated in such a way as to offend no theory as to axes of brim or of uterus, or so as to stand good whatever view is held on these points. When, before labour, or while the fœtal head is still mobile above the brim, it is placed with its sagittal suture not traversing the centre of the brim, but lying anterior to it (as Smellie figures), then it will during early labour be pressed, with a loss of force, against the pubes, not directly into the brim. It will then be worth while to try whether flexion of the spine, by putting the woman into the attitude assumed in stooping forward, will correct the direction of the head [which I consider an unnatural direction]. If it corrects it, the sagittal suture will be observed to leave the neighbourhood of the pubes and approach or reach the middle of the plane of the brim. Again, if the uterine axis, or the resultant of the forces of labour, has this posterior obliquity to the axis of the brim, then, in the first half of its course through the ligamentous pelvis, the fœtal head may be expected to show the Nægele obliquity—that is, its half lying in the anterior half of the pelvis will be lower than that in the posterior as regards the plane of the pelvic brim, being pushed down with greater force; and it will be well worth while to try whether or not flexion of the spine will correct this direction of the head [which I consider an unnatural direction].

II. The second curvature of the pelvis, which I proceed to describe, is, like the former, situated at the brim of the pelvis; but

* Jenaische Zeitschrift für Medicin und Natur-Wissenschaft, iii. Band. S. 272.

of its frequent existence there can be no doubt whatever. Its presence is indicated by the deflexion of the uterus from the mesial line to the right or to the left; and it is well known to be observed at all times—that is, before, during, and after pregnancy; but as this paper is concerned only with dynamical matters, this deflexion or deviation is interesting only as observed during labour. On the direction of this deflexion, to right or to left, I have no remarks to make, but I may refer the student first to the recent paper on this subject by Winkler,* and then to the earlier observations of Spiegelberg † on this uterine position during labour. For my present purpose it is more important to have some idea of the amount of deflexion which occurs. With a view to ascertain it, however imperfectly, I examined a series of cases which I found to present this condition. I did not, in all of these cases, make out whether or not the deflexion persisted during uterine action; but I ascertained that it did so in some of them. I hope to make further observations on this point, but such an inquiry is not essential to my present purpose, it being sufficient to know that the deviation does generally persist during the so-called erection of the uterus in a pain.

I proceeded as follows. Having the pregnant woman lying flat on her back, I made out the position of the uterus by feeling its outline with my hands; this manipulation shortly induced a pain which made the uterine form more distinct than previously; and then I could observe the outline mark the projection of the direction of the axis on the skin, and notice its just incidence on the outline of the fundus. Then I measured off, as on a plane, the angle between the projection of the axis and the vertical line joining the ensiform cartilage and the symphysis pubis. I did not try to have guidance from feeling the uterine angles and the parts attached thereto, as Winkler has done in similar circumstances, because I thought that such guidance would not ensure greater approach to accuracy in the measurements I wished to make with a view to purely dynamical considerations.

This angle I found in five cases to be 8, 10, 11, 14, 15 degrees respectively, or on an average about 10 degrees. The problem now

* Jenaische Zeitschrift, iv. Band. S. 522. 1868.

† Monatsschrift für Geburtskunde, xxix. Band. S. 92. 1867.

to be solved, is to make out from this angle on the surface of the spheroid what is the corresponding deflexion of the axis of the spheroid; and since the angle, as measured low down on the surface of the abdomen lies in a plane nearly parallel to that in which the axis of the uterus is deflected from the antero-posterior mesial plane, the deflexion of the axis may be regarded as nearly identical in amount with the angle measured on the surface. It is probable that this angle of deviation of the axis of the uterus from the axis of the brim of the pelvis has important physiological and practical bearings; but as yet little has been made out regarding them. It has been looked upon as affording some explanation of the alleged comparative frequency of laceration of the cervix on the left side in ordinary labour.* But the most interesting application of it is to assist in accounting for the production of face cases.† It has been shown how, under certain conditions, and supposing a right lateral deviation of the uterus, the part of the head on the left side of the brim—that is, the seat of the concavity of the curvature, will have a greater tendency to descend—that is, to be more powerfully pushed downwards than the part on the right side of the brim. Of this there can be no doubt; and the probability of this being a true theory or explanation of face cases is highly increased by remarking the apt manner in which other things, known in regard to face presentations, adapt themselves to it.

Another ingenious dynamical theory of face presentation has been started by Schatz. He states it as follows:—"When the uterus alone is in action, or when there is also acting uniform resistance around by the walls of the pelvis, a cranial presentation always occurs, if the occipital foramen of the fœtal head at the time of the first more important shortening of the long axis of the uterus lies backwards from this towards the back of the fœtus, but a face presentation, if it deviates forwards from this towards the breast side of the fœtus. With the co-operation of non-uniform resistance by the walls of the pelvis, cranial presentation is produced if the occurring positive or negative distance of the great occipital foramen towards the back of the fœtus from the

* Edinburgh Medical Journal, June 1871, p. 1061.

† Edinburgh Medical Journal, May 1870.

long axis of the uterus multiplied into the positive or negative difference of resistance by the walls of the pelvis, is greater on the posterior side of the fœtus than the product of the same factors on the breast side. In the opposite circumstances face presentation is produced."* To all this ingenious theorising there can be no objection if the conditions are assumed. But the two chief premises are merely assumed; they are not shown to occur; they are not shown to be more likely to occur in face presentation cases than in others. Under these circumstances, I submit that there can be no hesitation in preferring the formerly described theory of face cases, where the corresponding assumptions or premises are not mere assumptions, but well-known facts; I refer to the occasional lateral deviation of the uterus, the occasional dolichocephalous condition of the head, and the greater liability of cases of the second or right occipital position to be transformed into face cases than of the first or left occipital position.

III. The last curve of the developed genital passage which falls to be considered is the most extensive and the best known. It is the great curve in the antero-posterior vertical plane, which begins about the middle of the third bone of the sacrum and extends through the outlet of the ligamentous pelvis to the outlet from the soft parts. Its length may be greatly diminished by rupture of the perineum, and still more if the sphincter ani is torn through. It forms a curve, whose amount of bending varies from about 60 to about 150 degrees.

In connection with this curve fall to be studied the synclitic and allied movements of the fœtal head during its progress, to which Kueneke has recently directed attention, and which have been so carefully discussed at home and abroad,† that it is unnecessary to re-enter upon them here.

In connection with this curve have also to be studied the development of the lower part of the genital passage, the greater development posteriorly where the force is particularly or more strongly applied, than anteriorly where there is little more than

* Der Geburt's Mechanismus der Kopfendlagen, S. 72.

† See Edinburgh Medical Journal, June 1870, and the American Journal of the Medical Sciences, October 1870, &c.

counter-pressure, or pressure against a fixed wall, and that chiefly during the temporary abeyance of the power of parturition. There is to be noted, also, in connection with this curve, the inevitable tendency of the force of labour, not merely to distend the perineum, but also to rupture it centrally, to force the presenting part through it; a tendency the study of which, apart from other considerations, leaves no possible doubt as to the expediency of the practice of supporting the perineum, a practice which can be demonstrated to favour the maintenance of its entirety.

A novel practice, founded upon what I regard as a misapprehension of the conditions of this curvature, has been recently much dwelt upon by Professor Schultze of Jena.* The practice has for its object to facilitate and promote the advance of the child after its head has reached the floor of the pelvis. It is proposed to effect this by extension of the spine, with a view to which a hard pillow is to be placed beneath the loins as the woman lies on her back. The extension of the spine he believes to increase the posterior obliquity of the axis of the uterus, and therefore of the force of labour as exerted in this part. By the change supposed to be thus effected in the direction of the axis of the uterus, the axis of the force of labour is brought more nearly to the direction of the axis of the outlet of the pelvis, whereby there is supposed to be produced a diminution of the otherwise necessary loss of power arising from the change of direction of the passage at this part. Schultze alleges that he has found this extension of the spine to be useful in practice. If this utility is confirmed and ascertained, nothing, of course, can be said against it. But for the enforcement of his recommendation of this practice, it is evident that he trusts chiefly to theoretical arguments; and, therefore, I proceed to examine them, and believe I shall show that they are fallacious. Before doing so, it is worth while to point out that the attitude recommended by Schultze is a very unnatural one, and that a woman straining in labour advanced to the stage at present under consideration naturally assumes an attitude nearly opposite to that implied by extension of the spine, an attitude of some degree of flexion, an attitude which, keeping in view the relaxed state of the sacro-

* See Jenaische Zeitschrift für Medicin, &c. Band iii., 1867, and Lehrbuch für der Hebammenkunst, 1870.

sciatic ligaments, may be accompanied by some degree of enlargement of the outlet by the posterior nutation of the apex of the sacrum.

To Schultze's theory of the facilitation of the latter part of the second stage of labour by extension of the spine several objections may be made. First, it is inconsistent with his views as to the facilitation of the entry of the fœtal head into the brim of the pelvis by flexion of the spine. That view is based upon the assumption that the child's head enters the brim of the pelvis so as pretty nearly to occupy it and have a nearly vertical axis in the axis of the brim. If this be true of the fœtal head at the brim, it will be true of it during its course, *mutatis mutandis*, and it will be true of that part of the body which occupies the brim when the child's head is pressing on the perineum. It will be impossible, therefore, by any change of the axis of the uterus to bring the line of the labour force to bear upon the perineum in the direction of a straight line as Schultze represents it. Second, the upper cylindrical solid portion of the ligamentous pelvis, having a length of at least an inch and a half, has a well-determined axis with which must correspond the axis of any body fully occupying it, if the body is of uniform consistence,—conditions with which the fœtus nearly complies. If this be the case, the direction of the force of labour will follow the same axis, and no change of its direction above the brim of the pelvis, however produced, can have any effect upon its direction in any part below the brim of the pelvis. Third, Schultze forgets that his practice is intended to produce or increase posterior obliquity of the axis of the uterus to the brim, to increase the supposed curve of the natural promontory, and that every additional degree of that curve necessarily produces additional loss of power. The more, then, he extends the spine he will diminish the power of labour available at the outlet of the pelvis, instead of increasing it, as he expects. Fourth, if Schultze's * views, as illustrated by his diagrams, are correct, a dangerous amount and direction of force would be brought to bear upon the perineum, a structure whose integrity is already sufficiently imperilled by a force whose direction is gradually changed as the fœtus passes through the lower half of the ligamentous pelvis.

* Lehrbuch der Hebammenkunst, fig. liii.

Before concluding the consideration of the great curve of the genital passage in the anteroposterior vertical mesial plane, it is necessary to point out an important difficulty introduced into its study by the change in the condition of the ovum when passing through it, as compared with the ordinary condition of the ovum when passing the pelvic brim. Hitherto I have spoken on the assumption that the ordinary view of the action of the power of labour holds good at all parts of the course of the child. This view is, that the power is uniformly applied by the concave surface of the approximately spheroidal uterus to the uniform surface of the approximately spheroidal ovum, in a direction corresponding to the axis of the uterus and of the developed genital passage. Now, this view is probably nearly correct so long as the membranes are unruptured, or while no special part of the fœtus impinges on the uterus so as to injure its approximately spheroidal form, and provided no part of the fœtus impinges on the passage so as to cause special friction or obstruction at the part impinging. But while the great anteroposterior vertical curvature of the genital passage is being permeated, this view is no longer tenable, although even then it may, in a confessedly inexact way, be advantageously kept in mind, if other more exact conditions are not stated. While the curve is being described, the membranes are generally ruptured and the waters more or less completely discharged; and consequently the fœtus is in a variety of places impinging on and changing the form of the propelling uterus, and meeting with frictional obstruction in the passage at special points more than at others. These changes introduce an amount of complication of the problem which damages greatly the value of such considerations as I have above adduced, and I see no means at present of overcoming it and of arriving at exactness, though there is probably no insuperable difficulty in the matter. Another element of confusion is introduced by the want of uniformity which exists in the composition of the fœtus as a mechanical body. It is especially to be noted that it contains a longitudinally-placed elastic beam of connected vertebræ, which lies nearer the surface of the mass at one side than at the other.

The ovum or fœtus, in its passage through the developed genital canal, is subjected in various circumstances to various rotations on

some more or less longitudinally directed axis. It is also subject, in various circumstances, to various revolutions or sinuous deflexions, in which its long axis moves through portions of curves which are measured by corresponding angles. On these curves and their influence I have made a few remarks while feeling deeply their imperfection and the need of much further observation and research. The student who has followed the argument in this paper will have observed the resort to inferences when direct observations would have been preferable. This remark applies to every subject discussed in it; and while it is to be greatly regretted that such is the case, it is at the same time not to be forgotten that no method of making direct and exact observations has hitherto been discovered. The adoption of the homalographic method is surrounded with difficulties, not only in the method itself, but also in the procuring of subjects on which to use it; and while results obtained by it would be of great interest and importance, it is evident that they would not be complete or sufficient, for they can never be other than observations on parts in the repose of death, not in the turgescence and action of life. Until very recently, all our knowledge of the force of labour was on a like imperfect footing; but already ingenuity has suggested a means of basing this subject on exact observations, and Schatz has availed himself of these means, and greatly assisted us to arrive at results which we regard as probably the most important hitherto achieved in obstetric science. Till some ingenuity has succeeded in devising means of making like exact observations to settle the points discussed in this paper, we must be content to do our best to reach the truth by reasoning on what we do know more or less exactly. And it should be remembered that, by this method, we may reach the greatest assurance, if not certainty. A boy, playing with his dissected puzzle-map, may be certain that a county is rightly placed if it fits exactly into an entire hole formed of the conterminous boundaries of surrounding counties, especially if it also fits in nowhere else. So a theory which suits itself to all, or is in opposition to none, of numerous known conterminous conditions, may be, provisionally at least, assumed to be correct, and such assumption of correctness will vary with the number and testing character of the conditions so humoured by the theory.

3. On a Method of Determining the Explosive Power of Gaseous Combinations. By James Dewar, Esq.

(Abstract.)

The author describes an apparatus by means of which the explosive power of gaseous combinations can easily be determined, and from this, by Bunsen process, the temperature may readily be calculated. The essential feature of the apparatus is the registration of the "compression volume" of a given initial volume of air, on which the gaseous explosive mixture has been allowed to act. As the duration of the pressure is all but instantaneous, the well-known formula

$$\frac{P_2}{P_1} = \left(\frac{V_1}{V_2}\right)^{1 \cdot 4}$$

may be employed to ascertain the final pressure, more especially as the sudden rebound prevents any great loss of heat. In order to test the apparatus many experiments were made with mixtures of hydrogen and oxygen, and the mean result arrived at was a condensation to one-fifth the original volume of air (the initial volume being measured at 30 in. bar), when pure electrolytic gas was employed. This is equivalent to a pressure of 9·5 atmospheres, and therefore agrees with Bunsen's previous determination. The author hopes to be able to execute a series of determinations under varying conditions of temperature and pressure.

4. Note on Sprengel's Mercurial Air-Pump. By James Dewar, Esq.

The ordinary Sprengel, requiring careful manipulation, and being apt to get out of order, has not yet become an essential piece of lecture apparatus as it ought to be. The author exhibited to the Society two modifications adapted to lecture illustration. In both instruments the mercury receptacle is made of iron, and instead of the india-rubber joint of the original, a well-ground iron stopcock is substituted, the portion of iron tube before the stopcock terminating in a Y-shaped piece bored out of the solid. In the one form the drop-tube is of glass, attached by means of marine

glue; in the other, of carefully made india-rubber tube four or five millimetres in thickness, of a very small uniform bore, made expressly for the purpose by the Edinburgh Rubber Company. The iron funnel-shaped receptacles are ground at the inner apex, so as to fit perfectly finely-ground iron tubes. By means of these tubes the preliminary exhaustions are made by a hand pump, and then they are withdrawn. This device saves a separate joint. The barometer tubes are attached to solid T-shaped pieces of iron tube, and between these pieces and the main tubes each has a small glass bulb. Both forms work for all practical purposes as well as glass, and suit admirably for Frankland's water analyses, and Graham's experiments, &c. They may be procured from Mr Cameron, philosophical instrument maker, South Bridge, Edinburgh.

5. Professor Alexander Dickson exhibited a large series of abnormal cones of *Pinus Pinaster* which were to form the subject of a future communication to the Society.

The following Gentleman was balloted for and admitted as a Fellow of the Society:—

ARCHIBALD CONSTABLE, Esq.

Monday, 4th March 1872.

PROFESSOR MACQUORN RANKINE, Vice-President, in the Chair.

The following Communications were read:—

1. On the Connection between Chemical Constitution and Physiological Action—*Continued.* On the Physiological Action of the Salts of Trimethylsulphin. By Prof. Crum Brown and Dr Thomas R. Fraser.

In the former parts of this investigation we studied the physiological action of the salts of a considerable number of ammonium

bases—that is, of the salts formed by the union of an ether with the *nitride* of one or more alcohol radicals. Thus—

$(CH_3)'_3N$	+	CH_3I	=	$(CH_3)_4NI$
Trimethylamine (Nitride of Methyl).		Iodide of Methyl.		Iodide of Tetramethyl-ammonium.
$(C_8H_{14})''(CH_3)N$	+	CH_3I	=	$(C_8H_{14})''(CH_3)_2NI$
Methylconia (Nitride of Methyl and $(C_8H_{14})''$).		Iodide of Methyl.		Iodide of Dimethylconium.
$(C_{21}H_{22}NO_2)'''N$	+	CH_3I	=	$(C_{21}H_{22}NO_2)'''(CH_3)NI$
Strychnia (Nitride of $(C_{21}H_{22}NO_2)'''$).		Iodide of Methyl.		Iodide of Methylstrychnium.

The examination of the physiological action of such salts proved that, while differing from one another in many respects, there are two points in which they agree—they all paralyse the end-organs of the motor nerves, and none of them possess that stimulating action of the spinal cord which we observe in such a substance as strychnia.

Some years ago Von Œfele discovered that the sulphide of ethyl forms a compound with the iodide of ethyl, exactly as the nitride of ethyl (triethylamine) does. To this new salt he gave the name of iodide of triethylsulphin, and from it obtained the hydrated oxide and various other compounds of triethylsulphin. The number of known salts of this type has been increased by Cahours and Dehn.

As there are two ways in which the salts of the ammonium bases may be represented,—1st, as molecular compounds of nitrides with ethers; and 2d, as compounds of pentad nitrogen,—so the salts of the sulphin bases may be represented, either, 1st, as molecular compounds of sulphides with ethers; or, 2d, as compounds of tetrad sulphur.

As our physiological observations had led us to prefer the second mode of representing the constitution of the salts of the ammonium bases, it appeared to us that it would be of interest to examine the physiological action of the salts of the sulphin bases. We have accordingly commenced with the simplest salts of this type, viz., the salts of trimethylsulphin, and have made a number of experiments with the iodide and the sulphate of that radical. The iodide was employed in the form of pure white crystals; the sulphate,

which is an excessively deliquescent salt, was employed in the form of an aqueous solution of known strength. We found that the action of the two salts was identical, the difference of dose being nearly proportional to the chemical equivalent. In the case of warm-blooded animals the symptoms observed were—increasing weakness of the voluntary muscles ending with fatal doses in asphyxia, considerable contraction of the pupils, and profuse salivation.

In the case of frogs complete paralysis of the voluntary muscles was produced, along with a remarkable stiffness of the muscles of the anterior part of the body. By experiments conducted exactly as described in former papers read before the Society, we proved that the paralysis of the voluntary muscles was caused by the destruction of the function of the motor end-organs, the nerve trunks and the muscular fibres being still active. In fact, the action of these salts is almost identical with that of the salts of tetramethyl-ammonium, as formerly described by us.

We intend to continue these investigations, and to extend them to the corresponding compounds of selenium and tellurium and to the remarkable series of salts derived from $Se(CH_3)_2Cl_2$ and $Te(CH_3)_2Cl_2$, such as $Se(CH_3)_2OHNO_3$, &c.

2. On the Mean Monthly Rainfall of Scotland. By Alexander Buchan.

So far as regards the annual amounts of the rainfall of Scotland, deduced from observations made at 296 different places, the chief point brought out is the enormous difference between the rainfall of the west and that of the east; the stations along the west coast showing such figures as 40, 45, and 54 inches, as compared with 24, 27, and 30 inches at stations on the east coast, not situated in the immediate neighbourhood of hills. When it is considered that the source of the rainfall is the prevailing south-westerly winds, it is evident that the comparative dryness of such districts as the south shore of the Firth of Forth is due to high land lying to the south-west, which drains the winds of a large portion of their moisture in their passage across them. On the other hand, in the West Highlands, where arms of the sea open in upon the land in all direc-

tions from south round to west, the case is that of a high district, with currents of moist air poured in upon it, and the consequence is, an enormous rainfall, amounting, for example, at Glencroe to 128 inches, and at the head of Lochlomond to 115 inches. Between these extremes the amount of the rainfall varies, the variations being dependent on the physical configuration of the surface.

The monthly average rainfall has been examined by the discussion of observations made at 126 places for long terms of years —the number of years varying from 10 to 60, and the whole averaging 21 years. Of the stations dealt with, 54 are on the west slope, and 72 on the east slope. The mean annual rainfall for the whole country, deduced from these averages, is 44 inches; for the eastern slope 38 inches, and for the western slope 50 inches,—amounts which are probably not far from the true averages of these different regions.

In December, the general average for the whole country is greatly above the average monthly fall; in May it falls to the minimum, after which it continues to increase till it again rises considerably above the monthly average in October, to fall again, however, to about the average in November. The curve of the rainfall of the east, as compared with that of the west, shows the wet and dry seasons to be less strongly marked in the east; or the departures from the monthly averages are larger in the west. Since, however, the curves closely resemble each other, the general causes bringing about the deposition of rain in the west and in the east are the same. But at all seasons the absolute amount of the rainfall is greater in the west than in the east.

The largest monthly rainfall takes place in *December* in the north-western and western districts, and in the mountainous districts of the interior; in *January*, in the south-west, the Ochil Hills, and east of Perthshire; whereas, at a number of places in the drier districts, *August* is the month of largest rainfall.

The month of least rainfall is *April*, in the south of Scotland, *May* in the north, and *June* in Orkney, Shetland, and Farö; and it is remarkable that these same months are the months of largest (or very large) rainfall in various extensive regions on the continent of Europe.

3. Note on the Strain-Function. By Professor Tait.

When the linear and vector function expressing a strain is self-conjugate the strain is pure. When it is not self-conjugate, it may be broken up into pure and rotational parts in various ways (analogous to the separation of a quaternion into the *sum* of a scalar and a vector part, or into the *product* of a tensor and a versor part), of which two are particularly noticeable. Denoting by a bar a self-conjugate function, we have thus either

$$\phi = \overline{\psi} + \mathrm{V}.\epsilon(\ \ ),$$
$$\phi = q\overline{\varpi}(\ \ )q^{-1}, \text{ or } \phi = \overline{\varpi}.q(\ \ )q^{-1},$$

where ϵ is a vector, and q a quaternion (which may obviously be regarded as a mere versor).

That this is possible is seen from the fact that ϕ involves nine independent constants, while $\overline{\psi}$ and $\overline{\varpi}$ each involve six, and ϵ and q each three. If ϕ' be the function conjugate to ϕ, we have

$$\phi' = \overline{\psi} - \mathrm{V}.\epsilon(\ \ )$$

so that

$$2\overline{\psi} = \phi + \phi'$$

and

$$2\,\mathrm{V}.\epsilon(\ \ ) = \phi - \phi'$$

which completely determine the first decomposition. This is, of course, perfectly well known in quaternions, but it does not seem to have been noticed as a theorem in the kinematics of strains that there is always one, and but one, mode of resolving a strain into the geometrical composition of the separate effects of (1) a *pure* strain, and (2) a rotation accompanied by uniform dilatation perpendicular to its axis, the dilatation being measured by (sec. $\theta - 1$) where θ is the angle of rotation.

In the second form (whose solution does not appear to have been attempted) we have

$$\phi = q\overline{\varpi}(\ \ )q^{-1},$$

where the pure strain precedes the rotation; and from this

$$\phi' = \overline{\varpi}.q^{-1}(\ \ )q,$$

or in the conjugate strain the rotation (reversed) is followed by the pure strain. From these

$$\phi'\phi = \overline{\varpi}.q^{-1}(q\overline{\varpi}(\ \ )q^{-1})q$$
$$= \overline{\varpi}^2,$$

and ϖ is therefore to be found by the solution of a biquadratic equation, as in *Proc. R. S. E.*, 1870, p. 316. It is evident, indeed, from the identical equation

$$S.\sigma\phi'\phi\rho = S.\rho\phi'\phi\sigma$$

that the operator $\phi'\phi$ is self-conjugate.

In the same way

$$\phi\phi'(\ \ ) = q^{-1}\varpi^2(q(\ \ )q^{-1})q$$

or

$$q(\phi\phi'\rho)q^{-1} = \varpi^2(q\rho q^{-1}) = \phi'\phi(q\rho q^{-1})$$

which show the relations between $\phi\phi'$, $\phi'\phi$, and q.

To determine q we have

$$\phi\rho.q = q\varpi\rho$$

whatever be ρ, so that

$$S.Vq(\phi - \varpi)\rho = 0,$$

or

$$S.\rho(\phi' - \varpi)Vq = 0,$$

which gives

$$(\phi' - \varpi)Vq = 0.$$

The former equation gives evidently

$$Vq \parallel V.(\phi - \varpi)\alpha(\phi - \varpi)\beta$$

whatever be α and β; and the rest of the solution follows at once. A similar process gives us the solution when the rotation precedes the pure strain.

4. On the Motion of Rigid Solids in a Liquid circulating Irrotationally through Perforations in them or in any Fixed Solid.* By Sir William Thomson.

1. Let ψ, ϕ, ... be the values at time t, of generalised co-ordinates fully specifying the positions of any number of solids movable through space occupied by a perfect liquid destitute of rotational motion, and not acted on by any force which could produce

* The title and first part (§§ 1 ... 13) are new, The remainder (§§ 14, 15) was communicated to the Royal Society at the end of last December.—W. T. *September* 26, 1872.

it. Some or all of these solids being perforated, let χ, χ', χ'', &c., be the quantities of liquid which from any era of reckoning, up to the time t, have traversed the several apertures. According to an extension of Lagrange's general equations of motion, used in Vol. I. of Thomson and Tait's "Natural Philosophy," §§ 331...336, proved in §§ 329, 331 of the German translation of that volume, and to be farther developed in the second English edition now in the press, we may use these quantities χ, χ', ... as if they were co-ordinates so far as concerns the equations of motion. Thus, although the position of any part of the fluid is not only not explicitly specified, but is actually indeterminate, when ψ, φ, ... χ, χ', ... are all given, we may regard χ, χ' ...... as specifying all that it is necessary for us to take into account regarding the motion of the liquid, in forming the equations of motion of the solids; so that if ξ, η, ..., and Ψ, Φ ... denote the generalised components of momentum and of force [Thomson and Tait, § 313 (*a*) (*b*)] relatively to ψ, φ ..., and if κ, κ', ... K, K$'$ ... denote corresponding elements relatively to χ, χ'..., we have (Hamiltonian form of Lagrange's general equations)

$$\left.\begin{array}{l} \dfrac{d\xi}{dt} + \dfrac{\eth T}{d\psi} = \Psi, \ \dfrac{d\eta}{dt} + \dfrac{\eth T}{d\varphi} = \Phi \ldots\ldots \\[2ex] \dfrac{d\kappa}{dt} + \dfrac{\eth T}{d\chi} = K, \ \dfrac{d\kappa'}{dt} + \dfrac{\eth T}{d\chi'} = K' \ldots\ldots \end{array}\right\} \quad . \quad . \quad (1),$$

where T denotes the whole kinetic energy of the system, and $\eth$ differentiation on the hypothesis of ξ, η, $\cdots$ κ, κ' ... constant.

2. To illustrate the meaning of χ, K, κ, χ', $\cdots$, let B be one of the perforated solids, to be regarded generally as movable, draw an immaterial barrier surface Ω across the aperture to which they are related, and consider this barrier as fixed relatively to B. Let N denote the normal component velocity, relatively to B and Ω of the fluid at any point of Ω; and let $\iint d\sigma$ denote integration over the whole area of Ω: then

$$\iint N d\sigma = \dot{\chi} \quad . \quad . \quad . \quad (2);$$

and

$$\chi = \int dt \iint N d\sigma \quad . \quad . \quad . \quad (3),$$

which is a symbolical expression of the definition of χ. To the

surface of fluid coinciding with Ω at any instant, let pressure be applied of constant value K per unit of area, over the whole area; and at the same time let force (or force and couple) be applied to B equal and opposite to the resultant of this pressure supposed for a moment to act on a rigid material surface Ω rigidly connected with B. The "motive" (that is to say, system of forces) consisting of the pressure K on the fluid surface, and force and couple B as just defined, constitutes the generalised component force corresponding to χ [Thomson and Tait, § 313 (*b*)]; for it does no work upon any motion of B or other bodies of the system if χ is kept constant; and if χ varies work is done at the rate

$$K\dot{\chi} \text{ per unit of time,}$$

whatever other motions or forces there may be in the system. Lastly, calling the density of the fluid unity, let κ denote "circulation"* [V. M. § 60 (*a*)]† of the fluid in any circuit crossing β once, and only once: it is this which constitutes the generalised component momentum relatively to χ [Thomson and Tait, § 313 (*c*)]; for (V. M. § 72) we have

$$\kappa = \int_0^t K dt. \quad . \quad . \quad . \quad (4),$$

if the system given at rest (or in any state of motion for which $\kappa = 0$) be acted on by the motive K during time t.‡

3. The kinetic energy T is, of course, necessarily a quadratic function of the generalised momentum-components, $\xi, \eta, \ldots \kappa, \kappa' \ldots$; with coefficients generally functions of $\psi, \phi, \ldots$, but necessarily independent of $\chi, \chi', \ldots$. In consequence of this peculiarity it is convenient to put

$$T = Q(\xi - \alpha\kappa - \alpha'\kappa' - \&c., \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots) + \mathfrak{Q}(\kappa, \kappa', \ldots) \quad (5),$$

* Or $\int F ds$ if F denote the tangential component of the absolute velocity of the fluid at any point of the circuit, and $\int ds$ line integration once round the circuit.

† References distinguished by the initials V. M. are to the part already published of the author's paper on Vortex Motion. (*Transactions of the Royal Society of Edinburgh*, 1867–8 *and* 1868–9.)

‡ The general limitation, for impulsive action, that the displacements effected during it are infinitely small, is not necessary in this case. Compare § 5 (11), below.

where Q, $\mathfrak{Q}$ denote two quadratic functions. This we may clearly do, because, if i be the number of the variables $\xi, \eta, \cdots$, and j the number of $\kappa, \kappa' \ldots$; the whole number of coefficients in the single quadratic function expressing τ is $\frac{(i+j)(i+j+1)}{2}$, which is equal to the whole number of the coefficients $\frac{i(i+1)}{2} + \frac{j(j+1)}{2}$ of the two quadratic functions, together with the $i\,j$ available quantities $\alpha, \beta, \ldots \alpha', \beta', \ldots, \ldots$

4. The meaning of the quantities $\alpha, \beta, \ldots \alpha', \ldots$ thus introduced is evident when we remember that

$$\frac{dT}{d\xi} = \psi, \frac{dT}{d\eta} = \phi, \ldots \frac{dT}{d\kappa} = \dot{\chi}, \frac{dT}{d\kappa'} = \dot{\chi}', \ldots \quad . \quad (6).$$

For; differentiating (5), and using these, we find

$$\dot{\psi} = \frac{dQ}{d\xi}, \dot{\phi} = \frac{dQ}{d\eta}, \ldots \quad . \quad . \quad . \quad (7),$$

and using these latter,

$$\dot{\chi} = \frac{d\mathfrak{Q}}{d\kappa} - \alpha\dot{\psi} - \beta\dot{\phi} - \&c., \dot{\chi}' = \frac{d\mathfrak{Q}}{d\kappa'} - \alpha'\dot{\psi} - \beta'\dot{\phi} - \&c., \ldots \quad (8).$$

Equations (8) show that $-\alpha\dot{\psi}, -\beta\dot{\phi}, -\alpha'\dot{\psi}$, &c., are the contributions to the flux across Ω, Ω', &c., given by the separate velocity-components of the solids. And (7) show that to prevent the solids from being set in motion when impulses $\kappa, \kappa', \cdots$ are applied to the liquid at the barrier surfaces, we must apply to them impulses expressed by the equations

$$\xi = \alpha\kappa + \alpha'\kappa' + \&c., \eta = \beta\kappa + \beta'\kappa' + \&c., \ldots \quad . \quad (9).$$

5. To form the equations of motion, we have, in the first place,

$$\frac{\partial T}{d\chi} = 0, \frac{\partial T}{d\chi'} = 0 \ldots \quad . \quad . \quad . \quad (10),$$

and therefore, by (1),

$$\frac{d\kappa}{dt} = K, \frac{d\kappa'}{dt} = K', \ldots \quad . \quad (11);$$

which show that the acceleration of κ, under the influence of K, follows simply the law of acceleration of a mass under the influence of a force. Again (for the motions of the solids), let

$$\xi_0 = \xi - a\kappa - a'\kappa' - \&c., \; \eta_0 = \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots \qquad (12);$$

and let $\frac{\mathfrak{D}Q}{d\psi}$, &c., denote variations of Q on the hypothesis of ξ_0, η_0, ... each constant.

We have from (5), remembering that $\frac{\delta T}{d\psi}$, &c., denote variations of T, on the hypothesis of ξ, η, ... κ, κ', ... constant,

$$\frac{\delta T}{d\psi} = \frac{\mathfrak{D}Q}{d\psi} - \frac{dQ}{d\xi}\left(\kappa\frac{da}{d\psi} + \kappa'\frac{da'}{d\psi} + \ldots\right) - \frac{dQ}{d\eta}\left(\kappa\frac{d\beta}{d\psi} + \kappa'\frac{d\beta'}{d\psi} + \&c.\right) - \&c. + \frac{\delta Q}{d\psi},$$

or, by (7)

$$\frac{\delta T}{d\psi} = \frac{\mathfrak{D}Q}{d\psi} - \dot{\psi}\left(\kappa\frac{da}{d\psi} + \kappa'\frac{da}{d\psi} + \&c.\right) - \dot{\phi}\left(\kappa\frac{d\beta}{d\psi} + \kappa'\frac{d\beta'}{d\psi} + \&c.\right) - \&c. + \frac{\delta Q}{d\psi} \ldots \quad . \quad (13).$$

Hence by (1)

$$\frac{d\xi}{dt} + \frac{\mathfrak{D}Q}{d\psi} - \dot{\psi}\left(\kappa\frac{da}{d\psi} + \kappa'\frac{da'}{d\psi} + \&c.\right) - \dot{\phi}\left(\kappa\frac{d\beta}{d\psi} + \kappa'\frac{d\beta'}{d\psi} + \&c.\right) - \&c. + \frac{\delta Q}{d\psi} = \Psi \ldots \quad (14).$$

Now, remark that, according to the notation of (12), ξ_0, η_0,... are the momentum-components of the solids due to their own motion alone, without cyclic motion of the liquid; and therefore eliminate ξ, η,... by (12) from (14). Thus we find

$$\begin{aligned} \frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\psi} + a\frac{d\kappa}{dt} + a'\frac{d\kappa'}{dt} + \&c. + \dot{\phi}\left\{\kappa\left(\frac{da}{d\phi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{da'}{d\phi} - \frac{d\beta'}{d\psi}\right) + \&c.\right\} \\ + \dot{\theta}\left\{\kappa\left(\frac{da}{d\theta} - \frac{d\gamma}{d\psi}\right) + \kappa'\left(\frac{da'}{d\phi} - \frac{d\gamma'}{d\psi}\right) + \&c.\right\} \\ + \quad \&c. \qquad\qquad = \Psi - \frac{\delta Q}{d\psi} \ldots (15), \end{aligned}$$

which, with the corresponding equation for ξ_0, &c., and with (11) for κ, κ', &c., are the desired equations of motion.

6. The hypothetical mode of application of K, K',... (§ 1) is impossible, and every other (such as the influence of gravity on a real liquid at different temperatures in different parts) is impossible for our ideal "liquid," that is to say, a homogeneous incompressible perfect fluid. Hence we have K = 0, K' = 0, and from (11)

conclude that $\kappa, \kappa', \ldots$ are constants. [They are sometimes called the "cyclic constants (V. M. §§ 62...64)]. The equations of motion (15) thus become simply

$$\frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\psi} + \dot{\varphi}\left\{\kappa\left(\frac{d\alpha}{d\varphi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\varphi} - \frac{d\beta'}{d\psi}\right) + \ldots\right\}$$
$$+ \dot{\theta}\left\{\kappa\left(\frac{d\alpha}{d\theta} - \frac{d\gamma}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\theta} - \frac{d\beta'}{d\psi}\right) + \ldots\right\}$$
$$+ \&c. \qquad = \Psi - \frac{\delta\mathfrak{Q}}{d\psi} \ldots (16),$$

with corresponding equations for η_0, ζ_0, and with the following relations from (7), between $\xi_0, \eta_0 \cdots$ and $\dot{\psi}, \dot{\varphi}, \cdots$

$$\frac{dQ}{d\xi_0} = \dot{\psi}, \quad \frac{dQ}{d\eta_0} = \dot{\varphi}, \quad \frac{dQ}{d\zeta_0} = \dot{\theta}, \ \&c. \quad . \quad . \quad (17).$$

7. Let

$$\kappa\left(\frac{d\alpha}{d\varphi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\varphi} - \frac{d\beta'}{d\psi}\right) + \&c., \text{ be denoted by } \{\varphi, \psi\} \quad . \quad (18),$$

so that we have

$$\{\varphi, \psi\} = -\{\psi, \varphi\} \quad . \quad . \quad . \quad . \quad (19).$$

These quantities $\{\varphi, \psi\}$, $\{\theta, \psi\}$, &c., linear functions of the cyclic constants, with coefficients depending on the configuration of the system, are to be generally regarded simply as given functions of the co-ordinates $\psi, \varphi, \theta, \ldots$: and the equations of motion are

$$\left.\begin{array}{l} \dfrac{d\xi_0}{dt} + \dfrac{\mathfrak{D}Q}{d\psi} + \{\varphi, \psi\}\dot{\varphi} + \{\theta, \psi\}\dot{\theta} + \&c. = \Psi - \dfrac{\delta\mathfrak{Q}}{d\psi} \\ \dfrac{d\eta}{dt} + \dfrac{\mathfrak{D}Q}{d\varphi} - \{\varphi, \psi\}\dot{\psi} + \{\theta, \varphi\}\dot{\theta} + \&c. = \Theta - \dfrac{\delta\mathfrak{Q}}{d\theta} \\ \ldots\ldots\ldots\ldots\ldots \end{array}\right\} \quad . \quad . \quad (20).$$

In these (being of the Hamiltonian form) Q is regarded as a quadratic function of $\xi_0, \eta_0, \zeta_0 \cdots$ with its coefficients functions of ψ, φ, θ, &c.; and $\mathfrak{D}$ applied to it indicates variations of these coefficients. If now we eliminate $\xi_0, \eta_0, \zeta_0 \cdots$ from Q by the linear equations, of which (17) is an abbreviated expression, and so have Q expressed as a quadratic function of $\dot{\psi}, \dot{\varphi}, \dot{\theta}, \cdots$, with its coefficients functions of ψ, φ, θ, &c.; and if we denote by $\frac{dQ}{d\varphi}, \frac{dQ}{d\psi}$, &c., variations of Q depending on variations of these co-

efficients; and by $\frac{dQ}{d\dot{\psi}}$, $\frac{dQ}{d\dot{\phi}}$, &c., variations of Q depending on variations of $\dot{\psi}$, $\dot{\phi}$, &c.; we have [compare Thomson and Tait, § 329 (13) and (15)]

$$\left.\begin{array}{l} \xi_0 = \frac{dQ}{d\dot{\psi}}, \quad \eta_0 = \frac{dQ}{d\dot{\psi}}, \ \ldots\ldots\ldots\ldots\ldots \\ \text{and} \\ \frac{\mathfrak{d}Q}{d\psi} = -\frac{dQ}{d\psi}, \ \frac{\mathfrak{d}Q}{d\varphi} = -\frac{dQ}{d\varphi}, \ \ldots\ldots \end{array}\right\} \quad . \qquad (21);$$

and the equations of motion become

$$\left.\begin{array}{l} \frac{d}{dt}\frac{dQ}{d\dot{\psi}} - \frac{dQ}{d\psi} + \{\varphi, \psi\}\dot{\phi} + \{\theta, \psi\}\dot{\theta} + \ . \ \ . \ \ . = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi} \\ \frac{d}{dt}\frac{dQ}{d\dot{\phi}} - \frac{dQ}{d\varphi} - \{\varphi, \psi\}\dot{\psi} + \{\theta, \varphi\}\dot{\theta} + \ . \ \ . \ \ . = \Phi - \frac{\mathfrak{d}\mathfrak{Q}}{d\varphi} \\ \frac{d}{dt}\frac{dQ}{d\dot{\theta}} - \frac{dQ}{d\theta} - \{\theta, \psi\}\dot{\psi} - \{\theta, \varphi\}\dot{\phi} + \ . \ \ . \ \ . = \Theta - \frac{\mathfrak{d}\mathfrak{Q}}{d\theta} \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \end{array}\right\} (22).$$

The first members here are of Lagrange's form, with the remarkable addition of the terms involving the velocities simply (in multiplication with the cyclic constants) depending on the cyclic fluid motion. The last terms of the second members contain traces of their Hamiltonian origin in the symbols $\frac{\mathfrak{d}}{d\psi}$, $\frac{\mathfrak{d}}{d\varphi}$, ...

8. As a first application of these equations, let $\dot{\psi} = 0$, $\dot{\phi} = 0$, $\dot{\theta} = 0$, ... This makes $\xi_0 = 0$, $\eta_0 = 0$..., and therefore also $Q = 0$; and the equations of motion (16), (now equations of equilibrium of the solids under the influence of applied forces Ψ, Φ, &c., balancing the fluid pressure due to the polycyclic motion κ, κ',...), become

$$\Psi = \frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \ \Phi = \frac{\mathfrak{d}\mathfrak{Q}}{d\varphi}, \ \&c., \quad . \quad . \quad . \quad (23);$$

a result which a direct application of the principle of energy renders obvious (the augmentation of the whole energy produced by an infinitesimal displacement, $\delta\psi$, is $\frac{\mathfrak{d}\mathfrak{Q}}{d\psi}\delta\psi$, and $\Psi\delta\psi$ is the work done by the applied forces). It is proved in §§ 724 ... 730 of a volume of collected papers on electricity and magnetism soon to be

published, that $\frac{\eth\mathfrak{Q}}{d\psi}$, $\frac{\eth\mathfrak{Q}}{d\varphi}$, &c., are the components of the forces experienced by bodies of perfect diamagnetic inductive capacity placed in the magnetic field analogous* to the supposed cyclic irrotational motion. Hence the motive influence of the cyclic motion of the liquid upon the solids in equilibrium is equal and opposite to that of magnetism in the magnetic analogue.

This is proposition II. of the paper "On the Forces experienced by Solids immersed in a Moving Liquid," which relates to the forces required to keep the movable solids at rest. The present investigation shows Prop. II. of that article to be false. Compare "Reprint," § 740.

9. Equations (16) for the case of a single perforated movable solid undisturbed by others, agree substantially with equations (6) and (14) of my communication † to the Royal Society of Edinburgh of February 1871. The $\xi_0, \eta_0, \ldots$ of the present article correspond to the $\frac{dT}{du}$, $\frac{dT}{dv}$, &c., of the former; the $\xi, \eta, \ldots$ mean the same in both. The equations now demonstrated constitute an extension of the theory not readily discovered or proved by that simple consideration of the principle of momentum, and moment of momentum, on which alone was founded the investigation of my former article.

10. Going back to the analytical definition of $\mathfrak{Q}$ in § 3 (5), we see that when none of the movable solids is perforated, this configurational function is equal to the whole kinetic energy (E), which the polycyclic motion would have were there no movable solid, diminished by the energy (W) which would be given up were the liquid, which on this supposition flows through the space of the movable solid or solids, suddenly rigidified and brought to rest. Putting then

$$\mathfrak{Q} = \mathrm{E} - \mathrm{W} \quad . \quad . \quad . \quad (24),$$

and remarking that E is independent of the co-ordinates of the movable solids, we may put $-\mathrm{W}$ in place of $\mathfrak{Q}$ in the equations of motion, which, for this slight modification, need not be written

* Proposition I. of article on "The Forces experienced by Solids immersed in a Moving Liquid" (*Proceedings R. S. E., February* 1870, reprinted in Volume of Electric and Magnetic papers, §§ 733 ... 740).

† See Proceedings R. S. E., Session 1870–71, or reprint in Philosophical Magazine, Nov. 1871.

out again. W might be directly defined as the whole quantity of work required to remove the movable solids, each to an infinite distance from any other solid having a perforation with circulation through it; and, with this definition, $-$W may be put for Q in the equations of motion without exclusion of cases in which there is circulation through apertures in movable solids.

11. I conclude with a very simple case, the subject of my communication to the Royal Society of last December, in which the result was given without proof. Let there be only one moving body, and it spherical; let the perforated solid or solids be reduced to an infinitely fine immovable rigid curve or group of curves (endless, of course, that is, either finite and closed, or infinite), and let there be no other fixed solid. The rigid curve or curves will be called the "core" or "cores," as their part is simply that of core for the cyclic or polycyclic motion. In this case it is convenient to take for ψ, φ, θ, the rectangular co-ordinates (x, y, z) of the centre of the movable globe. Then, because the cores, being infinitely fine, offer no obstruction to the motion of the liquid, making way for the globe moving through it, we have

$$Q = \tfrac{1}{2}m(\dot{x}^2 + \dot{y}^2 + \dot{z}^2) \quad . \quad . \quad (25),$$

where m denotes the mass of the globe, together with half that of its bulk of the fluid. Hence

$$\left.\begin{array}{l} \dfrac{dQ}{dx} = 0,\ \dfrac{dQ}{dy} = 0,\ \dfrac{dQ}{dz} = 0, \\ \text{and} \\ \xi_0\left(= \dfrac{dQ}{d\dot{x}}\right) = m\dot{x},\ \eta_0 = m\dot{y},\ \zeta_0 = m\dot{z} \end{array}\right\} \quad . \quad (26).$$

A farther great simplification occurs, because in the present case $\alpha d\psi + \beta d\varphi + \ldots$, or, as we now have it, $\alpha dx + \beta dy + \gamma dz$, is a complete differential.* To prove this, let V be the velocity-potential at any point (a, b, c) due to the motion of the globe, irrespectively of any cyclic motion of the liquid. We have

$$V = \tfrac{1}{2}r^3\left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}\right)\frac{1}{D},$$

* Which means that if the globe, after any motion whatever, great or small, comes again to a position in which it has been before, the integral quantity of liquid which this motion has caused to cross any fixed area is zero.

where r denotes the radius of the globe, and $D = \{(x-a)^2 + (y-b)^2 + (z-c)^2\}^{\frac{1}{2}}$. Hence if N denote the component velocity of the liquid at (a, b, c) in any direction λ, μ, ν, we have

$$N = \left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}\right) F(x, y, z, a, b, c), \qquad (27),$$

where

$$F(x, y, z, a, b, c,) = \tfrac{1}{2}r^3\left(\lambda\frac{d}{da} + \mu\frac{d}{db} + \nu\frac{d}{dc}\right)\frac{1}{D}.$$

Let now (a, b, c) be any point of the barrier surface Ω (§ 2), and λ, μ, ν, the direction cosines of the normal. By (2) of § 2 we see that the part of $\dot{\chi}$ due to the motion of the globe is $\iint N d\sigma$, or, by (26),

$$\left(\dot{x}\frac{d}{dx} + \dot{y}\frac{d}{dy} + \dot{z}\frac{d}{dz}\right)\iint F(x, y, z, a, b, c)\, d\sigma \qquad (28).$$

Hence, putting

$$\iint F(x, y, z, a, b, c)\, d\sigma = U,$$

we see by (8) of § 4, that

$$\alpha = -\frac{dU}{dx}, \beta = -\frac{dU}{dy}, \gamma = -\frac{dU}{dz} \qquad (29).$$

Hence, with the notation of § 7 (18) for $x, y, \ldots$ instead of $\psi, \phi, \ldots$

$$\{y, z\} = 0, \{z, x\} = 0, \{x, y\} = 0.$$

By this and (25) the equations of motion (22), with (24), become simply

$$m\frac{d^2x}{dt^2} = X + \frac{\delta W}{dx}, \; m\frac{d^2y}{dt^2} = Y + \frac{\delta W}{dy}, \; m\frac{d^2z}{dt^2} = Z + \frac{\delta W}{dz} \quad (30).$$

These equations express that the globe moves as a material particle of mass m, with the forces (X, Y, Z) expressly applied to it, would move in a "field of force," having W for potential.

12. The value of W is of course easily found by aid of spherical harmonics, from the velocity potential, P, of the polycyclic motion which would exist were the globe removed, and which we must suppose known: and in working it out (small print below) it is readily seen that if, for the hypothetical undisturbed motion, q denote the velocity at the point really occupied by the centre of the rigid globe, we have

$$W = \tfrac{1}{2}\mu q^2 + w \qquad (31),$$

where μ denotes one and a half times the volume of the globe, and w denotes the kinetic energy of what we may call the internal motion of the liquid occupying for an instant in the undisturbed motion the space of the rigid globe in the real system. To define w, remark that the harmonic analysis proves the velocity of the centre of inertia of an irrotationally moving liquid globe to be equal to q, the velocity of the liquid at its centre;* and consider the velocity of any part of the liquid sphere, relatively to a rigid body moving with the velocity q. The kinetic energy of this relative motion is what is denoted by w. Remark also that if, by mutual forces between its parts, the liquid globe were suddenly rigidified, the velocity of the whole would be equal to q; and that $\frac{1}{2}mq^2$ is the work given up by the rigidified globe and surrounding liquid when the globe is suddenly brought to rest, being the same as the work required to start the globe with velocity q from rest in a motionless liquid.

Let $P + \psi$ be the velocity potential at (x, y, z) in the actual motion of the liquid when the rigid globe is fixed. Let a be the radius of the globe, r distance of (x, y, z) from its centre, and $\iint d\sigma$ integration over its surface. At any point of the surface of the instantaneous liquid globe, the component velocity perpendicular to the spherical surface in the undisturbed motion is $\left(\frac{dP}{dr}\right)_{r=a}$; and hence the impulsive pressure on the spherical surface required to change the velocity potential of the external liquid from P to $P+\psi$, being $-\psi$, undoes an amount of work equal to

$$\iint d\sigma \psi \cdot \tfrac{1}{2} \frac{dP}{dr},$$

in reducing the normal component from that value to zero. On the other hand, the internal velocity-potential is reduced from P to zero, and the work undone in this process is

$$\iint d\sigma P \cdot \tfrac{1}{2} \frac{dP}{dr}.$$

* This follows immediately from the proposition (Thomson and Tait's "Natural Philosophy," § 496) that any function V, satisfying Laplace's equation $\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2}$ throughout a spherical space has for its mean value through this space its value at the centre. For $\frac{dP}{dx}$ satisfies Laplace's equation.

Hence

$$W = \tfrac{1}{2}\iint d\sigma \, (P + \psi) \frac{dP}{dr}, \quad . \quad . \qquad (32).$$

The condition that with velocity-potential $P + \psi$ there is no flow perpendicular to the spherical surface, gives

$$\left(\frac{dP}{dr} + \frac{d\psi}{dr}\right)_{r=a} = 0 \quad . \quad . \quad . \qquad (33).$$

Now let

$$\left.\begin{aligned} P &= P_0 + P_1\frac{r}{a} + \ldots\ldots + P_i\left(\frac{r}{a}\right)^i + \&c. \\ \psi &= \Psi_1\left(\frac{a}{r}\right)^2 + \ldots\ldots + \Psi_i\left(\frac{a}{r}\right)^{i+1} + \&c. \end{aligned}\right\} \quad . \qquad (34),$$

be the spherical harmonic developments of P and ψ relatively to the centre of the rigid globe as origin, the former necessarily convergent throughout the largest spherical space which can be described from this point as centre without enclosing any part of the core; the latter necessarily convergent throughout space external to the sphere. By (33) we have

$$\Psi_i = \frac{i}{i+1} P_i \quad . \quad . \quad . \quad . \qquad (35).$$

Hence (32) gives

$$W = \tfrac{1}{2}\iint d\sigma \left(\Sigma \frac{2i+1}{i+1} P_i\right)\left(\Sigma i P_i\right),$$

which, by

$$\iint d\sigma P_i P_{i'} = 0,$$

becomes

$$W = \frac{1}{2a}\,\Sigma\frac{i(2i+1)}{i+1}\iint d\sigma P_i^2 \quad . \quad . \quad . \qquad (36).$$

Now, remarking that a solid spherical harmonic of the first degree may be any linear function of x, y, z, put

$$P_1\frac{r}{a} = Ax + By + Cz \quad . \quad . \quad . \qquad (37),$$

which gives

$$q^2 = A^2 + B^2 + C^2,$$

and

$$\frac{1}{a}\iint d\sigma P_1^2 = (A^2 + B^2 + C^2)\,.\,\frac{a}{3}\,.\iint d\sigma = q^2 \times \text{volume of globe} = \frac{2}{3}\mu q^2.$$

Hence by (36

$$W = \tfrac{1}{2}\mu q^2 + \tfrac{1}{2}\iint d\sigma\left(\frac{2\,.\,5}{3}\,P_2^2 + \frac{3\,.\,7}{4}\,P_3^2 + \ldots\right). \qquad (38);$$

and, therefore, by comparison with (31),

$$w = \tfrac{1}{2}\iint d\sigma\left(\frac{2\,.\,5}{3}\,P_2^2 + \frac{3\,.\,7}{4}\,P_3^2 + \ldots\right) \quad . \qquad (39).$$

13. When the radius of the globe is infinitely small,

$$W = \tfrac{1}{2}\mu q^2 \quad . \quad . \quad . \quad . \quad (40),$$

where μ denotes one and a half times the volume of the globule, and q the undisturbed velocity of the fluid in its neighbourhood. This corresponds to the formula which I gave twenty-five years ago for the force experienced by a small sphere (whether of ferromagnetic or diamagnetic non-crystalline substance) in virtue of the inductive influence which it experiences in a magnetic field.*

14. By taking an infinite straight line for the core a simple but very important example is afforded. In this case, the undisturbed motion of the fluid is in circles having their centres in the core (or axis, as we may now call it), and their planes perpendicular to it. As is well known, the velocity of irrotational revolution round a straight axis is inversely proportional to distance from the axis. Hence the potential function W for the force experienced by an infinitesimal solid sphere in the fluid is inversely as the square of the distance of its centre from the axis, and therefore the force is inversely as the cube of the distance, and is towards the nearest point of the axis. Hence, when the globule moves in a plane perpendicular to the axis, it describes one or other of the forms of Cotesian spirals†. If it be projected obliquely to the axis, the component velocity parallel to the axis will remain constant, and the other component will be unaffected by that one; so that the projection of the globule on the plane perpendicular to the axis will always describe the same Cotesian spiral as would be described were there no motion parallel to the axis. If the globule be left to itself in any position it will commence moving towards the axis as if attracted by a force varying inversely as the cube of the distance. It is remarkable that it traverses at right angles an increasing liquid current without any applied force to prevent it

* "On the Forces Experienced by Small Spheres under Magnetic Influence, and some of the Phenomena presented by Diamagnetic Substances" (*Cambridge and Dublin Mathematical Journal, May* 1847); and "Remarks on the Forces experienced by Inductively Magnetised Ferromagnetic or Diamagnetic Non-crystalline Substances" (*Phil. Mag. October* 1850). Reprint of Papers on Electrostatics and Magnetism, §§ 634–668. Macmillan, 1872.

† Tait and Steele's "Dynamics of a Particle," § 149 (15).

from being (as we might erroneously at first sight expect it to be) carried sideways with the augmented stream. A properly trained dynamical intelligence would at once perceive that the constancy of moment of momentum round the axis requires the globule to move directly towards it.

15. Suppose now the globule to be of the same density as the liquid. If (being infinitely small) it is projected in the direction and with the velocity of the liquid's motion, it will move round the axis in the same circle with the liquid; but this motion would be unstable [and the neglected term w (39) adds to the instability]. Compare Tait and Steele's "Dynamics of a Particle," § 149 (15), Species IV., case A = 0 and AB finite; also limiting variety between Species I. and Species V. The globule will describe the same circle in the opposite direction if projected with the same velocity opposite to that of the fluid. If the globule be projected either in the direction of the liquid's motion or opposite to it, with a velocity less than that of the liquid, it will move along the Cotesian spiral (Species I. of Tait and Steele), from apse to centre in a finite time, with an infinite number of turns. If it be projected in either of those directions with a velocity greater by v than that of the liquid, it will move along the Cotesian spiral (Species V. of Tait and Steele), from apse to asymptote. Its velocity along the asymptote, at an infinite distance from the axis, will be

$$\sqrt{v\left(v+\frac{\kappa}{\pi a}\right)},$$

and the distance of the asymptote from the axis will be

$$a\frac{v+\dfrac{\kappa}{2\pi a}}{\sqrt{v\left(v+\dfrac{\kappa}{\pi a}\right)}},$$

where a denotes the distance of the apse from the axis, and $\frac{\kappa}{2\pi a}$ the velocity of the liquid at that distance from the axis. If the globule be projected from any point in the direction of any straight line whose shortest distance from the axis is p, it will be drawn into the vortex or escape from it, according as the component velo-

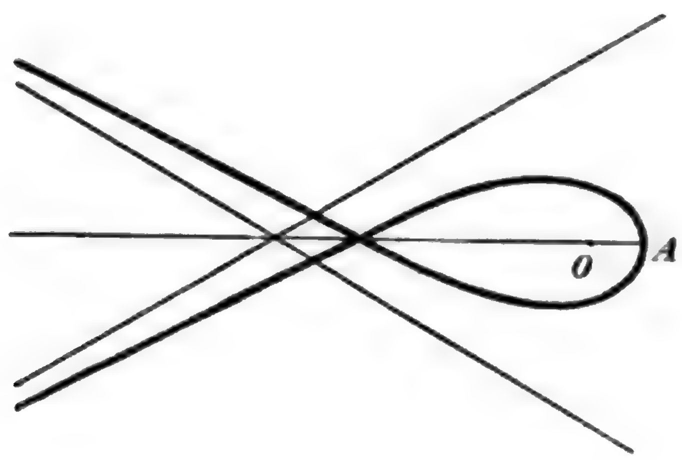
city in the plane perpendicular to the axis is less or greater than $\frac{\kappa}{2\pi p}$. It is to be remarked that in every case in which the globule is drawn in to the axis (except the extreme one in which its velocity is infinitely little less than that of the fluid, and its spiral path infinitely nearly perpendicular to the radius vector), the spiral by which it approaches, although it has always an infinite number of convolutions, is of finite length; and therefore, of course, the time taken to reach the axis is finite. Considering, for simplicity, motion in a plane perpendicular to the axis; at any point infinitely distant from the axis, let the globule be projected with a velocity v along a line passing at distance p on either side of the axis. Then if τ denote the velocity of the fluid at distance unity from the axis $\left[\text{which is equal to } \frac{\kappa}{2\pi}\right]$, and if we put

$$n^2 = 1 - \frac{\tau^2}{v^2p^2} \quad . \quad . \quad . \quad . \quad (41),$$

the polar equation of the path is

$$r = \frac{np}{\cos n\theta} \quad . \quad . \quad . \quad . \quad (42).$$

Hence the nearest approach to the axis attained by the globule is np, and the whole change of direction which it experiences is $\pi\left(\frac{1}{n} - 1\right)$. The case of $\frac{1}{n} = 2{\cdot}3$ is represented in the annexed diagram, copied from Tait and Steele's book [§ 149 (15), Species V.].



Monday, 18th March 1872.

PROFESSOR KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Extraction of the Square Root of a Matrix of the Third Order. By Professor Cayley.

Professor Tait has considered the question of finding the square root of a strain, or what is the same thing, that of a matrix of the third order—

$$\begin{pmatrix} a, & b, & c \\ d, & e, & f \\ g, & h, & i \end{pmatrix}.$$

A mode of doing this is indicated in my "Memoir on the Theory of Matrices" (*Phil. Trans.*, 1858, pp. 17–37), and it is interesting to work out the solution.

The notation and method will be understood from the simple case of a matrix of the second order. I write

$$(x_1,\ y_1) = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix} (x,\ y),$$

to denote the two equations, $x_1 = ax + by$, $y_1 = cx + dy$. This being so, putting

$$(x_2,\ y_2) = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix} (x_1,\ y_1), = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix}^2 (x,\ y),$$

we arrive at the value of the squared matrix, viz.,

$$\begin{pmatrix} a, & b \\ c, & d \end{pmatrix}^2 = \begin{pmatrix} a^2 + bc, & b(a+d) \\ c(a+d), & d^2 + bc \end{pmatrix},$$

and we have similarly the third, fourth, and higher powers of a matrix. The zero power is the matrix unity, $= \begin{pmatrix} 1, & 0 \\ 0, & 1 \end{pmatrix}$.

The zero matrix is $\begin{pmatrix} 0, & 0 \\ 0, & 0 \end{pmatrix}$, and when a matrix is put $= 0$, this means that it is a matrix of the last-mentioned form.

Consider the matrix $M = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix}$; write down the equation.

$$\begin{vmatrix} a - M, & b \\ c & , d - M \end{vmatrix} = 0,$$

where the function on the left hand is a determinant, M being therein regarded in the first instance as a quantity, viz., this equation is

$$M^2 - (a + d)M + (ad - bc)M^0 = 0;$$

and then substituting for M^2, M, M^0, their expressions as matrices, this equation is identically true, viz., it stands for the four identities—

$$\begin{aligned} a^2 + bc - (a + d)a + ad - bc &= 0, \\ b(a + d) - (a + d)b &= 0, \\ c(a + d) - (a + d)c &= 0, \\ d^2 + bc - (a + d)d + ad - bc &= 0, \end{aligned}$$

and the like property holds for a matrix of any order.

To extract the square root of the matrix $M = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix}$; in other words, to find a matrix $L = \begin{pmatrix} \mathrm{a}, & \mathrm{b} \\ \mathrm{c}, & \mathrm{d} \end{pmatrix}$ such that $L^2 = M$; that is

$$\begin{pmatrix} \mathrm{a}^2 + \mathrm{bc}, & \mathrm{b}(\mathrm{a} + \mathrm{d}) \\ \mathrm{c}(\mathrm{a} + \mathrm{d}), & \mathrm{d}^2 + \mathrm{bc} \end{pmatrix} = \begin{pmatrix} a, & b \\ c, & d \end{pmatrix},$$

(four equations for the determination of a, b, c, d):—

The solution is as follows: write

$$\begin{vmatrix} a - M, & b \\ c & , d - M \end{vmatrix} = M^2 - pM + q,$$

(q is here written for qM^0, and so in other cases); and similarly

$$\begin{vmatrix} \mathrm{a} - L, & \mathrm{b} \\ \mathrm{c} & , \mathrm{d} - L \end{vmatrix} = L^2 - \mathrm{p}L + \mathrm{q},$$

then we have

$$\begin{aligned} M^2 - pM + q &= 0, \\ L^2 - \mathrm{p}L + \mathrm{q} &= 0, \\ L^2 &= M; \end{aligned}$$

and from these equations we may express L as a linear function of M, M^0, with coefficients depending on p, q; and also determine the unknown quantities p, q in terms of p, q.

We, in fact, have

$$\mathrm{L} = \frac{1}{\mathrm{p}}(\mathrm{M} + \mathrm{q});$$

Also this gives $(\mathrm{M} + \mathrm{q})^2 - \mathrm{p}^2\mathrm{M} = 0$, that is

$$\mathrm{M}^2 - (\mathrm{p}^2 - 2\mathrm{q})\,\mathrm{M} + \mathrm{q}^2 = 0\,,$$

which must agree with

$$\mathrm{M}^2 - p\mathrm{M} + q = 0;$$

consequently,

$$\mathrm{p}^2 - 2\mathrm{q} = p,\ \mathrm{q}^2 = q\,,$$

that is,

$$\mathrm{q} = \sqrt{q},\ \mathrm{p} = \sqrt{p + 2\sqrt{q}}\,,$$

and then,

$$\mathrm{L} = \frac{1}{\mathrm{p}}(\mathrm{M} + \mathrm{q})\,,$$

which is the required solution; viz., this signifies

$$\mathrm{L} = \begin{pmatrix} \dfrac{a + \mathrm{q}}{\mathrm{p}}, & \dfrac{b}{\mathrm{p}} \\ \dfrac{c}{\mathrm{p}}\,, & \dfrac{d + \mathrm{q}}{\mathrm{p}} \end{pmatrix},$$

where p, q have the above-mentioned values—a result which can be at once verified. Observe that there are in all 4 solutions, but these correspond in pairs of solutions, differing only in their sign; the number of distinct solutions is taken to be = 2.

Passing now to the case of a matrix of the third order,

$$\mathrm{M} = \begin{pmatrix} a, & b, & c \\ d, & e, & f \\ g, & h, & i \end{pmatrix},$$

let the expanded value of the determinant

$$\begin{vmatrix} a - \mathrm{M}, & b, & c \\ d\ , & e - \mathrm{M}, & f \\ g\ ; & h\ , & i - \mathrm{M} \end{vmatrix} \text{ be } = -(\mathrm{M}^3 - p\mathrm{M}^2 + q\mathrm{M} - r);$$

and let the required square root be

$$\mathrm{L} = \begin{pmatrix} \mathrm{a}, & \mathrm{b}, & \mathrm{c} \\ \mathrm{d}, & \mathrm{e}, & \mathrm{f} \\ \mathrm{g}, & \mathrm{h}, & \mathrm{i} \end{pmatrix}$$

and p, q, r, have the like significations in regard to L. Then from the equations—

$$\begin{aligned} &M^3 - pM^2 + qM - r = 0\,, \\ &L^3 - \mathrm{p}L^2 + \mathrm{q}L - \mathrm{r} = 0\,, \\ &L^2 = M\,, \end{aligned}$$

we can express L as a linear function of M^2, M, M^0, with coefficients depending on p, q, r; and obtain expressions for p, q, r, in terms of p, q, r.

We have

$$L\,(M + \mathrm{q}) = \mathrm{p}M + \mathrm{r}\,,$$

that is,

$$L = \frac{\mathrm{p}M + \mathrm{r}}{M + \mathrm{q}}\,, = \mathrm{p} + \frac{\mathrm{r} - \mathrm{pq}}{M + \mathrm{q}}.$$

But we have

$$M^3 - pM^2 + qM - r = (M + \mathrm{q})\left(M^2 + \theta M + \varphi + \frac{\omega}{M + \mathrm{q}}\right)$$

where

$$\begin{aligned} -\theta &= \mathrm{q} + p\,, \\ \varphi &= \mathrm{q}^2 + \mathrm{q}p + q\,, \\ -\omega &= \mathrm{q}^3 + \mathrm{q}p^2 + \mathrm{q}q + r\,, \end{aligned}$$

and thence

$$L = \frac{\mathrm{pq} - \mathrm{r}}{\omega}(M^2 + \theta M + \varphi) + \mathrm{p}\,,$$

that is, $L = xM^2 + yM + z$, where x, y, z are given functions of p, q, r.

To determine these, observe that

$$\sqrt{M}(M + \mathrm{q}) = \mathrm{p}M + \mathrm{r}\,,$$

that is

$$M^3 - (\mathrm{p}^2 - 2\mathrm{q})M^2 + (\mathrm{q}^2 - 2\mathrm{pr})M - \mathrm{r}^2 = 0\,,$$

which must agree with

$$M^3 - pM^2 + qM - r = 0\,,$$

or we have

$$\mathrm{p}^2 - 2\mathrm{q} = p,\ \mathrm{q}^2 - 2\mathrm{pr} = q,\ \mathrm{r}^2 = r\,,$$

whence

$$\mathrm{r} = \sqrt{r}\,,$$

$$(\mathrm{q}^2 - q)^2 = 4\,(2\mathrm{q} + p)r\,,$$

$$\mathrm{p} = \frac{\mathrm{q} - q}{2\mathrm{r}}\,,$$

which are the required values; there being in all eight solutions, but these correspond in pairs of solutions of opposite sign, so that the number of independent solutions is = 4. The form of the result agrees in a remarkable manner with that obtained by Professor Tait on totally different principles (*ante*, p. 316).

I annex a further investigation, starting from the assumption that the solution is $\sqrt{M} = xM^2 + yM + z$; viz., writing for shortness—

$$M^2 = \left(\begin{array}{c} a', b', c' \\ d', e', f' \\ g', h', i' \end{array}\right),$$

then the solution is

$$\sqrt{M} = \left(\begin{array}{lll} xa' + ya + z, & xb' + yb, & xc' + yc \\ xd' + yd, & xe' + ye + z, & xf' + yf \\ xg' + yg, & xh' + yh, & xi' + yi + z \end{array}\right)$$

where observe that only a, e, i contain z; and that the differences e – i, i – a, a – e are independent of z. We ought to have

$$\begin{array}{l|l|l} a^2 + cg + bd = a & b(a + e) + ch = b & d(a + e) + fg = d, \\ e^2 + db + fh = e & f(e + i) + dc = f & h(e + i) + gb = h, \\ i^2 + hf + cg = i & g(i + a) + hd = g & c(i + a) + bf = c, \end{array}$$

viz., these nine equations should be satisfied by a common set of values of x, y, z; or, what is the same thing, the whole system should be equivalent to the first triad of equations. To verify this, observe that we can from the first triad (by the linear elimination of z^2 and z) obtain an equation of the form $(x, y)^3 + x = 0$; say this is the equation $\Omega = 0$. In fact, multiplying by e – i, i – a, a – e and adding, the three equations give

$$\begin{aligned}(e-i)(i-a)(a-e) + fh\,(e-i) + gc\,(i-a) + bd\,(a-e) \\ + \quad a\,(e-i) + \quad e\,(i-a) + \quad i\,(a-e) = 0,\end{aligned}$$

where the first line contains terms of the form $(x, y)^3$, the second line is linear and

$$= [a(e' - i') + e\,(i' - a') + i\,(a' - e')]x,$$

viz., this is

$$= [(e-i)(i-a)(a-e) + fh(e-i) + gc(i-a) + bd(a-e)]x.$$

The whole equation divides by the coefficient of x, and the result is $(x, y)^3 + x = 0$.

Now, from any one of the remaining six equations, together with two equations of the first triad, we can obtain the same result $\Omega = 0$. Thus, if the selected equation is $\mathrm{b}(\mathrm{a}+\mathrm{e})+\mathrm{ch}-b=0$, then from the first and second equations of the triad we have

$$(\mathrm{a}^2-\mathrm{e}^2)+\mathrm{cg}-\mathrm{fh}-(a-e)=0\,,$$

and thence

$$(\mathrm{a}-\mathrm{e})(b-\mathrm{ch})+\mathrm{b}(\mathrm{cg}-\mathrm{fh})-\mathrm{b}(a-e)=0\,.$$

There is here the linear term $b(\mathrm{a}-\mathrm{e})-\mathrm{b}(a-e)$, viz., this is

$$=[b(a'-e')-b'(a-e)]x\,,$$

which is

$$=[-(a-e)ch+b(cg-fh)]x\,.$$

The whole equation divides by the coefficient of x, and gives the foregoing equation, $\Omega = 0$.

Thus the equations reduce themselves to the first triad: writing these under the form

$$\frac{1}{a}(\mathrm{a}^2+\mathrm{cg}+\mathrm{bd})=\frac{1}{e}(\mathrm{e}^2+\mathrm{bd}+\mathrm{fh})=\frac{1}{i}(\mathrm{i}^2+\mathrm{hf}+\mathrm{cg})=1\,,$$

then omitting the last equation $(=1)$, these are of the form $\mathrm{U}=\mathrm{V}=\mathrm{W}$, where U, V, W are homogeneous quadric functions of x, y, z; viz., treating these as co-ordinates they represent two quadric cones, having a common vertex, and intersecting in 4 lines: or we have 4 sets of values of the ratios $x:y:z$: or for x, y, z themselves 8 sets of values; but, as before, these correspond in pairs, and the number of distinct solutions is taken to be $=4$.

I return to the equation $\Omega = 0$. This is found to be

$$\begin{vmatrix} (a-p)x-y, & bx & cx \\ dx & (e-p)x-y & fx \\ gx & hx & (i-p)x-y \end{vmatrix} - x = 0$$

$(p = a + e + i$ as before); or what is the same thing, the equation is

$$\begin{vmatrix} a-p-\frac{y}{x}, & b, & c \\ d, & e-p-\frac{y}{x}, & f \\ g, & h, & i-p-\frac{y}{x} \end{vmatrix} - \frac{1}{x^2} = 0\,.$$

I verify this by the former solution, as follows:—We have

$$x = \frac{\mathrm{pq} - \mathrm{r}}{\omega},\ y = \frac{\mathrm{pq} - \mathrm{r}}{\omega}\theta;\ \text{that is, } \frac{y}{x} = \theta,\ = -p - \mathrm{q}.$$

The equation thus becomes

$$\begin{vmatrix} a + \mathrm{q}, & b, & c \\ d & , e + \mathrm{q}, & f \\ g & , h & , i + \mathrm{q} \end{vmatrix} - \frac{\omega^2}{(\mathrm{pq} - \mathrm{r})^2} = 0,$$

that is

$$\mathrm{q}^3 + p\mathrm{q}^2 + q\mathrm{q} + r - \frac{\omega^2}{(\mathrm{pq} - \mathrm{r})^2} = 0.$$

But we have

$$-\omega = \mathrm{q}^3 + p\mathrm{q}^2 + q\mathrm{q} + r,$$

and the equation thus becomes

$$\mathrm{q}^3 + p\mathrm{q}^2 + q\mathrm{q} + r - (\mathrm{pq} - \mathrm{r})^2 = 0;$$

viz., substituting for p, q, r their values in terms of p, q, r, this is the identity,

$$\mathrm{q}^3 + \mathrm{q}^2(\mathrm{p}^2 - 2\mathrm{q}) + \mathrm{q}(\mathrm{q}^2 - 2\mathrm{pr}) + \mathrm{r}^2 - (\mathrm{pq} - \mathrm{r})^2 = 0.$$

An interesting case is where the given matrix M is unity; that is

$$\mathrm{M} = \begin{pmatrix} 1, & 0, & 0 \\ 0, & 1, & 0 \\ 0, & 0, & 1 \end{pmatrix}.$$

We have here $p = 3$, $q = 3$, $r = 1$; the equation in q is

$$\mathrm{q}^4 - 6\mathrm{q}^2 - 8\mathrm{q} - 3 = 0;$$

that is $(\mathrm{q} - 3)(\mathrm{q} + 1)^3 = 0$; viz., $\mathrm{q} = 3$ or $\mathrm{q} = -1$. Taking, as we may do, $\mathrm{r} = +1$, we have the two solutions ($\mathrm{p} = 3$, $\mathrm{q} = 3$, $\mathrm{r} = 1$) and ($\mathrm{p} = -1$, $\mathrm{q} = -1$, $\mathrm{r} = 1$).

For the first of these $\theta = -6$, $\phi = 21$, $\omega = -64$, $\mathrm{pq} - \mathrm{r} = 8$, and thence

$$\mathrm{L} = -\frac{1}{8}(\mathrm{M}^2 - 6\mathrm{M} + 21) + 3, = 1, \text{ on writing therein } \mathrm{M} = 1;$$

viz., we have L the matrix unity, a self-evident solution.

But for the second, $\theta = -2$, $\phi = 1$, $\omega = 0$, $\mathrm{pq} - \mathrm{r} = 0$, and the solution takes the form $\sqrt{\mathrm{M}} = \frac{0}{0}(\mathrm{M} - 1)^2 - 1$. There is, in

fact, a solution containing four arbitrary constants, given (with some misprints) in the "Memoir on Matrices," and which (for convenience changing the signs) is as follows:—

$$L = \left(\begin{vmatrix} \dfrac{-\alpha}{\alpha+\beta+\gamma}, & \dfrac{(\beta+\gamma)\frac{\lambda}{\mu}}{\alpha+\beta+\gamma}, & \dfrac{(\beta+\gamma)\frac{\lambda}{\nu}}{\alpha+\beta+\gamma} \\ \dfrac{(\gamma+\alpha)\frac{\mu}{\lambda}}{\alpha+\beta+\gamma}, & \dfrac{-\beta}{\alpha+\beta+\gamma}, & \dfrac{(\gamma+\alpha)\frac{\mu}{\nu}}{\alpha+\beta+\gamma} \\ \dfrac{(\alpha+\beta)\frac{\nu}{\lambda}}{\alpha+\beta+\gamma}, & \dfrac{(\alpha+\beta)\frac{\nu}{\mu}}{\alpha+\beta+\gamma}, & \dfrac{-\gamma}{\alpha+\beta+\gamma} \end{vmatrix} \right),$$

(or, what is the same thing, we may omit the denominators, assuming $\alpha+\beta+\gamma=1$); it is, in fact, easy to verify that this has for its square the matrix unity. Moreover, we have, as above, $p=-1$, $q=-1$, $r=1$.

2. Second Note on the Strain Function. By Prof. Tait.

3. Note on the Rate of Cooling at High Temperatures. By Professor Tait.

4. Notice of a Large Boulder in the Parish of Rattray, and County of Perth, having on one of its sides Cups and Grooves, apparently artificial. By D. Milne Home.

About a year ago, the Council of this Society appointed a Committee to make inquiry about boulders in Scotland.

The Committee intend to submit to the Council a general report of their proceedings, showing the progress made.

The object of the present notice is to give to the Society an account of one of the boulders reported to the Committee, as a specimen of the information which they have been obtaining.

The Rev. Mr Herdman, minister of Rattray, in Perthshire, sent to the Committee an answer to their circular, specifying the following boulders and standing stones in his parish:—

1*st*, A stone known from time immemorial as the *Standing Stone of Glenballoch*.

This boulder is angular, and rudely pyramided in form. Its entire height is 12 feet. At its base it is about 8 feet square; and half-way up, about 6 feet square. Its weight is estimated at about 25 tons.

It rests on what Mr Herdman describes as a firm, hard, dry, sandy, reddish yellow clay, called by the farmers of the district, till.

On one side of this stone, viz., that facing the glen, on the north bank of which it stands, there are cuttings or incisions, which Mr Herdman, and others skilled in archæology who have examined them, believe to be artificial. These incisions are of two kinds: *First*, hemispherical cavities, about twelve or thirteen in number; and *second*, grooves which on some points touch or run into these cavities.

2*d*, In another part of the same estate, viz., of Craighall, belonging to Colonel Clark Rattray, there is a spot known as "The Stannin' Stanes." This name occurs in the parish records, Mr Herdman says, so far back as 300 years. There was a small farm long known by the name of "Stannin' Stanes;" and about forty years ago, there were dwelling-houses at the place, forming a hamlet which bore the same name.

Though there is only one large stone at this place, Mr Herdman is of opinion that it once had companions. These have disappeared. They are probably in dykes and cattle sheds, not far off.

The stone which remains, is, in length above ground, about 5 feet, and is about 4 feet square. It is believed to be sunk in the ground 3 feet. Its weight is estimated at 8 or 9 tons. It stands upright.

3*d*, There is *a group of stones*, each containing about 7 cubic yards of rock, and each weighing, probably, about 14 tons, situated on the farm of Glenballoch, not far from the large stone first mentioned. Lines joining these 4 stones would form an irregular square. The intervals between the stones are from 9 to 12 feet. The stone at the south-west angle is higher than the others, reaching to a point 5 feet above the ground. The other three stones lie on their sides.

4*th*, There is *another group of stones*, five or six in number, *on Hatton Hill*, about 500 yards to the east of the hill top, and about 20 feet below its level. Each of these stones is on average about a cubic yard in solid content, and weighs about two tons.

Hatton Hill is at its top about 900 feet above the sea. The farm of Glenballoch, on which most of the other stones are, is about 750 feet above the sea.

To revert now to the stone first mentioned, the annexed woodcut will give an idea of its shape. The cups or cavities on its sides—which, however, are not well shown on the diagram—are from 2 to 3 inches in diameter, and from half an inch to one inch deep. The grooves are about half an inch deep and about half an inch wide.

The cup-shaped cavities were first noticed about fourteen or fifteen years ago, by the Rev. Mr Herdman, and were shown by him to Dr Wise, a well-known archæologist. At that time the part of the stone above the surface of the ground measured about 9½ feet

from the top, and in that part of the stone there were only five or six cups discernible; plaster casts of these, however, were taken and sent to the Society of Scottish Antiquaries. No doubt was entertained by those who then examined the stone and the casts, that these cup cavities were artificial and not natural.

About six years ago the late Sir James Simpson turned his attention to the subject of these antique and mysterious cuttings and sculpturings, and drew out a memoir on the subject, illustrated by numerous lithographs, which was published by the Society of Antiquaries.

Mr Herdman having heard of this inquiry, was induced to make a farther examination of the stone, and had some of the earth cleared away from its sides. He then discovered other hemispherical cavities sharper and more distinct than those in the higher and more exposed part of the stone, and which greater distinctness he naturally ascribed to the covering of earth by which they had been protected from the weather. He also on this occasion observed that there were grooves or ruts on the surface of the stone, in the parts which had been covered up, and which were prolonged into grooves on the upper part of the stone where they were more faint.

It will be seen from the diagram,—*first*, that on the middle of the stone and near the cups there are two long grooves, with a cross groove at two places; *second*, that at the right hand there is a zigzag groove; and *third*, that at the left hand there is a straight groove, running up vertically, but more faint than the others. The second and third of these grooves were only discovered lately, and in consequence of investigations for the Boulder Committee.

Whenever the discovery of these additional cups and grooves was made, Mr Herdman lost no time in sending an account of them to Sir James Simpson. But by this time his memoir had been printed; and the only notice which appears in that memoir of the Glenballoch Stone, is in the following terms, p. 15:—

"*Circle at Craighall, Perthshire.*—Cup excavations exist upon an erect stone standing at a megalithic circle behind Craighall House, Blairgowrie. The cups are five or six in number, and placed in a group near the foot of the stone."

The account is incorrect in several particulars. Instead of there being only five or six cups, there are thirteen or fourteen. The four vertical and three transverse grooves are not mentioned. There is no reason to suppose that a circle of stones ever existed here. In fact the rapid slope of the ground, where the boulder stands, would have prevented such a circle being made. Megalithic circles are always on a flat piece of land. Sir James Simpson was never at Glenballoch, as he told Mr Herdman himself shortly before his death.

Whilst to Mr Herdman belongs the merit of discovering these markings, the still greater merit belongs to him of having saved this boulder from the fate which has befallen several others in his parish, and hundreds, or probably thousands, equally curious throughout Scotland. The boulder stands within the precincts of a field which bears good crops, and as it was a considerable obstruction to farming operations, the tenant about six years ago was preparing to break it up, and the more especially as he was then in want of stones for a new farm-house. His intentions having become known, the Rev. Mr Herdman would have applied to the proprietor himself had he been at home, to save the boulder. But he was abroad; and so the factor was appealed to, and fortunately with success.

The tenant has several times since thrown out dark hints about the inconvenience to which he is exposed by the presence of this boulder in an arable field, and also by the occasional visits of the curious to examine it. He has recently spoken of the damage done to his "neeps" by Mr Herdman's excavations; and it was only after much persuasion that Mr Herdman obtained from him a promise in these words, "Weel, I'll lat the stane alane, if you dinna *howk* muckle mair about it." Notwithstanding this assurance, Mr Herdman thinks it might be as well that the Royal Society Committee should communicate with the proprietor, Col. Clark Rattray, and ask him to give strict orders for the preservation of the boulder.

These remarks apply to the Glenballoch stone only in its archæological relations. But it is probably also interesting geologically. Mr Herdman states that he has not much knowledge of rocks, and no experience in geological researches. Nevertheless, the facts

related by him suggest some questions of considerable importance. He has had the kindness to send chips of all the stones specified by him. Mr Herdman describes them as, in his opinion, a black coloured trap. But they appear to be all bits of micaceous schist. The prevailing rock in the parish of Rattray is a coarse red sandstone—probably Old Red Sandstone, containing thick beds of coarse conglomerate.

The nearest rocks of micaceous schist are in the hills to the north and west. How far off they are it is not stated, nor how much higher in level than Rattray parish. But it is pretty evident that all these boulders came from the hills, and by natural agency of some kind. The stone of Glenballoch, weighing as it does 25 tons, must have come in that way; and it is almost certain that it now occupies the spot and position on which it was originally placed. The other stones specified by Mr Herdman probably do not now occupy their original site and position, as they seem to have been set up for the purposes—whatever these were—for which they were wanted. Probably the group of stones near the top of Hatton Hill are in their original position, for they do not seem to be artificially arranged; and, moreover, it is not uncommon to find boulders in heaps near the tops of hills, as if these hills had somehow obstructed the farther progress of the agent (whatever that was), which transported the boulders, and caused it to discharge its cargo on or near the top of the hill.

Assuming, then, that the stone of Glenballoch is an erratic from some northern or westerly point, one question would be, What caused the transporting agent to drop it at the place where it now stands? Why should it not have been carried farther? Perhaps an examination of the country might suggest data to aid in the solution of this question.

The position of the boulder and its attitude appear to deserve attention, provided it can be correctly assumed that they were received by natural and not by human agency.

Mr Herdman states that the boulder stands in a field which slopes pretty rapidly down towards a stream, running through a narrow glen. This field seems to form one side of that glen, or small valley, through which, he says, there was formerly a pass much

frequented between Craighall and Banff; and "*balloch*" is a Celtic word for "*pass*." How high above the bottom of the glen the boulder stands, Mr Herdman does not explain. The boulder, therefore, stands in rather a critical position; and considering its great weight, it does not seem likely that it could have been put into that position by human agency.

Then its attitude is singular, because boulders having a longer and shorter axis are generally and naturally found lying with their longer axis parallel with the ground; but this boulder has its longer axis vertical, and stands on a basis of only 8 feet square. If the present position and attitude are those it received when it fell from the agent which transported it, what was the nature of the agent which allowed it to fall, so as to take that attitude?

The two theories for the transport of such boulders are *land ice*, as by a glacier, and *floating ice*, as by an iceberg or ice floe. Whether the country between Rattray parish and the mountains to the north is of such a nature as to have allowed the formation of a glacier may be a question, but supposing it were, which of these two ice agents, glacier or floating ice, would have been most likely to cause this pear-shaped block to fall into the position and attitude which it occupies? This is a question as much for a mathematician as for a geologist to solve.

5. On the Fruiting of the Ipecacuan Plant (*Cephaelis Ipecacuanha*, Rich.) in the Royal Botanic Garden. By Prof. Balfour.

The cultivation of the Ipecacuan plant in this country has received an impetus from the demand on the part of His Grace the Duke of Argyll, for a large supply of fresh plants for India. The object of the India office is to cultivate the plant extensively, and thus prevent the evils which might arise from scarcity of a drug which is so important in the treatment of dysentery. The risk of such an occurrence is due to the mode in which the plant is gathered in Brazil, and the want of care in preserving it. A similar fate threatens Ipecacuan as that which has occurred in the case of Cinchona.

The Secretary of State for India has, in the first place, endeavoured to introduce the plant from this country—leaving for after consideration the propriety of getting specimens sent direct from Rio Janeiro to India. The plants in this country have been supplied from various sources. The original specimen, cultivated by Sir William Hooker in Glasgow, came from Liege, and the Messrs Lawson have imported recently a quantity of specimens from Belgium and Germany. In the Royal Botanic Garden of Edinburgh we are indebted for specimens—first, to Sir William Hooker; and, secondly, to Dr Gunning of Palmeiras, Rio Janeiro. Sir Robert Christison has taken a warm interest in the subject, and has aided much in procuring specimens. Mr M'Nab found that by cutting the root of the original garden plant he could propagate it easily, and in this way he secured a large stock. He gave to the Botanical Society of Edinburgh a notice of his mode of cultivation. This account was printed for the India office, and copies of it were extensively distributed. The specimens from Rio Janeiro were treated in a similar manner.

The plants were sent to India in Wardian cases, sometimes under charge of gentlemen of the forest department going to India, and sometimes without any one in charge. The results have been very successful.

The Duke of Argyll has forwarded to me a report by Dr G. King, superintendent of the Botanic Garden, Calcutta, to whose care the cases were consigned.

From Dr G. King, *Superintendent, Botanic Garden, Calcutta, to the Secretary to the Government of Bengal.*

"I have the honour to report, for the information of Government, the arrival from England of five consignments of Ipecacuanha plants. Five of these consignments, consisting of a single case each, were brought out under the care of Messrs Walton, Whittall, Jellicoe, Ferrais, and Gamble, officers newly appointed to the Forest Department. The sixth, consisting of three closed Wardian cases, came as deck-baggage on board the Suez Canal steamer, 'City of Mecca,' under the special care of no one.

"As will be seen by the following tabular statement, the total number of plants despatched from England was 277. On arrival

in Calcutta 15 plants were found to be dead, and 36 in a sickly state, leaving a balance of 226 healthy.

	Healthy.	Sickly.	Dead.	Total.	
Brought by Mr Walton, .	12	...	...	12	
„ Mr Jellicoe, .	...	26	4	30	
„ Mr Ferrais, .	12	...	...	12	120 Botanic Garden.
„ Mr Gamble, .	27	2	5	34	
„ Mr Whittall, .	26	4	2	32	
Received *ex* 'City of Mecca,'	149	4	4	157	Lawson
TOTAL, .	226	36	15	277	

"It will be observed that the mortality and sickness has been greatest amongst the plants brought out under the care of the members of the Forest Department. I have no doubt this result is due to over-kindness during the voyage. The plants have been apparently freely watered and over-shaded; and in the close and moist atmosphere of the cases, unnatural forced growth has been the result. Mr Gamble's consignment is an exception, the plants brought out by him being in quite as good health as those that came untended in the 'City of Mecca.' The condition of the latter is wonderfully good, and indicates extreme care in the selection of plants, and in the mode of packing them.

"As soon as the plants shall have recovered a little from their journey, I propose to despatch them to Sikkim.

"I take this opportunity of stating that the twelve plants brought out in July last by Mr Walton were forwarded to Sikkim three months ago, and that eleven of them are now in excellent order; the twelfth unfortunately died during the journey to Sikkim.

"The condition of the eleven plants just alluded to, of the five old plants formerly sent from this garden to Sikkim, and of the young ones propagated from them, leads me to entertain hopes that in that province the Ipecacuanha experiment will be attended with great success."

A question has been started whether there are not plants in India which may be used as Ipecacuan. One of these is the *Tylophora asthmatica*, W. et A., an Asclepiadaceous plant, which

has been known under various names:—*Cynanchum Ipecacuanha*, Willd.; *Asclepias asthmatica*, Roxb. Fl. Ind.; *Cynanchum vomitorium*, Lam. Dr Roxburgh and Dr Anderson used the plant for dysentery in India with great success.

There are some peculiar features in the plant now under cultivation which require investigation, and I am not able to give a full paper on the whole subject until further cultivation. The plant which has been long in the garden has flowered regularly. Even the young cuttings have sent forth their flowers. The plant, on the other hand, sent from Rio Janeiro, although treated in the same way as the other, has not flowered.*

The former, although flowering freely, has not produced perfect fruit until the present year. The plants were carefully fertilised by the application of the pollen of one flower to the stigma of another. By this means we have secured a number of fruiting specimens, and I now exhibit fruiting plants with drawings of the fruit and sections.

The fruit is drupaceous, of a dark purple colour, shining and glossy on the outside. It is about the size of a large currant, and when ripe it falls off easily. Each fruit contains two seeds. These are seen in the section of the fruit. The albumen of the seed is very hard. I have not seen any figure of the fruit in botanical works containing plates of the plant. There is a resemblance between it and that of *Psychotria emetica*.

We expect that some of the seeds will ripen, and that we shall then be able to propagate the plant from seed.

The following Gentlemen were elected Fellows of the Society:—

George Seton, M.A. Oxon., Advocate.
Captain Charles Hunter.

* Since this communication was made the plant has flowered, and has shown peculiarities in the relative length of the stamen and pistil. July 1872.

Monday, 1st April 1872.

PROFESSOR SIR ROBERT CHRISTISON, BART., President, in the Chair.

The following Communications were read:—

1. On Cardiocarpon. By Professor Duns, D.D., F.R.S.E., New College.

The attention of the Society was called to many beautiful specimens of Sphenopteris laid on the table. These had been obtained by Dr Duns and his predecessor, Dr Fleming, from the old workings in the Burdiehouse limestones, near Edinburgh, well known from Hibbert's Memoir (1835), and from the papers of more recent observers. The species exhibited were chiefly *S. artemisiæfolia* and *S. affinis.* An Antholite (*A. Pitcairniæ*) was also shown, in which the pedicels that spring from the flower-like buds in the axils of the bracts, sub-opposite in the spike, are well represented. The author then referred to *Cardiocarpon*, Brong., and to the species named by Brongniart, Lindley, and Hutton, and more recently by Dawson and Lesquereux. It was pointed out, that very many *Cardiocarpa* occur in association with the specimens of Sphenopteris on the table. On three of these alone there are above 160. Of these, some are almost globular, others are oval. Some taper to a single sharp point; others, and the majority, have an acute bifid apex. In many the medial ridge is not seen, in others it is highly marked. In a few this ridge has an excurrent appearance, both at the apex and at the supposed point of attachment to the plant. Many of the forms are so placed as to present an appearance of organic connection with the Sphenopterides. The author then showed that it "is needful to guard against a tendency to give undue importance to the mere fact of association. If in other departments this has lead to most erroneous inferences, it will be sure to mislead in the study of palæobotany. Some weight is, no doubt, to be given to the fact, but to use it to any extent as a guide in determining the affinities of fossil plants is, to say the least, not safe. Principal Dawson has pointed to the occurrence

of *Cardiocarpa* along with the stems of *Sigillaria* as corroborative of the theory of the conifer or cycad character of *Sigillaria*. He says, "Some botanists, conspicuous among whom is Brongniart, hold that *Sigillaria* were gymnospermous plants allied to Cycadaceæ. Others are disposed to regard them as Acrogens, and as closely allied to Lycopodiaceæ. . . . . . In favour of the former view we may adduce the exogenous structure of the stem of *Sigillaria*, and the obvious affinity of its tissues to these of conifers and cycads, as well as the constant association with trees of this genus of the evidently phanerogamous fruits, known as *Trigonocarpum* and *Cardiocarpum*." And he adds, "The higher *Sigillariæ* unquestionably resemble cycads in the structure of their stems. Their long, rigid, narrow leaves may be compared to single pinnæ of the leaves of cycads. Their cord-like rootlets, as I have ascertained by actual comparison, are similar to those of cycads. If their fruit was of the nature of *Cardiocarpon* or *Trigonocarpum*, this would also correspond." (See *Quarterly Journal of the Geological Society*, *May* 1871.) This assumes throughout that palæobotanists are agreed as to the nature of these fossil fruits, which is far from being the case.

In August 1870, Mr C. W. Peach, to whom Scottish natural science is so much indebted, found specimens of *Cardiocarpon* organically united with a plant long known by the name, *Antholites Pitcairniæ*. The specimens were obtained from carboniferous shale at Cleuch, near Falkirk. Specimen No. 16, on the table, is *Antholites Pitcairniæ*, from shale near Bathgate. By the kindness of Mr Peach, I am able to show the Society an example of *Antholites* with the fruit organically attached. The importance of this discovery is at once recognised. In a department where facts are the letters, and their association the words by which we read the history of creative manifestation, every worker will acknowledge the value of an observation like that referred to, even though he may not see his way to accept views implying generic identity between the fruit now associated with *Antholites* and *Cardiocarpon*. On the assumption of this identity, Mr Carruthers has recently limited the term *Antholites* to the place, or rather the use assigned to it by Brongniart—"Les espèces indéterminable sont généralement désignées sous le nom d'Antholites."—*Prod.* p. 149. In-

stead of *Antholites Pitcairniæ*, Lindley, he has proposed *Cardiocarpon Lindleyi*, Carruthers. (*Geolog. Mag., Feb.* 1872., pp. 54–57.) Along with a figure of the Falkirk specimen, another is given from an unknown locality, supposed to be from mines in Derbyshire. The fruit on the latter is regarded as similar to *Cardiocarpon acutum* of Lindley.

It was stated that, so far as the author is aware, there is no certain record as to the form of the fructification of such Sphenopterides as *S. artemisiæfolia* and *S. affinis*, or, indeed, of any of the species closely related to these by their bipinnate leaf and the deep pinnatifid segments of their leaflet. Gœppert and Unger's statement, that the fructification is "punctiform or marginal," may be true of species like *S. dilata*, or *S. latior* (Dawson), but these differ widely from the specimens now noticed, though they bear some resemblance to living forms. As regards *S. artemisiæfolia*, Brongniart himself has said, that he has not been able to find the least resemblance between it and living ferns. It was shown that this remark is especially applicable to *S. affinis*. The question seemed to be raised by what might be said to be the almost constant association of *Cardiocarpa* with these two species, "Have they their proper place under the genus *Sphenopteris?*" Dr Duns stated in conclusion, that while these species must still be regarded as true ferns, and while the idea even of organic connection between such forms as the samaroid fruit *Cardiocarpon* and the species *S. artemisiæfolia*, and *S. affinis* is opposed to all accepted views of plant affinity, yet the association, as shown in the numerous specimens on the table, is so frequent, and often so remarkably like organic, as to call for the attention of observers.

2. On the Composition of the Flesh of the Salmon in the "Clean" and "Foul" condition. By Sir Robert Christison, Bart.

Having had occasion lately to fill up some blanks in a table of the Nutritive Value of different kinds of Food, I was unable to find for the purpose an analysis of the flesh of the Salmon. I have therefore made such an analysis as is necessary; and as

the results may be useful to others, I beg to offer them to the Society.

I first examined the composition of a very fine "Clean" fish, caught in the estuary of the Tay in May last year, and weighing 20 pounds. I have never seen a finer fish from that far-famed salmon-river.

I have also, in contrast with this, examined a "Foul" fish, or Kelt, taken in the beginning of March last from a pool where spawned fish are known to congregate at that season in the Isla, a principal tributary of the Tay. It weighed 27 pounds the day after it was caught, and would probably have weighed 35 pounds in good condition. In order to account for my being in lawful possession of such an article, I must mention that I owe it to the consent of the Commissioners for the Tay Fisheries, whose kindness in presenting, for a scientific object, what otherwise cannot be easily obtained without infringing the law, may receive, as I hope, some return in the additional proof which analysis supplies of the inferiority of the salmon as food when in the state of a Kelt, and the folly of destroying it before it recovers condition.

The clean salmon of last May presented abundance of fat under the skin, and in masses betwixt the muscles. Avoiding all accumulations of fat in mass, I cut one piece of muscle from the dorsal region a little in front of the dorsal fin, and another from the ventral region directly opposite; so that the one should represent the "thick," and the other the "thin," of a slice of salmon Four hundred grains of each being cut into fine chips about twelve hours after the fish was caught, each was separately exhausted by ether; and the ether was distilled off at a gentle heat. When the residual oil was deprived of a little adhering alcohol and water by heating it gently for an hour in an open vessel, it had a bright amber colour, and a strong odour not very different from that of cod-liver oil. The fibrous residuum was dried at 212° till it ceased to lose weight. A portion of the dry residue was incinerated in order to determine the fixed saline constituents. The difference denoted the dry nitrogenous nutritive principles, fibrin, albumen, and extractive matter usually called osmazone.

The results were as follows:—

	Dorsal.	Abdominal.	Mean.
Oil	16·66	20·4	18·53
Fibre, albumen, extractive matter .	20·57	18·82	19·70
Saline matter .	0·88	0·88	0·88
Water . . .	61·89	59·90	60·89
	100·00	100·00	100·00

The Kelt of last March was as ugly a specimen of the *Salmo Salar* as I have ever seen. It was 38 inches long, weighed 27 pounds, and was very lank in the belly, soft in the flesh, much lacerated in the dorsal fin and tail, and of a uniform, disagreeable, mottled-grey colour over the entire skin. In its structure otherwise it was a true male salmon. I subjected it to analysis in the same way as the clean fish, with the following results. The analysis was made about forty-eight hours after the fish was caught; and in the interval it was shut up in a box, so that there could not have occurred any appreciable loss by evaporation.

	Dorsal.	Abdominal.	Mean.
Oil	1·2	1·30	1·25
Fibrin, albumen, extractive matter .	16·92	17·22	17·07
Saline matter [inferred from the former analysis] . . .	0·88	0·88	8·88
Water	81·0	80·60	80·80
	100·00	100·00	100·00

Thus it appears—1. That the nitrogenous solids of a Clean salmon, and its oil or fat, constitute together in round numbers 38 per cent of its flesh; the remaining 62 per cent being water, with a little saline matter (0·9 per cent.). 2. That the fat and the nitrogenous constituents are nearly equal to one another. 3. That there is decidedly more fat in the "thin" or abdominal region than in

the "thick" or dorsal region, but somewhat less of nitrogenous constituents. 4. That there is very little difference in constitution between the dorsal and abdominal regions of a "Foul" fish or Kelt. But, 5. That the Kelt is a much more watery fish than the clean salmon; and that this is slightly owing to a deficiency in nitrogenous ingredients, but much more to an enormous deficiency of oil or fat,—which is reduced to almost a sixteenth only of its amount in a clean-run fish.

I am not aware of any good authority for the prevalent notion that a Kelt is unwholesome food. But it is plain from the foregoing analysis, that the Parisian gastronome,—who, before the late stringent measures against river-poaching in Scotland during close-time, consumed a large proportion of Scottish Kelts,—must have been indebted for his enjoyment therein much more to his cook than to his fish. On the other hand, it is easy to see why an Apicius, whose taste has been cultivated on the banks of a Scottish salmon-river, should wonder how any one can imagine, that the delicate flavour of a fish in good condition is improved by besmearing it with butyraceous sauces, simple or compound.

3. On Recent Estimates of Solar Temperature. By James Dewar, Esq.

(*Abstract.*)

After referring to the recent discussion on the temperature of the sun, in which Secchi, Zollner, Vicare, Deville, and Ericsson have taken part, the author proceeds to group all the known methods of arriving at a knowledge of high temperatures under eight different processes. The following table gives the names of the physicists who have specially employed each process, together with the principle on which it is founded:—

(1.) Guyton and Daniell, Prinsep, &c.—Expansion of Solids and Gases.

(2.) Draper.—Refrangibility of Light.

(3.) Clement and Desormes, Deville.—Specific Heat.

(4.) Becquerel, Seamens.—Thermo-Electricity and Electric Conductivity.

(5.) Bunsen, Zollner.—Explosive Power of Gases.

(6.) Newton, Waterston, Ericsson, Secchi.—Radiation.

(7.) Thomson, Helmholtz.—Mechanical Equivalent of Heat.

(8.) Deville, Debray.—Dissociation.

After treating of the great disparity of opinion regarding the temperature of the sun, the author proceeds to detail how it is possible, from the known luminous intensity of the sun, to derive a new estimate of solar temperature. This calculation is based on a definite law relating temperature and luminosity in the case of solids, viz., the total luminous intensity is a parabolic function of the temperature, above that temperature where all kinds of luminous rays occur. So that if T is a certain initial temperature, and I its luminous intensity, a a certain increment of temperature, then we have the following relation:—

$$\mathrm{T} + n(a) = n^2\,\mathrm{I}\,.$$

The temperature T is so high as to include all kinds of luminous rays, viz., 990° C., and the increment a is 46° C. This formula expresses well the results of Draper, and I have used his numbers as a first approximation. It results from the above equation, that at a temperature of 2400° C., the total luminous intensity will be 900 times that which it was at 1037° C. Now, the temperature of the oxyhydrogen flame does not exceed 2400° C, and we know from Fiseau and Foucalt's experiments that sunlight has 150 times the luminous intensity of the lime light; so that we only require to calculate at what temperature this intensity is reached in order to get the solar temperature. This temperature is 16,000° C., in round numbers. Enormously high temperatures are not required, therefore, to produce great luminous intensities, and the temperature of the sun need not, at least, exceed the above number. Sir William Thomson, in his celebrated article, "On the Age of the Sun's Heat," says, "It is almost certain that the sun's mean temperature is even now as high as 14,000° C.," and this is the estimate with which the luminous intensity calculation agrees well.

4. On the Temperature of the Electric Spark. By James Dewar, Esq.

(*Abstract.*)

The author begins this paper by calculating the highest hypothetical temperature that could be produced by the chemical combination of the most energetic elements if all the heat evolved could be thrown into the product. This would not exceed 19,500° C. in the case of silica, and 15,000° C. in the oxides of aluminum and magnesium, and these are the highest results. The estimation of the temperature of the electric spark is based on the thermal value of each spark, together with the volume of the same. The methods of observing these quantities are fully detailed in the memoir. The general result may be stated thus,—the temperature of the electric spark used in the experiments ranged between 10,000° C. and 15,000° C.

The following Gentlemen were admitted Fellows of the Society:—

James Thomson Bottomley.
Thomas Knox, Esq.
Dr D. Argyll Robertson.

Monday, 15th April 1872.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Action of Water on Lead. By Sir Robert Christison, Bart.

After summarising the conclusions at which he had arrived from numerous experiments made more than forty years ago, as published in his Treatise on Poisons, and in the Transactions of this Society, the author alluded to various blanks left at that time in the inquiry which had not been yet filled up, and to various criticisms and doubts which had been recently expressed relative to the facts and principles formerly announced.

The general results of the former inquiries are—1. That the purest waters act the most powerfully on lead, corroding it, and forming a carbonate of peculiar and uniform composition; 2. That all salts impede this action, and many prevent it altogether, some of them in extremely minute proportion; and 3. That the proportion of each salt required to prevent action is nearly in the inverse ratio of the insolubility of the compound which its acid forms with the oxide of lead. The effect of certain inorganic and organic ingredients of water in modifying the preservative power of the salts was not investigated, but has been since made the subject of numerous observations and inquiries by others, chiefly, however, of a desultory nature, some of them much too succinctly described, and some also of questionable accuracy.

The first part of the present paper dealt with the influence of inorganic substances. The second part, on the influence of organic matters, was reserved for a subsequent article.

It had been denied thatwater acts by reason, and in the ratio, of its purity; and it had even been alleged that distilled water itself does not act, if really quite pure. The author, however, had invariably found the reverse, and could assign no other explanation of these statements except some error in manipulation. For example, a very pure spring water was sent to him from the south of England, with the assurance that it had been found to be incapable of attaching lead. But, on making trial of it, he had found it act with an energy not inferior to that of distilled water. Also, it had been stated that ordinary distilled water is apt to contain a trace of nitric or nitrous acid, from nitrates incidentally present in the water subjected to distillation; and that such water, if distilled after the addition of a little potash to fix the acid thoroughly, yields a distillate which has no action on lead. But when the author prepared distilled water in this way, with great care to prevent the access of impurities from other sources, the only result was that the action was even greater than that of the ordinary distilled water of the laboratory, and so great as he had never observed before.

An interesting statement had been made by Dr Nevins, that some salts appear to allow of a certain action going on when they are present largely in water, although their influence, when they

exist in very small proportion, is to act as preventives. The author sometimes obtained the same result, and found the action such as might prove dangerous. But its limit requires to be defined; and there is reason to suppose that the proportion required to permit action will be found so great as never occurs in the instance of waters applicable to household use.

It has been also stated, but in general terms, without experimental proof, that the presence of carbonate of soda, even in a hard water, takes away the preventive influence of other salts, and enables water to dissolve lead. There appears to be some foundation for this statement. But here, too, it is necessary to fix what is the limit to such influence before its importance can be valued. Moreover, as bicarbonate of soda appeared to the author to have no such effect, and this is the usual form of the carbonate in natural waters, the practical importance of the fact is inconsiderable.

The author called attention to some observers not having understood the nature of the corrosive action of water on lead, and having confounded it with other causes of corrosion. Thus the true action has been confounded with the corrosive action of potent agents accidentally coming in contact with the metal in presence of water, —as, for example, when a lead pipe has been led through fresh mortar which is frequently or permanently kept moist, or when lumps of fresh mortar have been allowed to fall upon the bottom of a lead cistern. Several remarkable examples of rapid corrosion of this local kind were exhibited. The true or simple action of water had been not infrequently confounded also with the effects of galvanic action. Thus, if a lead pipe or cistern be soldered with pewter-solder, and not with lead, erosion takes place near the line of junction of the solder with the lead, of which characteristic examples were shown. The presence of bars of other metals crossing lead, or bits of them lying on it, will also develope the same action; and some facts seem to point to the same property being possessed in a minor degree by some stony and earthy substances. This observation may explain the local erosion sometime observed in cisterns containing hard water; since, if galvanic action be excited, it will be increased by saline matter existing more largely in these waters than in soft, or comparatively pure, water.

Lastly, some observers have contradicted former statements,

because in certain circumstances, which led them to anticipate no action, they nevertheless found lead in water, but only in extremely minute and unimportant proportion. The test for lead, the hydrosulphuric acid, when employed in the way now usually practised, is so delicate as to detect that metal dissolved in ten million parts of water, and even more. But facts warrant the conclusion that the impregnation must amount to at least ten times as much before water can act injuriously on man, however long it may be used.

2. On the Preservation of Iron Ships. By James Young, Esq., of Kellie.

My attention was called in January last year to the rusting of iron vessels by observing that the bilge water of my yacht (the "Myanza," 214 tons) was much discoloured by red oxide. Knowing that bilge water is apt to become acid, and thus to attack iron, the result was easily accounted for. Even when the water does not become acid, we may expect some action on the iron to take place when sulphuretted hydrogen exists, as it frequently does there, in which case, first a sulphide, then an oxide, and some sulphate, are formed. The remedy seemed to be easy, because the acid can be neutralised by lime. This earth would also prevent the formation of sulphuretted hydrogen.

I put this immediately into practice, adding lime until the bilge water was alkaline; and samples were taken every fourteen days, which showed the amount of rust to be rapidly diminishing. After six months the liquid became perfectly clear, so that the cure is complete. The yacht is a composite one, and the action is therefore greater than in iron vessels generally, because of the copper or cupreous bolts which are used. These bolts cause galvanic currents with the iron, and greatly assist in its oxidation and solution.

As a very little lime will last a long period, the plan causes neither trouble nor expense. Seeing in the newspapers that the destruction of the "Mægara" was attributed to the action of bilge water, I thought that my experience might be of some value.

3. First Report by the Committee on Boulders appointed by the Society.

In April 1871, a paper was read in this Society proposing a scheme for the conservation of boulder or erratic blocks in Scotland, in so far as they were remarkable for size or other features of interest. The Council of the Society approved of the scheme, appointed a committee to carry it out, and agreed to aid in meeting the expense of any circulars which might be necessary for conducting the inquiries.

The objects of the committee were twofold. They were first to ascertain the districts in Scotland where any remarkable boulders were situated; and, second, to select those which might be deemed worthy of preservation, with the view of requesting landed proprietors and tenants of farms not to destroy them.

The labours of the committee have as yet been directed only to the first of these objects.

In order to procure information, they drew out a set of printed queries, applicable to boulders apparently above 50 tons in weight, in order to ascertain the parishes in which they were situated, and the names of the proprietor and tenant on whose lands they were; and also to learn other features, such as the nature of the rocks composing the boulders, their form, and the existence of striations upon them. Inquiry was also made whether the boulders had any traditional names or popular legend connected with them, or exhibited any artificial markings.

The committee thought that, with a view to the conservation of the boulders, the greater the interest which could be shown to attach to them, the more chance there would be of inducing proprietors and tenants to preserve such as the committee might select for preservation.

Besides queries about boulders, there was one query directed to ascertain the occurrence of *kaimes* or *eskars*, *i.e.*, long banks of sand and gravel, as some persons imagined that the agents which transported boulders might have had some relation with, or might throw some light on those which were concerned in the formation of those deposits.

Circulars containing queries, a copy of the minute of Council

approving of the scheme, and appointing a committee, and an abstract of the paper read in the Society in April 1871, explaining the scheme, were transmitted to the ministers of all rural parishes in Scotland.

About 700 circulars were issued. After the lapse of six months about 100 answers were received.

The committee, on considering these, were of opinion, that in making their queries applicable only to boulders exceeding 50 tons in weight, they had probably erred, by excluding many boulders of interest, and to this circumstance they attributed the small number of answers sent.

They therefore resolved to issue another circular containing the same queries as before, to cover boulders exceeding 20 tons in weight. This circular was addressed to parochial schoolmasters, as the committee feared that they might be considered troublesome, if they made a second application to ministers of parishes.

This second circular brought to the committee a large amount of information, and they desire now to express their cordial thanks to both classes of reporters for responding so readily.

When the committee was appointed, an expectation was expressed that they should, from time to time, lay before the Society some account of their proceedings, and of the progress made by them.

In now proceeding to the performance of this duty, the committee will confine themselves to a statement of facts communicated, and avoid at present attempting to draw conclusions from these facts.

1. In order to show the situations of the boulders reported on, the committee have drawn up a list,* according to counties, giving the names of the parishes where boulders occur, adding shortly any particulars regarding them, such as size, nature of the rock composing the boulder, direction of the longer axis, striations, popular names, and legend, if any.

They have also, on a general map of Scotland, indicated by a red cross the exact position of the most remarkable boulders.

From this table and map, it will be seen that *Aberdeenshire* possesses the largest number of boulders, and also the boulders of greatest magnitude.

* This list is in the Appendix.

Ross and Cromarty stand next, then *Perth*, *Argyll*, *Inverness*, *Kirkcudbright*, and *Forfar*.

2. In regard to *size*, the largest boulder reported is one of granite, in the parish of Pitlochry, called "Clach Mhòr" (big stone), being about eight yards square, and estimated about 800 tons.

There are two boulders between 500 and 600 tons weight, one in Ross, the other in The Lewis.

There are three boulders between 200 and 500 tons, seven between 100 and 200 tons, twenty between 50 and 100 tons.

3. With regard to the *nature of the rocks* composing the boulders, the largest reported are of granite, though there is one known to the convener of the committee, still larger, of conglomerate, in Doune parish. The most numerous are composed of compact greenstone; but these are generally of small size. The next most numerous class are of grey granite. There are also many of gneiss, grey-wacke, and conglomerate.

4. The boulders reported generally *differ* in regard to the nature of the rocks composing them, from that of the rocks of the district in which they are situated; and, in many of the reports, reference is made to the district from which the boulder is supposed to have come.

Thus, in those parts of Perthshire, Forfarshire, and Kincardineshire where the old red sandstone formation prevails, and over which multitudes of granite, gneiss, and conglomerate boulders are lying, most of the reporters have no hesitation in pointing out that the parent rock is in the Grampian range, lying to the north or west. So also in Wigtonshire, where the greywacke formation prevails, and on which many boulders of grey granite are lying, the general opinion is that they came from the granite hills of Kirkcudbrightshire.

But where a boulder happens to be of a species of rock the same as that of the rocks of the neighbourhood, it is more difficult to recognise it as a true erratic. Hence, in the Lewis, where there are huge single blocks of gneiss, which is also the prevailing rock of the country, the reporters say that they cannot tell whether these blocks are erratics or not.

5. The boulders mentioned in the reports are of various *shapes*. Some approach a cube, well rounded of course on the corners and sides. That is the shape mostly possessed by granite boulders.

Others again are of an oblong shape, and this is particularly the case with whinstone and greywacke boulders. The difference in this respect is probably mainly due to a difference in the natural structure of the parent rocks.

A point of some importance occurs in regard to oblong-shaped boulders.

The direction of their *longer axis*, in the great majority of cases, is stated to coincide with the direction in which they have come from the parent rock, when the situation of that rock has been ascertained. Thus, in Auchterarder parish, there is a boulder 10 feet long by 6 broad, the longer axis of which points north-west. In Auchtergaven parish there is a granite boulder 10 feet long by 8 broad, the longer axis of which points due north. In Menmuir parish, Forfarshire, there are two large granite boulders, the one 14 by 9, and the other 13 by 9, the longer axis of which points north-west. In each of these cases the reporters seem satisfied of the situation of the parent rock, and in each case the longer axis of the boulder points towards it.

It appears, also, that where there are natural *striations or ruts* on the boulders, these almost always run in a direction parallel with the longer axis; and that when there are striæ crossing these, the number of such oblique striæ are comparatively few.

6. Notice in the reports is taken of the remarkable *positions* occupied by some boulders.

Thus, the Ardentinny report refers to a large boulder called "*Clachan Udalain*," or the nicely balanced stone,* so-called, as the reporter states, because "it stands on the very edge of a precipice, and must have been gently deposited there." In the same parish there is another boulder, called "*The Giant's Putting Stone*. It is pear shaped, and rests on its small end. It looks," says the reporter, "as if a push would roll it over."

In Menmuir parish (Forfarshire), two boulders are reported, each from 30 to 40 tons in weight, and perched on or near the top of a hill, having come there, as the reporter thinks, from a parent rock 15 miles distant, with several valleys intervening.

Cases of the same kind are reported from islands.

On Iona, near the top of the highest hill in the island, which is

* Another translator represents this word to mean "*of the swivel*."

about 250 feet above the sea, there is a great boulder of granite. There is no granite in the island. The nearest place where that rock occurs is in the Ross of Mull, with an arm of the sea intervening.

In the Island of Eday, in Orkney, there is a conglomerate boulder, called the "*Giant's Stone,*" about 8 tons in weight, near the top of a hill—the only one in the island—about 300 feet high. There is no conglomerate rock in Eday. But conglomerate rock occurs in the Island of Stronsay, situated to the south-east, a few miles distant.

7. The report from the parish of Benholm (Forfarshire), by the Rev. Mr Myres, gives information and suggestions to the committee of considerable interest. On the sea coast of that parish, two sets of boulders are described. One set are supposed to have come from the Grampian range many miles to the north-west, and consist of granite and gneiss rocks. But another set, also consisting of primitive rocks, are believed to be derived from a different source altogether, viz., from the great beds of conglomerate rock, which forms a band crossing the whole of Scotland from Stonehaven and Bervie, in a south-west direction, to Dumbarton and Rothesay. Some of the rounded masses in the conglomerate are stated to be several feet in diameter, and a few present appearances of striation; a fact which, if established, would seem to prove that, at a very early period indeed, ice action had existed, and had formed boulders just as it did at a later period.

This report from Benholm parish was read lately at a meeting of the Geological Society of Edinburgh, and was illustrated by drawings and specimens which afforded strong evidence of the correctness of these views.

8. With regard to *kaims* or long embankments of gravel or sand, there are twenty-three parishes reported to the committee as containing them.

They appear to be most numerous in Aberdeenshire, Forfarshire, and in the east of Perthshire. In Kemnay parish there is a kaim said to be 2½ miles long, running east and west. In Airlie parish there is a kaim 2 miles long, also running east and west. In Fettercairn parish, Kincardineshire, and also in Tarbet parish, Ross-shire, there are several kaims parallel to, and not far distant from, one another.

In two cases the reporters, who seem to have visited Switzerland, whilst mentioning kaims in their parishes, express an opinion that they are evidently lateral and terminal moraines.

In several cases, oddly enough, these kaims exist at much the same level above the sea, viz., between 700 and 800 feet, which happens also to be the height of similar deposits in Berwickshire and Mid-Lothian.

The committee wish it to be understood, that in the present report, they confine themselves simply to a statement of the information received. They do not think it would be wise as yet to attempt to draw theoretical conclusions. Almost every day they are receiving more answers to their circulars; and they think that the wider the basis for considering the important geological questions connected with the transport of boulders and the formation of kaims, there will be the more probability of reaching the truth.

One object which the committee have in view in explaining the nature of the information communicated to them, is to show and to acknowledge the deep debt of gratitude which this society lies under to the gentlemen who have responded to the circulars of the committee.

But whilst the information supplied is undoubtedly valuable, the committee cannot but feel the truth of what many of the reporters themselves modestly and properly state, that they are so little acquainted with geology or mineralogy, that they may not have correctly understood the queries, or they may not have made their observations in the way necessary to answer the queries. Moreover, the committee itself may not in all cases have rightly understood the answers given.

Having regard to these considerations, the committee would very much desire that the boulders reported should be examined by experienced geologists, who should at the same time make a survey of the district, in order to see whether it presents any special features bearing on the nature of the agency by which the boulders were transported. The information in the reports received by the committee would greatly facilitate such an inspection, as they indicate not only the parish and the farm where

the boulder is situated, but generally record other features of interest.

The committee entertain a hope, that were this wish on their part made known, some geologists, who may be either resident in Scotland or who may purpose to visit Scotland during the course of the ensuing summer or autumn, might offer their services in the way, and for the purpose now suggested. In that case, the committee would willingly lend the reports which they have received, on condition that the results of the inspection were made known to the committee.

The committee will place in the library of this Society, the list of boulders before referred to, showing the parishes in each county in which the boulders and kaims are situated, so that any person may see where these parishes are, and be able to judge whether it would be convenient for him to visit these.

Were this list published, and generally circulated, good would result in another way. As it would show all the parishes from which reports of remarkable boulders and kaims had come, some persons might be able to discover parishes from which reports had been omitted to be sent, and if these were pointed out to the committee, they would make the requisite inquiry.

II. The committee proceed next to notice points of *archæological* interest connected with boulders.

1. The committee were surprised with the large number of individual boulders possessing names by which they were known in the district.

The names may be classified under several heads:—*First*, there are names having reference to the agency by which the boulders were supposed to have come into the district. *Second*, there are names indicative of the use to which boulders were put. *Third*, there are names making the boulders commemorative of certain events.

Many of the boulders, besides having a name, have also a *legend*, which explains and illustrates the name.

The *Giant's Stone, Fingal's Putting Stone*, the *Witches' Stone*, the *Carlin Stone, Heathens, Hell Stones*, the *Deil's Stone*, the *Deil's Putting Stone*, the *Deil's Mither's Stone*,—these are among the names, almost all in the Gaelic language, which ap-

parently were given to account for the way in which particular boulders came into the district. *

To show that this was the origin and object of the names, a few of the legends, as stated in the reports, may be given. They indicate, no doubt, a very deplorable state of ignorance and credulity; but they indicate also that in many cases our forefathers had satisfied themselves that the boulders had been transported into the district. Their perplexity was how to account for their transport. Not knowing anything of glaciers or icebergs, they had to resort to supernatural agency for an explanation. A few examples may be given.

Reference has already been made to a large conglomerate boulder near the top of a hill, in the Island of Eday, one of the Orkneys. It goes under the name of "*Giant's Stone.*" The legend for it is, that it was flung by a giant from the Island of Stronsay. Now, as already stated, there is no conglomerate rock which could have supplied the boulder in Eday Island, but there is in Stromsa.

So also in the Island of Sanday, one of the Orkneys, there is a granite or gneiss boulder; the legend about which is, that it was thrown from the Shetland Islands by a giantess, who had been jilted by a Westray man. She intended to throw it into Westray, but she made a bad shot, and it fell into the Island of Sanday. There is no rock which could have produced the boulder in Sanday, but there is abundance of it in the Shetlands.

About 1½ miles west of St Andrew's in Fife, there is a large conglomerate boulder, and the legend about it is, that when the "*Four knockit steeple*" in that town was being built, a giant who lived at Drumcarro Crags, a hill about 5 miles to the north-west of St Andrews, was indignant, and resolved to demolish the edifice. He, therefore, got the largest stone he could find, and borrowing his mother's apron, he made a sling of it, and threw it at St Andrews. But the stone being too heavy, the apron broke, and the stone did not quite reach its destination, and there it has lain ever since. There is no conglomerate rock where the boulder lies, but there is at or near Drumcarro Crags.

* The Rev. Mr Joass of Golspie refers to a boulder in Sutherland, called "*Clach Mhic Mhios,*" or stone of the Manthold son, believed to have been thrown from a hill two miles off by Baby Fingalian.

The Witches' Stone, which is on the estate of Pitferran, near Dunfermline, has this legend: A witch who lived among the hills to the west, wishing to confer a favour on the Pitferran family, resolved to give them a cheese-press, the heaviest she could find. She selected a large block of basalt of the proper shape, and carried it in her apron, which, however, broke under the load before she reached the family residence; and there it has lain ever since. There is no rock of that kind near Dunfermline, but there is to the westward.

In the parish of Carnwath there are one or two spots where there are or have been groups or collections of whinstone boulders, between the river Clyde and a hill of whinstone, known by the name of the *Yelpin Craigs*. The distance between the river and this hill is three or four miles. These heaps of boulders have from time immemorial gone by the name of *Hellstanes*, insomuch that places near them are called *Hellstanes Loan*, *Hellstanes Gate*, &c. The legend is, that Michael Scott and a great band of witches, wishing to dam back the Clyde, gathered stones at the *Yelpin Craigs*, and were bringing them towards the Clyde, when one of the young witches, groaning beneath her load, cried out, "Oh Lord, but I am tired." As soon as she uttered the sacred name, the spell broke, the stones fell down, and have remained there ever since.*

There are many legends founded on the agency of the devil, and on his hatred of churches and clergy. Thus near the old church of Invergowrie, now in ruins, there is a large whinstone boulder, called the *Paddock Stone*. The legend about it is, that the devil, going about in Fife, descried the church shortly after it was begun to be built, and wishing to stop the work, threw a large stone at it across the Frith of Tay. There is no whinstone rock at or near Invergowrie, but there is abundance of it immediately opposite in Fife.

In the parish of Kemnay (Aberdeenshire), there is a boulder of grey granite, called the *Devil's Stone*, estimated to weigh about 250 tons, which lies not far from the old kirk. There is no rock of that nature in Kemnay parish, but there is at Bennachie, a hill about seven or eight miles to the westward. The legend explain-

* This legend is given more fully in "Scenery of Scotland," p. 314, by Professor Geikie.

ing how this boulder came from Bennachie forms the subject of a ballad,* a few verses of which may be given.

" It was the feast o' Sanct Barnabas,
I' the merry month o' June,
When the woods are a' i' their green livery,
And the wild birds a' in tune ;

" And the priest o' Kemnay has gaen to the kirk,
And prayed an earnest prayer,
That Satan might for aye be bund
To his dark and byrnand lair.

" And aye the haly organ rang,
And the sounds rose higher, higher,
Till they reached the Fiend on Bennachie,
And he bit his nails for ire.

" And he lookit east, and he lookit west,
And he lookit aboon, beneath ;
But nocht could he see save the baul' grey rocks
That glower'd out through the heath.

" He lifted aloft a ponderous rock,
And hurl'd it through the air ;
' 'Twere pity ye sud want reward
For sae devout a prayer ! '

" The miller o' Kemnay cries to his knave,
' Lift up the back sluice, loon !
For a cloud comes o'er frae Bennachie
Eneuch the mill to droon.'

" The boatman hurries his boat ashore,
And fears he'll be o'er late ;
Gif yon black cloud come doon in rain,
It 's fit to raise a spate.

" But the ponderous rock came on and on,
Well aimed for Kemnay Kirk ;
And cross'd it field, or cross'd it flood,
Its shadow gar'd a' grow mirk.

" But the fervent prayers o' the haly priest,
And the power o' the Sanct Anne,
They turn'd the murderous rock aside,
And foil'd the foul Fiend's plan.

* From " Flights of Fancy and Lays of Bon Accord." By William Cadenhead Aberdeen. Edinburgh : Oliver and Boyd, 1853.

"And it lichted doon frae the darken'd lift,
Like the greedy Erne bird,—
And there it stands i' the auld kirk-lands,
Half-buried in the yird."

These legends, in explanation of the transport of Scotch boulders, are of the same nature as the legend which professes to explain how the Blue Stones of Stonehenge came to Salisbury Plain in England. Jeffrey of Monmouth, who was the first author to write a description of Stonehenge, says that certain of the stones were brought by Merlin and a band of giants from Ireland. Mr Fergusson, in his book on Ancient Stone Monuments, recently published, says that some geological friends of his have told him, that these blue stones of Stonehenge are a species of trap, which is not known in England, but is well known in Ireland; and therefore Mr Fergusson supposes that they probably were brought from Ireland in ships. It seems quite as likely that these blue stones were boulders, and were brought from Ireland by natural agency, and deposited on Salisbury Plain in that way. There are strong proofs to show that there was an agency of some kind which swept over Ireland from the westward, and brought boulders across what is now the Irish Channel to the south-west districts of England.

In these legends we see the efforts of the people in those early times to account, in the best way they could, for the transport of boulders into their districts. It is evident that they had investigated the subject, and had made considerable approaches to the truth. Finding that many of these great blocks differed in composition from all the rocks of the district where the blocks lay, and inferring that their rounded shapes were probably due to friction, they inferred that they must have come into the district from some distant quarter; and this inference was confirmed by discovering that in certain other districts there was rock of the same description as the blocks. But how blocks exceeding 100 tons weight could have been brought many miles, and over a tract of country uneven and broken in its surface, their knowledge of nature's laws did not enable them to explain. The only agency which they could think of was superhuman and supernatural; and hence the invention of such legends as assumed the agency of Merlin, giants, Michael Scott, witches, and the devil.

2. The second class of names by which particular boulders are known, have reference to the *uses* to which these stones were put.

In remote periods in the history of Scotland, when there were no maps, roads, or even names of parishes, it was important to have some other means of indicating spots or districts where people required to congregate for special purposes.

One of the boulders reported to the Committee (in the Island of Harris), still goes by the name of "*Clachan Treudach*," or the Gathering Stone.

What were the special purposes for which our early forefathers gathered together is of course not easily discovered. But the ancient names of the boulders seem to throw light on the subject. (1.) Such names as "*Clach-sleuchdaidh*," or Stones of Worship (in the parish of Kirkmichael); "*Clach an t-Tobairt*," or Stone of Sacrifice; "*Clach na Greine*," Stone of the Sun; "*Clach na h'Annait*." Stone of Victory, (a Scandinavian deity); and "*Clach mhòr a Che*," Great Stone of Che, (another deity), seem very plainly to indicate that these boulders were used as trysting-places for *worship*; and they were all the more suitable if they were looked upon with superstitious awe, on account of their supposed connection with spiritual agency. On two of the boulders reported to the Committee, there are artificial circular markings, other examples of which are very numerous throughout Scotland; and though archæologists are not yet agreed as to the meaning of these marks, one theory is, that they were symbols of a religious character. It is well known that these great stones were in some way or other, hindrances to the reception and diffusion of Christianity in most of the countries of Western Europe; for between the years 500 and 800 there are numbers of decrees and edicts requiring these stones to be destroyed, as being objects of superstition. There are some archæologists who go so far as to maintain that the word "Kirk" is actually synonymous with the word "Circle," meaning the circle of stones where Celtic worship was performed.

(2.) Another use to which these boulders were applied was *Sepulture*. There is in Berwickshire, a boulder known by the name of the "Pech or Pict's Stone," round which human bones in very large quantities were found a few years ago; and similar discoveries

have been made at boulders in many other districts, especially where they formed circles.

If these great boulders were used as places of worship, it was natural that they should also be used for sepulture, on account of the supposed sanctity of the place. Indeed, the fact of a place having been used for sepulture, creates of itself a presumption that it was used also for worship.

(3.) Another important purpose for which the boulders were used, was for the *trial of offenders* and the issuing of *judicial sentences*. Thus, in Little Dunkeld parish, there is a large boulder called "*Clach a mhoid*,"* or Stone of the place of Justice, where the baron of the district could try *offenders*, with right to hang or drown those convicted. In Ayrshire there is another large boulder called the *Stone of Judgment*, for the barony of Killochan. Several large rocking stones have been reported. In ancient times, when very rude tests of guilt or innocence were employed, the rocking stone was used in the trial of persons accused of crimes.

> "It moves obsequious to the gentlest touch,
> Of him whose breast is pure. But to the traitor,
> Though even a giant's prowess nerved him,
> It stands as fixed as Snowdon."

(4.) There are boulders which are known to have been used as *trysting places* for military gatherings; a large boulder on Culloden Moor is one example. It was on a whinstone boulder called *The Bore Stone*, that Robert Bruce planted his standard before the Battle of Bannockburn. A sandstone boulder on the Borough Muir, near Edinburgh, was the gathering point for the army collected by James IV. before the Battle of Flodden. Both of these stones are in existence. The Bannockburn stone is protected by an iron grating. The other stone is also preserved, being fixed on a wall near Morningside parish church, having on it a brass plate, bearing an inscription, given by the late Sir John Forbes.

(5.) Some boulders are said to have been used as trysting places for the *contracting* of *engagements*, such as matrimonial contracts, and others less important. There is a boulder in the parish of Coldstream (Berwickshire), called the *Grey Stone* from its colour, at which within the last hundred years marriages took place. The

* New Stat. Acc. vol. x. p. 1007.

bride and bridegroom stood on tiptoe on each side of the stone and joined hands over the top, whilst the friends of each party surrounded the stone to witness the engagement. The *Stone of Odin* in the Orkneys, at which marriages were celebrated, was held in peculiar veneration; for in one case where a man was prosecuted for deserting his wife, it was stated to be an aggravation of his offence, that they had been married at the Stone of Odin.

3. A third class of names given to boulders had relation to them as *commemorative of important events.*

Thus there is in Badenoch the "*Clach an Charra*," or Stone of Vengeance, so called because a profligate and tyrannical feudal baron was killed by his own people near it.*

There is in Lewis the "*Clach D'hois*," or Stone of D'hois, a boulder of gneiss, weighing about 120 tons. It is called after a person named D'hois, who slew a giant near the boulder, and who also himself died immediately after, from the wounds received in the conflict.†

4. Some boulders were used to mark the boundaries of estates, parishes, and counties, and are still in many parts of Scotland recognised as affording evidence on that subject.

In Ross-shire, the boundary between the districts of Urray and Contin is marked by the boulder called "*Clachloundron*."

A great boulder is said to indicate the spot where the three counties of Dumfries, Ayr, and Lanark meet.

The line of boundary between England and Scotland was in the eastern borders originally indicated by boulders, several of which still remain.

5. Some of the boulders have curious popular predictions connected with them.

Thus, near Invergowrie, on the north side of the Frith of Tay, there were in the days of Thomas the Rhymer two boulders entirely surrounded by the water, of which the seer sang—

> "When Gows of Gowrie come to land
> The day of judgment's near at hand."

These two boulders, called the Gows (probably because always frequented by sea-gulls), are now no longer surrounded by water.

* Proceedings Soc. of Scotch Antiquaries, vol. vi. 328.

† This Boulder and its legend reported to the committee by Captain Thomas, R.N.

But it is not they which have come to land, the land has come to them.

In the parish of Crieff a boulder of whinstone is reported, with a vein of white quartz through and partially round it, in consequence of which the stone has from time immemorial been known as the *Belted Stane.* The prediction about it is, that the white belt will gradually increase in length till it envelopes the stone; and that whenever the two ends meet, a great battle will be fought, on which occasion a king will be seen mounting his horse at the stone,—

> "'Twixt the Gartmore Gap and *the Belted Stane*
> The nobles bluid shall run like a stream."

Geologists, however, are of opinion that there is not much chance of the quartz vein extending.

Perhaps some persons may think that the time of the Royal Society should not be taken up by any allusion to these absurd popular legends. There are, however, good reasons for referring to them. In the *first* place, they are evidence of the extraordinary ignorance and superstition which prevailed in former times in our own land, and even at no very distant date. In the *second* place, the archæological and even historical associations with which many of the boulders are invested, may induce many proprietors to take an interest in them and save them from destruction, if the committee think them worthy of preservation.

There is even yet among our fellow-countrymen a considerable amount of interest felt in these boulders, and particularly such as have traditionary names and legends; and it is to this feeling that several are indebted for their preservation. Professor Geikie at the last meeting of the British Association told this anecdote of the Ayrshire boulder, known as the Killochan Stone of Judgment. An enterprizing tenant, a stranger to the district, finding this stone much in his way, was preparing to blow it up with gunpowder. His intention becoming known, some of the old residenters went to the laird's factor and asked whether he knew what was intended. On his stating that he did not, he was entreated to prevent the stone from being destroyed. The proprietor was communicated with, and the new tenant was interdicted from meddling

with the stone. Shortly afterwards this inscription was put on the stone,—"*The Baron's Stone of Killochan.*" *

It is a boulder of blue whinstone, on which stands the market cross of Inverness. For some reason or other, it is preserved as the Palladium of the town, ever since the battle of Harlaw in the year 1411. It is called "*Clach na cudden*," or "Stone of the tubs," from the circumstance that the people carrying water from the river used long ago to rest their tubs on it. It was till lately in the middle of the street; but having ceased to be of use, when water was brought into the town by pipes, it was removed to the side of the street opposite to the town hall, with the old cross of the town and the Scottish arms resting on it. "*Clach na cudden boys*," is a *nom de guerre* for Invernessians; and "*All our friends round clach na cudden*," is a toast given in many a distant land.

In the parish of Rattray, there is a remarkable boulder of micaceous schist, weighing about 25 tons, of which some account was given a short time ago in this Society. It bears a number of artificial markings of a very ancient date. The tenant of the farm on which it is situated proposed to blow it up. Some of the inhabitants having heard of this, went to the minister of the parish, and begged him to take steps to save the old stone of Glenballoch. The proprietor being on the Continent, the rev. gentleman applied to the factor, and through his good offices saved the stone. This gentleman being still under anxiety about it, lately requested this committee to communicate with the proprietor, Colonel Clark Rattray, with the view of obtaining from him a promise that the stone should be preserved. Colonel Clark Rattray was accordingly written to by the convener of the committee, and he at once acceded to the request.

There is on the shore at Prestonpans, on the south side of the Firth of Forth, a large basaltic boulder, which has long been known under the name of "*Johnny Moat.*" The Convener wishing to see this boulder, he went out from Edinburgh a few weeks ago by rail to Tranent Station, and walked towards the shore in search of it. Between the railway station and Prestonpans he met a boy, whom he stopped, and telling him that he had come to see

* An account of this boulder was published in Macmillan's Magazine for March 1868, by Professor Geikie.

the boulder called "Johnny Moat," he asked the way. The boy pointed it out at once. Three or four other persons in succession, two of them women, had to be asked the same question before the spot was reached. Every one knew "*Johnny Moat.*" The last person accosted was a fisherman, and he volunteered to be guide. He seemed somewhat suspicious of the stranger's intentions; for after reaching the stone, he remained beside him till he saw it was only to measure its dimensions and make a sketch of it, that he had come. From what was observed during this visit, it was evident that every inhabitant of Prestonpans, not only knew of the boulder, but took a personal interest in it, and would sternly resist any attempt to destroy it.

It is satisfactory to find this popular feeling still prevailing to some extent. But the feeling is not of itself sufficient to prevent the wholesale destruction which is going on in many parts of Scotland. Thus, the minister of Bendochy reports to the committee, that "on the rising ground behind his manse, there was a circle of large stones, boulders, standing on their ends (Druidical); but some years ago they were removed. The place is yet called '*The Nine Stanes.*'"

There was formerly a rocking stone in Aberdeenshire, estimated at about 50 tons weight; but it has now been converted into field dykes.

Numberless cases of the same kind can be specified.

It is therefore most necessary to take steps to preserve what remain of these megalithic relics; and it is especially gratifying to the committee to be able to state, that the movement towards this object, made by this Society, has met with general approval.

The British Association, at its last meeting, so highly approved of the scheme, that it appointed a committee of some of its most influential geologists to carry out a similar scheme for England and Ireland.

In the last number of the "Geological Magazine," there is a laudatory notice of the object and operations of the committee; and the readiness with which all parties applied to in Scotland have responded to the circulars of the Committee, proves how much they also approve, to say nothing of express commendations contained in individual reports. Even in Switzerland notice has been taken

of our Scottish movement, and in very complimentary terms; for a few weeks ago, a pamphlet by Professor Favre of Geneva was received by the convener, alluding to our Society's movement in this matter, and anticipating important results from it.

List of Boulders reported to Royal Society, arranged by Counties and Parishes.

ABERDEEN.

Aberdeen (Town).—In excavating for foundation of house in Union Street, boulder of black sienite, 6 × 5 × 4 feet found. No rock like it *in situ* nearer than Huntly or Ballater, about 30 miles to N.W. or W. Under surface of boulder, striated. The direction of striæ coincides with the longer axis of boulder, viz., about east and west. Preserved, and set up in Court of Marischall College. (Reporter—Professor Nicol.)

Ballater.—On top of Morven, 3000 feet above sea, several granite boulders, unlike rock of hill, and apparently from mountains to west. (Jamieson, "Geol. Soc. Jour.," xxi. p. 165.)

Belhelvie.—Gneiss boulder, about 8 feet diameter, called the "Caple Stone," near parochial school. Rocks *in situ;* near it are granite. (Reporter—Alex. Cruickshanks, Aberdeen.)

Sienite boulder, in a wall, *King Street Road*, about $3\frac{1}{2} \times 2$ feet. The face covered with striæ parallel to longer axis.

Cairney Granite Quarry, 3 miles N.W. of Aberdeen, and about 400 feet above sea. When boulder clay removed, surface of rock found to be smoothed and grooved in a direction E.N.E. and W.S.W. (true.) (Reporter—Alex. Cruickshanks, Aberdeen.)

Bourtie.—1. Four Greenstone boulders, supposed to be Druidical; what is called "The Altar Stone," 16 × 6 × 5 feet, weighs about 18 tons. 2. Boulder, about 20 tons. Longer axis E. and W. Called "Bell Stane," the church bell having once hung from a post erected in it. 3. Whinstone boulder, about 20 tons, on Barra Hill, called "Wallace's Putting Stane," 24 feet in circumference. Legend, that thrown from Bennachie Hill, distant about nine miles to west. 4. Whinstone boulder, called "Piper's Stone." Origin of name given. 5. Whinstone boulder, called "Maiden Stane." Tradition

accounting for name. 6. Several Druidical circles described. (Reporters—Rev. Dr Bisset, and Mr Jamieson of Ellon.)

Braemar.—At head of Glen Sluggan, several large erratics. These stand exactly on watershed or summit level. Near shooting-lodge there, a cluster of four or five immense angular granite boulders. They touch one another, and may be fragments of one enormous mass. The adjacent rock is quartz. These blocks situated at end of a long low ridge or mound, which extends from south extremity of Ben Avon Hills, and which strewn thickly over with great granite blocks. The mound composed of a mixed debris of earth and stones, and is apparently a moraine. The adjoining mountain of "Cairn a Drochid" is composed of quartz and granite. On top of it are large granite boulders, many of which situated on quartz rock. (Reporter—Mr Jamieson, Ellon, in letter to convener.)

Chapel Garioch.—Boulder, $19 \times 15\frac{1}{2} \times 11\frac{1}{2}$ feet, weighing about 250 tons above ground. Height above sea 280 feet. Rests on drift. Longer axis E. and W. Legend, that thrown from Bennachie Hill to north-west. The rock of boulder differs from rocks adjoining. Kaims abound in parish. (Reporter—Rev. G. W. Sprott.)

Cruden.—In Boddom Dean, a granite boulder called "The Hanging Stone," measuring 37 feet in circumference and 27 feet over it, resting on several small blocks of granite. Supposed to be Druidical. Half a mile east there is another of 20 tons. (Buchan's Peterhead, published in 1819, and James Mitchell, Boddam.) Huge granite boulder, called "The Gray Stone of Ardendraught," broken up in 1777 to build walls of Parish Church. It was the stone on which "Hallow" fires* used to be lighted. (Jamieson, "Geol. Soc. Jour.," xiv. p. 525.)

* "Hallow" fires were lighted on 31st October, and were called "Saimh-theine." The "Beil-theine" fires were lighted on 1st May. These practices, formerly general in the Highlands of Scotland, were probably connected with the worship of the sun, whose departure in autumn, and return in spring, were signified by these rites. The Rev. Mr Pratt published an account of Buchan in the year 1858, and states (page 21), "Hallow fires are still kindled on the eve of All Saints, by the inhabitants of Buchan—from sixty to eighty fires being frequently seen from one point." (*Old Stat. Acct. of Scotland*, vol. xi. p. 621, and vol. xii. p. 458.)

At Menie Coast Guard Station, granite boulder, 54 feet in circumference and 7 feet above ground; also a greenstone boulder, 78 feet in circumference and 6 feet above ground. (Jamieson, "Geol. Soc. Jour.," xiv. p. 513.)

Near the "Bullers of Buchan," there stands "The Hare or Cleft Stone," which marks the boundary between the parishes of Cruden and Peterhead. Granite 9 × 8 feet, 160 feet above sea. (Pratt's "Buchan," 1858, page 47, and James Mitchell, Boddam.)

In this parish, and to north, numerous mounds and ridges of gravel, called at one place "Hills of Fife," at another, "Kippet Hills." The generic name of these mounds and ridges in this part of Scotland, is Celtic word "Druim" or "Drum." They are composed sometimes of sand, more frequently of gravel. The gravel consists of fragments of rock, generally from westward. They are always well rounded, by the friction they have undergone. They sometimes reach a size of 2 feet in diameter. The pebbles are chiefly gneiss.

On top of some of the knolls and ridges there are large boulders. There is one, near Menie, being a coarse crystalline rock, with a greenish tint, 8 × 5 feet. Another boulder of greenstone lies near it. Very frequently a stratum of red clay lies over the gravel ridges, encircling the base of boulders, indicating that after the gravelly ridges had been formed, and the boulders deposited, muddy sediment had been deposited in deep water. (Jamieson, "Geol. Soc. Journ.")

The following additional information sent by Mr James Mitchell, Boddam:—

No. 1 boulder, in a ravine at Bullers of Buchan, granite, 14 × 8 × 5 feet. About 15 feet above sea.

No. 2 boulder, on confines of Cruden and Peterhead. Granite, 18 × 12 × $5\frac{1}{2}$ feet (above ground), 290 feet above sea.

No. 3, half a mile to E. of No. 2, a granite boulder, 13 × 9 × 5 feet, at a height of 260 feet above sea.

Along the south side of Peterhead Bay, and as far as Buchan Ness, the shore is strewed with blocks of granite, gneiss, trap, and sandstone; many of them belonging to rocks not found nearer than 20 or 30 miles.

A belt of gravel and calcareous sand forms a semicircular arc, with a radius of about 3 miles from the coast, passing through Crudens and Slains. The most conspicuous hillock in the line is a narrow Kaim in Slains parish, called the *Kipet Hill*,—the abode of fairies and elf bulls.

Compact groups of boulders form lines generally in a N.E. and S.W. direction. But a large number have been sown broadcast.

Culsalmond (Garioch).—Boulder of blue gneiss, $6\frac{1}{2} \times 2\frac{1}{2}$ feet, known as the Newton Stone, containing Ogham and other very antique inscriptions. (Professor Nicol in letter to Convener.)

Ellon.—At junction of Ythan and Ebrie, sienitic greenstone boulder, $22 \times 9\frac{1}{2} \times 8\frac{1}{2}$ feet, resting on gneiss. Near same place, another still larger. All these boulders have come from W. or W.N.W. (Jamieson, in letter to Convener.)

Glass (5 or 6 miles west of Huntly).—Five blocks called "*Clachan Duibh*" (Black Stones), on Tod Hill. Girth of each about 50 feet, and height from 10 to 12 feet. Being of same rock as hill, not certain whether brought from a distance. Other boulders on hill apparently different from adjoining rocks. Height above sea about 1000 feet. (Reporter—J. F. Macdonald, parochial schoolmaster.)

Kemnay.—Boulder, $38 \times 30 \times 10\frac{1}{2}$ feet, about 300 feet above sea; longer axis, E. and W. Boulder, $35 \times 30 \times 10$ feet, about 325 feet above sea; longer axis N. and S. Boulder, $25 \times 23 \times 8$ feet, about 325 feet above sea; longer axis, E. and W. Boulder, $28 \times 25 \times 8$ feet, about 325 feet above sea; longer axis N. and S. Boulder, $30 \times 28 \times 10$ feet, about 360 feet above sea; longer axis, N. and S. Boulder, $33 \times 27 \times 6$ feet, about 360 feet above sea; longer axis, N. and S. Boulder, $21 \times 20 \times 3$ feet. All these boulders are blue gneiss, whilst rocks adjoining are a coarse grey granite. On Quarry Hill, situated to north, 600 feet above sea, the rocks show striations indicating movement from west. Kaimes in valley parallel with valley running N.E. and S.W. for two or three miles. Legend, about devil throwing boulders at church from Bennachie Hill, situated to N.W. about eight miles. See ballad in Report. (Reporter—Rev. George Peter, M.A., parish minister.)

Logie Coldstone.—This parish thirty miles N.W. of Aberdeen. Surrounded at N.W. by amphitheatre of hills, of which Morven 2850 feet high. It contains numerous mounds of gravel and sand, in layers, showing action of water. They have the form of "kaims." Though there are no boulders, there are pebbles up to a *cwt.* or more, imbedded in water-worn gravel and fine sand. The pebbles are of same rock as adjoining hills—gneiss, granite, and hornblende. Two singularly shaped mounds, one 60 feet high, the other composed entirely of sand. They resemble the terminal moraines seen in the Grindelwald and other parts of Switzerland. Some years ago a number of boulders (from 3 to 6 tons in weight) were destroyed at a place situated to the north of this. They were of a soft, bluish granite, differing from any granite rock within a distance of nine or ten miles. One of these boulders might weigh 20 tons. This place had all the appearance of an ancient lake. The boulders may have been brought to it by same agency as that now seen on the Märjelin See, near Aletsch Glacier. (Reporter—J. G. Michie, school-house, Coldstone, Tarland.)

New Deer.—A great number of boulders, from 1 cwt. to several tons, lie in a sort of line for more than a mile S.E. from farm of Green of Savoch, as far, at least, as the hill of Coldwells and Toddlehills, in parish of Ellon. Elsewhere they are mostly on surface. Locally called "Blue Heathens." On Whitestone Hill, Ellon, and on Dudwick Hill, chalk flints are exceedingly abundant. (Reporter—James Moir, Savoch, by Ellon.)

In this parish formerly there was a rocking-stone, called "The Muckle Stone of Auchmaliddie." On the Hill of Culsh, formerly a Druidical circle. About seventy years ago the stones were carried away to aid in building a manse. Farm where situated still called, "The Standing Stones of Culsh." (Rev. J. Pratt's Account of Buchan, 1858.)

Towie.—Stone of unhewn granite, standing about 7 feet above ground, on north side of river Don, near bridge. Supposed to be Druidical ("New Statistical Account" of parish).

ARGYLL.

Appin.—Granite boulder $20 \times 18 \times 11$ feet, about 290 tons. Differs from adjoining rocks. Longer axis N.E. Striated. Apparently has come from head of valley, which to N. or N.E. There is also a line of boulders;—rocks striated in direction of glen. (Reporters—James M'Dougall and Sir James Alexander, who sends a sketch.)

Ardentinny.—1. Boulder, called "Pulag" * (Big Round Stone), about 30 tons. In critical position on edge of cliff. 2. Boulder, called "Giant's Putting Stone," pear-shaped, and rests on small end. 3. Boulder, called "Clachan Udalain" (nicely-balanced stone), larger. (Reporter—Rev. Robert Craig.)

Duncansburgh (near Kilmallie).—Granite boulder, $7 \times 5\frac{1}{2} \times 5$ feet, called "Trysting Stone." Tradition. There are larger boulders nearer Ben Nevis. (Reporter—Patrick Gordon, min., *Q. S.* Duncansburgh, Fort-William.)

Dunoon (Kirn).—Trap boulder, $21 \times 14 \times 7$ feet, about 164 tons. The adjoining rocks are mica schist and clay slate; striated. Photograph sent. (Reporter—Rev. James Hay, minister of Kirn.)

Glencoe.—Trap boulder, about 90 feet in girth and about 10 feet high. It is nearly round, and lies on an extensive flat, so that very conspicuous from a distance. (Reporter—Captain White, R.E.)

Inishail (North of Inverary).—Granite boulder about 8 feet above ground, called "Rob Roy's Putting Stone," about 1 mile from Taynuilt Inn on Oban road, about 60 feet above sea. A mountain of same rock about 1 mile distant. Longer axis, E. and W. Due west from above about $1\frac{1}{2}$ miles, another boulder on a ridge on side of Loch Etive, in Muckairn parish. Several large boulders on road between Dalmally and Tyndrum; also on road between Tyndrum and Black Mount, about 4 or 5 miles from Tyndrum. A fine boulder on Corryghoil farm (Mr Campbell) between Inishail and Dalmally. (Re-

* Another translator states that "*Pulag*" in Gaelic means a "*dome.*"

porter—Rev. Robert M. Macfarlane, minister of Glenorchy and Inishail).

Inverchaolain.—Gneiss boulder, $10\frac{1}{2} \times 7 \times 5\frac{1}{2}$ feet, about 30 tons. Called "Craig nan Cailleach" (Old Wife's Rock). Differs from rocks of district. At head of Loch Striven, many boulders, same as rocks. (Reporter—John R. Thompson, schoolmaster, Inellan.)

Iona (Island).—Granite boulder, $24 \times 18 \times 6$ feet, 190 tons. Longer axis N.W. There are a great many others, chiefly on E.S.E. side of island, opposite to Ross of Mull, from which boulder supposed to have come. On other hand, Duke of Argyll is said to consider that the granite of the boulder is not the same variety as that of Ross. There are several boulders oddly placed near top of highest hill on N.W. side. (Reporter—Allan M'Donald, parish schoolmaster.)

Kilbrandon (Easdale by Oban).—On Lord Breadalbane's estate, grey granite boulders from 21 to 28 feet in girth, and standing from 3 to 4 feet above ground. Longer axis generally N.W. Ruts or grooves on tops and sides of some, bearing N.W. These boulders sometimes single, sometimes in groups, sometimes piled on one another. Occur at all levels from shore up to hill tops. No granite *in situ* nearer than Mull, which is 15 or 20 miles distant to N.W. (magn.) (Reporter—Alexander M'Millan, schoolmaster, Kilbrandon.)

Kilmallie.—Boulder, $12 \times 10 \times 10$ feet, about 100 tons. There is another, said to be larger, in the distant moors; also quartz boulder, about 9 feet square, supposed to have come from Glenfinnan, about 15 miles to N.W. by W. (Reporters—Rev. Arch. Clerk, and C. Livingston, schoolmaster.)

Kilmore and Kilbride (near Oban).—Granite boulder, 12 feet long; diameter of shortest axis, 5 feet; longer axis, E. and W. A few feet above sea mark. Adjacent rocks conglomerate. Another stone, about 200 yards distant, called "Dog Stone," of which photograph sent. It is a conglomorate. (Reporter—C. M'Dougall, Dunollie, Oban).

Lismore (Island of).—Boulders of granite, red and grey, lie on the limestone rocks of the island. An old sea terrace described, as encircling the island, on one part of which a cave, from the

crevices of which shells picked by Reporter (Alexander Carmichael, Esq., of South Uist, Lochmaddy, who refers also to the Rev. Mr Macgrigor, minister of Lismore).

Saddell (Kintyre).—Several small granite boulders, though there are no granite rocks in Kintyre. A good many whinstone standing stones. (Reporter—Rev. John G. Levach, Manse of Saddell.)

South of Campbelton, many granite boulders, like Arran granite, one near Macharioch, 4 × 5 × 2 feet. (Reporter—Professor Nicol, Aberdeen.)

At Southend, a boulder of coarse grey granite, about 18 feet in circumference, and weighing more than 3 tons, now broken up.

Another granite boulder, about 12 feet in circumference.

Two boulders of sienite, each 2 or 3 tons, about 200 feet above sea.

No granite rocks in neighbourhood. Rocks chiefly limestone and red sandstone. (Reporter—D. Montgommerie, Southend parish school.)

AYR.

Coylton.—Granite boulder, $11 \times 7\frac{1}{2} \times 5$ feet, about 30 tons. Longer axis N. and S. There are four more boulders, about 4, 8, and 12 tons. They form a line running N. and S. Legend, that King Coil dined on large boulder. (Reporter—Rev. James Glasgow.)

Dailly.—Granite boulder about 36 tons on Killochan Estate, called "The Baron's Stone." About 100 feet above sea. Lies on Silurian rocks. Apparently derived from granite hills situated S.S.E., near Loch Doon, about 13 miles distant. Boulder proposed to be blown up by tenant of farm. But old inhabitants interposed, and an inscription put on it by proprietor, Sir John Cathcart, in these terms, "The Baron's Stone of Killochan." Granite boulders of various sizes, on hill slopes, south of river Girvan. One on Maxwelton farm 800 feet above sea, contains 240 cubic feet. Another, 16 feet long, on top of Barony Hill above Lannielane, mostly buried under turf. Level mark on it by Ord. surveyors of 1047 feet above sea.

Doone Loch.—Two miles south of,—a granite boulder, about 25 × 20 × 12 feet, called "Kirk Stane." (Seen by Convener.)

Girvan.—Thousands of granite boulders for miles along shore near Turnberry Point, and some whinstones. Rocks *in situ* sandstone. (Reporter—Superintendent of Turnberry Lighthouse works.)

Along coast 4 miles south, in a ravine, two boulders of altered Greywacke. Largest, 17 × 13 feet, and weighs 180 tons. Other weighs about 100 tons. Have probably come from hills to S. or S.E.

Maybole.—Granite boulder, flat and oblong, on slope of hill above river Doon, on Auchindrane, at height of 230 feet, known as Wallace's Stone, from tradition, that a rude cross carved on it represents the sword of that hero. (These cases from Dailly, Girvan, and Maybole, communicated by Professor Geikie).

Banffshire.

Banff.—In district between Banff and Peterhead, beds of glacial clay, of a dark blue colour, very similar to beds in Caithness, and probably drifted from Caithness. Near Peterhead, many boulders of granite and trap. One of these, 4½ × 2½ × 1 feet, a fine grained tough trap, of a greenish colour, not known *in situ* in Aberdeenshire, but occurs in Caithness. (Jamieson, "Geol. Soc. Jour.," xxii. p. 272.)

Boyndie.—Hypersthene boulders along shore, and found for some miles running S.W. Supposed to have come from rock to S.E., called "Boyndie Heathens." (Reporter—James Hunter, Academy, Banff.)

Fordyce.—A line of boulders can be traced running through parishes of Ordiquhill, Marnock, Grange, Rothiemay, and Cairney, in a direction S. and N. The boulders are a blue whinstone. In Ordiquhill parish, boulders, so close as to almost touch. They are called "Heathens." 500 feet above sea. (Reporter—Parish minister.)

Caithness.

Dunnet.—Conglomerate boulder of small size, apparently from "Maiden Pap" Hill, thirty miles to south. Several large

boulders in parishes of Olrich and Cannesby. (Reporter—Robt. Campbell, parish schoolmaster.)

Thurso.—Near Castletown, large granite boulder, which supposed to have come from Sutherland.* Between Weydale and Stonegun, several large conglomerate boulders.

Wick.—Three large boulders, differing from adjoining rocks, weighing from 20 to 60 tons. One is a conglomerate, apparently from mountains twenty miles to south.† (Reporters—John Cleghorn and J. Anderson.)

Granite boulder, 12 feet long, in drift, striated. Fragments of lias, oolite, and chalk flints, in same drift. Striations of rocks and boulders in Caithness indicate a general movement from N.W., *i.e.*, from sea.

Dumfries.

Kirkconnell.—Granite boulder, about 9 feet diameter, 20 to 30 tons; 700 feet above sea, called "Deil's Stone." Differs from adjoining rocks. Granite rocks in Spango Water, about three miles to north. (Reporter—R. L. Jack (Geolog. Survey).)

Tynron.—Three whinstone boulders, each weighing from 20 to 30 tons; also several conglomerate boulders. All have apparently come from N.W. (Reporter—James Shaw, schoolmaster, Tynron, Thornhill.)

Wamphray.—Large whinstone boulder. King Charles II. halted with his army and breakfasted here. (Reporter—Parish minister.)

Edinburgh.

Arthur Seat.—On west side of, boulders of limestone, supposed to have come from west. Rocks at height of 400 feet above sea, smoothed and striated in direction N.W.

Between Arthur Seat and Musselburgh, boulders smoothed and striated. Striæ run from N.W. and W.N.W. (Roy. Soc. of Ed. Proceedings, vol. ii. p. 96.)

* Rev. Mr Joass, of Golspie, states that granite occurs at a less remote locality.

† Rev. Mr Joass states that conglomerate rock occurs to the westward at a less distance.

Pentland Hills.—1. Mica-slate boulder of 8 or 10 tons. Supposed by Mr Maclaren to have come from Grampians, 50 miles to N., or from Cantyre, 80 miles to W., about 1400 feet above sea. 2. Greenstone boulder, 12 or 14 tons. Nearest greenstone rock *in situ*, 500 or 600 feet lower in level to N.W. 3. Sandstone boulder, about 8 tons, differing from adjacent rocks. (The above mentioned in Maclaren's "Fife and Lothians," p. 300.) 4. Greenstone boulder, about 10 tons, near Dreghorn. (Fleming's "Lithology of Edinburgh," p. 82.)

West Calder.—Whinstone boulder, 8 × 7 × 7 feet, about 28 tons. Adjoining rocks are sandstone. (Reporter—S. B. Landells, teacher.)

Elgin.

Dallas.—Numbers of small granite boulders found here, which supposed to have come from Ross-shire.

Duffus.—On Roseile Estate, conglomerate boulder called, "Hare, or Witch's Stone," 21 × 14 × 4 feet, longer axis N.W. Farm named "Keam," from being situated on a sandy ridge.

Elgin.—1. Conglomerate boulder on Bogton farm, 4 miles south of Elgin, 15 × 10 × 8 feet, about 80 tons. Longer axis is E.N.E., called "Carlin's Stone." Also a smaller one, called the "Young Carlin," to N.W. about half a mile. 2. Conglomerate boulder, 4 × 4 × 3 feet, about 3 tons. 3. Gneiss boulder, 13 × 8 × 6 feet, about 46 tons, called "Chapel Stone." Situated west of Pluscardine Chapel. 4. Sienite boulder, 12 × 8 × 3 feet, about 13 tons. 5. Sienite boulder, 8 × 6 × 2 feet, about 7 tons. The rocks *in situ* are all Old Red Sandstone. On Carden Hill, rocks smoothed and striated;—the direction of striæ N.W. (Reporter—John Martin, South Guildry Street, Elgin.)

Forres.—Conglomerate boulder, 9½ × 8 × 8 feet, about 44 tons, called "Doupping Stone." (Reporter—John Martin.)

Llanbryde, St Andrews.—Gneiss boulder, 15 × 9 × 7 feet, about 70 tons, in bed of old Spynie Loch, called "Grey Stone;" longer axis is N.N.E. and S.S.W. (Reporter—John Martin.)

New Spynie.—Four conglomerate boulders, lying on Old Red Sandstone rocks. (Reporter—John Martin.)

Rothes.—Six hornblende boulders, lying on gneiss rocks; dimensions and positions given. (Reporter—John Martin.)

FIFE.

Balmerino.—Mica schist (?) boulder, $12 \times 9 \times 8$ feet; destroyed some time ago. (Reporter—James Powrie, Esq., Reswallie, Forfar.)

Crail.—Granite boulder, $10 \times 8 \times 6$ feet, called "Blue Stone o' Balcomie," close to sea margin at East Neuk. Also trap boulder, $12 \times 8 \times 7\frac{1}{2}$ feet. (Reporter—Captain White, R.E.)

Dunfermline.—Whinstone boulder, $17 \times 15 \times 6$ feet, about 114 tons, called "Witch Stone." Legend. (Reporter—Robert Bell, Pitconochie.)

Leslie.—Kaim of sand and gravel near village, 100 to 300 feet wide, and 20 feet high, cut through by a brook. (Reporter—John Sang, C.E., Kirkcaldy.)

Newburgh.—On shore, near Flisk point, boulder of sienitic gneiss, about 15 tons. Legend is, that a giant who lived in Perthshire hills flung it at Flisk church. (Dr Fleming, "Lithology of Edinburgh," p. 83.)

West Lomond.—Hill about 1450 feet above sea, boulder of red sandstone and porphyry lying on carboniferous limestone. (John Sang, C.E., Kirkcaldy.)

FORFAR.

Airlie.—A remarkable kaim running two miles eastward from Airlie Castle. (Reporter—Daniel Taylor, schoolmaster.)

Barry.—Granite, sienite, and gneiss boulders and pebbles, on shore, and also on raised beaches, 11 and 45 feet respectively above sea level. (Reporter—James Proctor.)

Benholm.—Huge granite boulder, called "Stone of Benholm," now destroyed. Boulders on sea shore, of granite and gneiss, many of which are supposed to have come out of the conglomerate rocks, which occur here *in situ*. One boulder $18 \times 12 \times 3$ feet, another $12 \times 6 \times 4$ feet. "Stone of Benholm," stood on apex of a Trap knoll. The Trap knoll presents a surface of rock, which has apparently been ground down and smoothed by some agent passing over it from west; the exact line of move-

ment seems 10° to 20° south of west (magn.) In this Trap knoll there are agate pebbles, which have been mostly all flattened on west side, and been left steep and rough on east sides. Small hills which range in a direction north and south are scalloped, as if some powerful agent passing over them from westward had scooped out the softer parts. Hills ranging east and west, form a ridge with a tolerably level surface. Gourdon Hill and Craig Davie show marks of great abrasion. (Reporter—Rev. Mr Smart Myers, parish minister.)

Carmyllie.—Granite or gneiss boulder, from 7 to 10 tons. Differs from rocks near it. It lies on a height. Called "The Cold Stone of the Crofts." Supposed to have come from hills thirty miles to north. (Reporter—Rev. George Anderson.)

Cortachy.—Whinstone (?) boulder, $13 \times 10 \times 8$ feet, about 78 tons Longer axis E. and W. Supposed to have come from a trap dyke situated to N.W. Legend, that thrown from N.W (Reporter—Rev. Geo. Gordon Milne.)

Mr Powrie of Reswallie reports a mica schist boulder as situated in South Esk river, about 60 or 80 yards below bridge, and within Earl of Airlie's park. Parent rock supposed to be 2 or 3 miles to N.W. This boulder probably same as that mentioned by Rev. Mr Milne.

Farnell.—Boulder $9\frac{1}{2} \times 7\frac{1}{2} \times 2\frac{1}{4}$ feet, about 12 tons. Supposed to have come from N.W. about thirty miles. (Reporter—Rev. A. O. Hood, parish minister.)

Inverarity.—Two grey granite boulders, from 2 to 5 tons each; destroyed some time ago. (Reporter—Rev. Patrick Stevenson.)

Kirkden.—Kaims, 440 paces long, running E. and W.; slope on each side from 22 to 30 paces; composed of gravel and sand. (Reporter—Rev. James Anderson.)

Kirriemuir.—A number of granite boulders in centre of parish, both grey and red. They lie chiefly between Stronehill and Craigleahill. Supposed to have come from Aberdeenshire.

Two kaims on Airlie Estate, one 100 yards long and 30 feet high, N.W. and S.E. on Upper Clintlaw Farm; other on Mid Scithie Farm, about 200 yards long and 30 feet high. At south base of Criechhill, a group of kaims, apparently

caused by confluence of great streams from N.E. and N.W. glens.

Old Red Sandstone rocks in S. of parish. Igneous rock towards N. at Craigieloch.

Slate rocks in Lintrathan and Kingoldrum. (Reporter—David Lindsay, Lintrathan, by Kirriemuir.)

Liff.—1. Mica schist boulder, 8 × 6 × 4 feet, called "Paddock Stone." Legend. Longer axis, N. and S. One report bears that it is whinstone, and may have come from Pitroddie Quarry, fourteen miles west. 2. Two boulders of mica schist, each 8 or 10 tons, called "Gows of Gowrie," noticed by Thomas the Rhymer. 3. A Druidical circle of nine large stones—three mica schist, one granite, five whinstone. Central stone, longer axis N. and S. (Reporters —James Powrie, Esq., Reswallie, Forfar; P. Anthony Anton, St Regulus Cottage, St Andrews.)

Menmuir.—1. Granite boulder, 14 × 9 × 4 feet, about 36 tons. Longer axis N. and W. Striated. Called the "Witch Stone." 2. Granite boulder, 13 × 9 × 4 feet, about 34 tons. There are many others smaller. (Reporter—Rev. Mark Anderson, Menmuir, Brechin.)

Montrose.—On Garvock and other hills, striæ on rocks point W. by N., *i.e.*, obliquely across the hills, which range W.S.W. and E.N.E.

On Sunnyside Hill, pieces of red shale found, derived from rocks *in situ* many miles to N.W. at a locality 100 feet lowest level.

Large blocks of gneiss, several tons in weight, occur, which must have come from Grampians, many miles farther to west. (James Howden, "Edin. Geol. Soc. Trans." vol. i. p. 140.)

Rescobie.—Mica slate boulder, 13 × 7 × 7 feet, near top of Pitscandly Hill, lying on drift. Rocks *in situ* Old Red Sandstone. Sir Charles Lyell says it came from Creigh Hill, about seventeen miles to W.N.W. Longer axis N. by E. 550 feet above sea. Valley of Strathmore lies between boulder and parent rock, and there are several hills also between boulder and parent rock, higher than boulder. Many smaller boulders of old rocks on same hill. (Reporter—James Powrie, Esq., Reswallie, Forfar).

St Vigeans.—Gneiss boulder, now destroyed. Supposed to have come from mountains situated to N.W. If so, it had to cross valleys and ridges of hills. Kaims in parish full of granite and gneiss boulders. (Reporter—Rev. William Duke, minister.)

Hebrides.

Barvas.—On Estate of Sir James Matheson, a monolith, called *Clach an Trendach*, or "Gathering Stone." Height above ground, 18 feet 9 inches, and girth 16 feet. (Reporter—Rev. James Strachan.)

Harris.—A large boulder on a tidal island, broken into two fragments, 100 feet apart. (Reporter—Alex. Carmichael.)

North Uist.—On a small island called Cāneum, north of Lochmaddy Bay, there are two boulders of Laurentian gneiss, which, though 100 feet apart, are evidently the two fragments of one block. The rocks *in situ* are also gneiss; but there is no hill or cliff near, from which the block could have fallen or come. One boulder weighs about 15, the other about 50 tons. They are both on the sea-beach, with a ridge or isthmus of rock between them. The boulders have each a side—in the one concave, and in the other convex—which face one another, and correspond exactly in shape and size. The edges of these two sides (viz., the convex and concave) are sharp, whereas the other sides in both boulders are rounded, suggesting that the original block had undergone much weathering or other wearing action before being fractured. The larger boulder rests fantastically and insecurely on two smaller blocks. Reporter thinks the boulder brought by ice, and that it fell from a height, and was split by the fall.

In Long Island the hills even to the summits are covered with blocks and boulders. As a rule the edges of these are sharp, whereas the native rock, whether low down or high up, is glaciated, grooved, and striated to a very remarkable degree. The best places to see these marks are where drift, covering them, has been recently removed. They are obliterated in the rocks, which have been much weathered. (Reporter—Alex. Carmichael, Esq., South Uist, by Lochmaddy.)

The Lewis.—(Q. S. Parish of Bernera. On farm of Rhisgarry, be

longing to Lord Dunmore.) Gneiss boulder, $8\frac{1}{2} \times 7 \times 3$ feet. Longer axis N. and S. 30 feet above sea. Striated N. and S. Striæ from 2 to 4 feet long. Same rock as those *in situ*. Called "Craig nan Ramh." (Reporter—Rev. Hugh Macdonald, Manse, Bernera.)

The Lewis (Stornoway, Tolsta).—A rocking stone of gneiss $12 \times 5 \times 4\frac{1}{2}$ feet. Longer axis N.W. and S.E. About 200 feet above sea. Rocks *in situ* also gneiss. There are boulders of trap, apparently brought from eastward, where there are trap dykes. At a corner of a rocky hill near Tolsta, there are huge pieces of rock lying, suggesting idea of having been broken off by an iceberg. On Park Farm, beside a loch, there is a solitary boulder. Near Stornoway Tile Works, a boulder of Cambrian rock, supposed to have come from mainland to eastward. (Reporter—Mr Peter Liddell, Gregs, by Stornoway.)

Stornoway.—Several boulders occur near Garabast, of a rock similar to that which exists at Gairloch, on mainland to east (about 35 miles across the sea). There is also a large standing stone at Paible. (Reporter—Henry Caunter, Esq., Stornoway.)

In Forest of Harris, and beween Fincastle and Glen Ulledale, there are many evidences of (supposed) ice action, viz., rocks smoothed and striated, and boulders lying in lines. (Reporter—Capt. Thomas, R.N.)

Report by Mr Campbell of Islay.

The well-known author of "Frost and Fire," who has studied the subject of the transport of boulders, not only in Scotland, but in many foreign lands on both sides of the Atlantic, has sent to the Committee a report, from which the following extracts are made:—

"I find in Scotland, upon ridges which separate rivers, marks of glaciation upon a large scale. These enable me to say, with tolerable certainty, that the ice which grooved rocks in the Outer Hebrides, at low levels, near sounds, moved from the ocean in the direction which tides now follow in the straits beside which the striæ are found.

"The conclusion at which I have arrived, by the examination of all these phenomena, boulders included, is, that a system of glaciations prevailed in Scotland, which can be ex-

plained by the system now existing in Greenland. There, a vast system of Continental ice, as great in area as all India, radiates seawards, and launches icebergs, which move about in tides and currents. This system certainly existed in Scotland previous to the smaller system.

"Following any glen in Scotland, say Glenfyne, the smaller system of glaciation follows the course of the river (as in Switzerland), and the course of the tides in the sea loch (as glaciers do in Greenland); and, furthermore, often overruns low watersheds, and runs out to sea in some direct line. The striæ which mark the run of ice from the head of Glenfyne to Lochgilphead, run over a col and down Loch Killisport. They run past Tarbert, down both sides of Ceantyre and Arran, and out to sea. At Ormsary, by the roadside, and on the sea-beach, is a train of large boulders to which the usual legends are attached. One was thrown from Knapdale at a giant who was eating a cow on the other side of the loch. One of these boulders close to Ormsary House, at a small roadside cottage, is the biggest I have seen in Scotland. I did not try to ascertain whence it came. I think it was pushed a short distance only. But the striæ and trains of blocks show that it moved from N.E. to S.W. along the general line of hollows in the Western Highlands.

"On the outer islands in Scotland are marks equivalent to those so conspicuous on shore. In the Long Island, from Barra Head to the Butt of Lewis, the whole country glaciated, and the boulders everywhere perched upon the hills. Where surface newly exposed, the striations and smooth polishing so perfect and fresh, that marks can be copied as *brasses* are copied in churches by antiquaries. I showed to you samples taken last year in Barra and Uist. I have a large series taken wherever I have wandered. These enable me to say, with tolerable certainty, that the ice which grooved rocks in Outer Hebrides at low levels, near sounds, moved from the ocean in the direction which tides now follow in the straits, beside which the striæ are found. For example, the grooves upon the flat at Iochdar, at the north end of South Uist, aim directly at the Cuchullin Hills in Skye. At the Mull of Ceantyre, at a

great height above the sea, grooves aim at Rhinns of Islay parallel to the run of the tides. And so it is at a great many other places all round the coast."

In a letter from the same gentleman to Mr Carmichael, of South Uist, dated 29th March 1872, the following passages occur :—

"Glacial striæ occur upon fixed rocks in Tiree, Minglay, Barra, South and North Uist. They correspond with a direction from the N.W., or thereabouts.

"The striæ abound, and are especially fresh in the low levels, and opposite to hollows in hills, which would be under water, and traversed by tides, if those levels were now to sink a few hundred feet. The hills, so far as I have examined them, are ice-worn to the very top. Transported blocks are scattered all over these islands. In some places regular boulder-clay is left in patches. Under the clay, the rocks are smooth as polished marble. The boulders, so far as I have been able to ascertain, are of the same rock as the rock of the islands named. Boulders in Tiree, for example, may have come from Uist or Barra. They are perched upon the highest hill-top in Tiree.

"I was unable to find any sample of the rocks of Skye in Uist or in Tiree."

Inverness.

Kilmallie.—Boulder, fully 2000 feet above sea, on summit of a hill, 12 × 10 feet. Another still larger among the mountains between Loch Shiel and Loch Arkaig. Also boulder drifts and moraines in numbers. (Reporter—Rev. Archibald Clerk, Kilmallie Manse.)

Kilmallie (near Ardgour).—Quartz and mica boulders, nearly round, and remarkable on bare hill side. Different from adjacent rocks. 110 feet above sea. Same kind does not occur nearer than Glenfinnan, situated fifteen miles to N.W. by W. (Reporter—C. Livingston, parochial schoolmaster.)

Kilmonivaig (Glengarry, N.W. of Fort William), Estate of Edward Ellice, M.P.—Boulder on Monerrigie Farm, near Lochgarry, about 16½ feet long at base, and 23 feet at top, and about 9 feet high. Round at top. Quartzite rock. No rock *in situ* near.

Longer axis N. and S. Several boulders on Leek Farm, near Loch Lundie, considerably larger. Some of boulders examined by Mr Jolly, school inspector, Inverness, and found by him to be striated. On Faicheam Ard Farm boulders very peculiar, being entirely different from all rocks in neighbourhood. Have been objects of curiosity to many geologists. The boulders generally arranged in groups, except at Faicheam Ard, where piled on one another. They rest on gravel. At Leek, near Iron Suspension Bridge, rocks *in situ* well striated.

There are "kaims" in another part of parish. At mouth of Glengarry a delta of fine gravel. In Lochaber also, along banks of Spean and Lochy. (Reporter—Parochial Schoolmaster.)

Kiltarlity (on Lord Lovat's lands).—A group of boulders called whinstones. Rock of same kind "a little southwards." Dimensions of two largest are (1.) 15 feet long, 9 feet high, 10 feet broad; (2.) 8 feet long, 6½ feet high, 13 feet broad. Longer axis of both E. & W. Angular in shape. Several natural ruts on both 4 or 5 feet long, running N.W. About 300 feet above sea. (Schoolmaster's schedule, but omitted to be signed.)

Kingairloch (near Fort William).—Boulder, 5 × 5 × 4 feet, about 5 tons; 8 feet above sea. Different from adjacent rocks. (Reporter—D. Cameron, teacher.)

Kingussie.—Boulder of a slaty rock, 15½ × 12 × 9, about 120 tons. Longer axis, E. & W. Called "Fingal's Putting Stone." About 900 feet above sea. Several other large boulders near Laggan Free Church. (Reporter—Cluny M'Pherson, Cluny Castle, Kingussie.)

Lochaber.—Near summit of Craig Dhu, between Glens Spean and Roy, a black sienite boulder, 14 × 8 × 4 feet. On same hill lower down, boulders of red granite and felspar. (Observed by Professor Nicol and Mr Jamieson of Ellon. Mr Jamieson states that parent rock is in Glen Spean, to S.E. of Craig Dhu, and at a level far below boulders.) ("Lond. Geol. Soc. Journal," Aug. 1862 and Aug. 1863.)

On second Glenroy shelf, near the "Gap," a boulder of sienite, 8 × 7 × 4 feet. (Reporter—Professor Nicol.)

Morvern (near Fort William).—Grey granite boulder, called "Clach na'm Buachaillean." Length—North side, 17 yards; south side, 7½ yards; 17 yards "round about;" 13 yards "round top from ground to ground;" 11½ yards "across middle from ground to ground." A large boulder to east of above on a hill about 2640 yards distant, and "peculiarly laid upon other smaller stones." (Schoolmaster's schedule, but omitted to be signed.)

Kincardine.

Banchory.—On property of John Michell, Esq. of Glessel, not far from Glessel Railway Station, a boulder called the "Bishop's Stone;" circumference 44 feet, height above ground 8 feet, estimated to weigh 70 tons; bluish granite, differing from adjoining granite rocks. An ancient stone circle of boulders about 200 yards distant. (Reporter—Sir James Burnett of Crathes.)

The hill of Farre, situated two miles to north, forms an elongated range, running E. and W. Rocks on it glaciated, the striæ running about E. and W., *i.e.*, nearly coincident with valley of Dee. (Reporter—Thos. F. Jameson, Ellon.)

Fettercairn.—No boulder now left in parish, of any size. Long banks of gravel and sand occur, running parallel to one another. (Reporter—A. C. Cameron, parish schoolmaster.)

Maryculter.—Boulder, 5½ × 6 × 6 feet, about 14 tons. Longer axis N. and S. Rock of boulder considered same as rock situated to eastward. (Reporter—David Durward.)

Kirkcudbright.

Galloway.—A great accumulation of blocks at head of Loch Valley at Loch Narroch. Among these are blocks of the peculiar graphic granite of Loch Enoch to the north, so that these blocks must have been carried from Loch Enoch southwards into the basin of Loch Neldricken, on to the spur of Craignaw between it and Loch Valley, and still onwards right over Craiglee and its deep scooped lake basins into Glen Trool. Craiglee is remarkable for the number of perched blocks, some of immense size, scattered over its ridges and highest peaks.

The many boulders along its ridgy crest give the appearance of an old broken-toothed saw.

Throughout the whole region travelled blocks and boulders occur, even to the summit of the Merrick, the highest peak south of the Grampians (2764 feet). One set of perched blocks is interesting, viz., poised blocks, known as Rocking Stones. Such blocks are natural, and have been placed by no human hands. Their exquisite balance is the result of the weathering of the block and of the rock below, caused by wind and storm.

There are well-marked striated rock surfaces more than 1600 feet above the sea-level.

Various moraines described, as stretching across valleys like ramparts, and forming dams to existing lakes. (William Jolly in "Edin. Geol. Soc. Trans." i. 155.)

Kells.—On Craigenbay Farm, a grey whinstone boulder, about 10 feet high and 17 feet long, with girth of 54 feet; 800 feet above sea. Longer axis N. and S. (Reporter—Robert Wallace, Auchenbrack, Tynron.)

Kirkbean.—Grey Granite boulder, $16 \times 9\frac{1}{2} \times 7\frac{1}{2}$ feet, and girth about 38 feet, weighing about 80 tons. On sea shore at Arbigland. Longer axis, S.E. by E. Superficial groovings on top and S.W. front running N.N.W. Rests on freestone.

Criffel is about 3 miles to N.N.W. Granite rock there same as boulder. In all the glens, between sea shore and Criffel, numerous granite boulders generally in lines parallel with glens. Several kaims 40 to 50 feet high, run from $\frac{1}{4}$ to $\frac{1}{2}$ mile. (Reporter—Rev. James Fraser, Colvend Manse, by Dalbeattie).

Penninghame.—Granite boulders chiefly, supposed to have come from Minnigaff Hills, situated to N.E. Larger boulders on watersheds between Lochs Dee and Troul. (Reporters—Rev. William M'Lean, parish minister, and Rev. George Wilson, F.C. minister.)

Twynholm.—Granite boulder, supposed to have come from Galloway Hills, six or seven miles to westward. Several Druidical circles. (Reporter—Rev. John Milligan, Manse of Twynholm.)

LANARK.

Carluke.—Sandstone boulder, 20 × 14 × 14 feet, about 290 tons. Called "Samson's Sling Stone." Doubtful if an erratic. (Reporter—D. R. R.)

Carnwath.—Whinstone boulders in large heaps. Supposed to have come from "Yelpin Craigs," three or four miles to north. Legend about Michael Scott and witches. (Reporter—Rev. Mr M'Lean.)

NAIRN.

Auldearn.—A great many boulders in this parish, of old rocks, and lying chiefly on Old Red Sandstone rocks. Chiefly conglomerates, and apparently derived from same kind of rock, characterised by pebbles in it of angular quartz or hornstone, liver coloured. These boulders all lie on sides of hills facing N.W., and they have generally one of their sides smooth which fronts the west. (Reporter—James Rennie, schoolmaster.)

Ardclach.—At Raemore Burn, about 270 feet above sea, and 5 miles distant from sea, a conglomerate boulder with five sides, measuring altogether about 17 yards, and 3 yards above ground. Surrounded by hills of no great height; but lowest of these is to N.W. Fragments in conglomerate of quartz, hornstone, sienite, felspar, and other very hard rocks. The block is scarcely rounded at its edges and corners. (Reporter—Dr Gregor, Nairn.)

Cawdor.—On hill of Urquenay, the following boulders—1. At top of hill, about 690 feet above sea, conglomerate called "*Clach na Gillean,*" or "*Young man's stone,*" in girth about 54 feet, and height 10 feet. It rests on bare granite rock. 2. Half-way down hill, about 580 feet above sea, conglomerate called "*Clach na Cailleach,*" or "*Old wife's stone,*" in girth about 54 feet and height 15 feet. It seems to rest on drift gravel. 3. At foot of hill, and at east end of a kaim of gravel and sand, about 300 feet above sea, conglomerate called "*Clach an oglach,*" or "*Boy's stone,*" in girth about 69 feet, and average height about 9 feet.

Within policy woods of Cawdor Castle, on side of a burn

facing W.N.W., a conglomerate boulder about 250 feet above sea, in girth about 100 feet, and about 12 feet high.

The above four conglomerate boulders lie on granite rocks.

On Piper's Hill, where rocks *in situ* are Old Red Sandstone, a conglomerate boulder, on the side of a kaim facing N.W., weighing about 10 tons. Above sea about 300 feet.

No conglomerate rock of the same hard description in Nairnshire. On the granite rocks there lie boulders of sandstone, evidently transported from the north, where the Old Red Sandstone only exists, in the low country. (Reporters—W. Stables, Esq., commissioner; and his clerk, Mr John Grant, Cawdor Castle.)

Croy.—Conglomerate boulder, called "Tomreach," about 15 feet high, and girth of 27 yards. About 300 or 400 feet above sea. Sketch sent. (Reporter—Captain White, R.E.)

Orkney and Shetland.

Bressay (Shetland).—A number of boulders consisting of a coarse white sandstone at various heights, viz., from 40 to 360 feet above sea. They lie on east side of island, and are conjectured to have come from Norway. Largest boulder, $10 \times 7 \times 4$ feet. Longer axis, N.W. Distinct groovings N.E. and S.W. (true); some of them 3 inches deep. (Reporter—Schoolmaster?)

Eday (Orkney).—Conglomerate boulder, $12 \times 7 \times 1\frac{1}{2}$ feet, about 8 tons. Longer axis N.E. Situated near top of hill, about 250 feet above sea. Called "Giant Stone." Legend, as to it being thrown from island of Stronsay. No conglomerate in Eday, but there is in Stronsay. (Reporter—G. Miller, schoolmaster, Cross and Burness.)

Frith and Stennis (Orkney).—Pebbles of white freestone on the hills. No white freestone rock in district; all red sandstone. (Reporter—Robert Scarth.)

Housay Island (Shetland).—On a cliff, 200 feet above sea, there are loose blocks resting on rounded knolls and polished rock, all polished before the burthen they now bear was thrown upon them. Some of the stones hang on ridges on the rounded sides of the hill.

Lerwick (Shetland).—At Lunna, a large block, broken into two, called the "Stones of Stoffus," but uncertain whether erratics. (Reporters — James Irvine, teacher, and Robert Bell, proprietor.)

North Unst.—Here ice action plain. The serpentine rock has suffered severely. Ruts and striæ on it W.N.W. A hill 500 feet high, whole of upper part of which for 150 feet from top polished. Striated stones and blocks also plentiful. All over Unst the rocks show signs of abrasion, and in many places deposits of drift, inclosing stones of all sizes, some of which are rounded and striated.

In the *Island of Ueay*, large perched blocks, some many tons in weight, lie scattered about everywhere.

Thus then, at both ends, and in the middle of this group of islands, traces of glacial action have been found. (Peach, Brit. Assoc. Rep. 1864.)

Sanday (Orkney).—Gneiss boulder, 7 × 2½ × 6 feet, about 14 tons. Rocks of island are Old Red Sandstone. At Stromness, thirty miles to S.W., gneiss rocks occur *in situ*, also in Shetland Islands to north. Legend, that thrown from Shetland. (Reporter—G. Miller, schoolmaster, Cross and Burness.)

Sumburgh Head (Shetland).—Conglomerate boulder, lying over sandstone. (Reporter—William Lawrence, teacher.)

Walls (Orkney).—Lydian stone boulder, 9 × 7 × 6 feet, about 28 tons. Large quantities of granite boulders scattered over hills; valleys show glacier and iceberg agency. (Reporter—James Russell, teacher.)

Peebles.

Kirkurd.—Three boulders of gneiss or trap (?) differing from adjacent rocks. (Reporter—James Palmey, schoolmaster, Kirkurd, Dolphinton.)

Newlands.—Remarkable kaims. (Reporter—E. Blacklock, schoolmaster.)

Perth.

Aberfeldy (Tullypowrie village). 1. On north side of village, a considerable assemblage of schist boulders, the rocks *in situ* being clay slate. Most of boulders round in shape as if rolled.

One large boulder angular, 16 × 14 × 7 feet, named "Clach Chinean," or "Stone of Doom." These boulders all rest on heaps of drift, much resembling a moraine. On the opposite or south side of the valley there are similar masses of drift, containing, however, stratified beds of sand and gravel.

2. About 2 miles north of Tullypowrie village, near the hills, two very large boulders of mica slate occur, about 1500 feet above sea. They rest apparently on a heap of drift. They are both cubical in form, and with sharp angles, as if never exposed to friction. One of them measured, and found to be 71 feet in girth and 17 feet high. The hills are more than ¼ mile distant. They must have been brought by ice of some kind, and let down without violence; for a fall from any height would have probably caused such large masses to break in pieces. The adjoining hills form a range to N. and W., reaching fully 700 feet above the boulders. But to N.W. (magn.) of the boulders, and within a ¼ mile a passage occurs through the hills, the level of which is only about 200 feet above the boulders. They might have come through this passage, carrying the boulders and stranding them where they now lie. These boulders, called "Clach M'had," or "Stones of the Fox."

3. Above Pitnacree House, a boulder of schist resembling hypersthene, 15 × 11½ × 4 feet above ground. It is called "Clack odhar," or "Dun Stone." No hills are near it, and it differs from all rocks *in situ* near it. (Reporter—Mr M'Naughton, merchant, Tullypowrie).

Arngask.—Rocking stone of mica slate, in Glenfarg ("New Statistical Account," vol. x. p. 888).

Auchterarder.—Boulder, 10 × 6 × 2 feet, about 8 tons. Longer axis N.W. Called "Wallace's Putting Stone." (Reporter—Rev. Dr Nisbet, Edinburgh.)

Auchtergaven.—Granite boulder, 10 × 8 × 3 feet, about 8 tons; 260 feet above sea. Longer axis N. and S. Called the "Deil's Stone." Has numerous and distinct "cup" markings on its sides. Supposed to have come from mountains situated thirty miles to north. Has been mutilated by slices cut off it for building, &c. Several standing stones and Druidical circles in

this parish, composed of boulders. (Reporter—William Duff, schoolmaster.)

Bendochy.—Formerly a Druidical circle of nine large stones, now destroyed, but name still preserved of "Nine Stones." Long kaims of gravel or sand, which supposed may have caused river Tay to fall into sea at Montrose. (Reporter—Rev. Dr Barty.)

Callendar (Stirling).—Gneiss boulder on top of Bochastle Hill, called "Samson's Putting Stone," 14 × 9 × 9 ft., resting on conglomerate rock. Longer axis N.E. Sketch sent, showing unstable position. Has come from westward. (Reporter—J. B. Hamilton, Leny.)

Collace.—Large stones said to be here. Query,—are they erratics? (Reporter—Peter Norae, schoolhouse, Collace.)

Comrie.—Four boulders of whinstone, and one of granite, 13 × 9 × 7½ feet, weighing about 20 tons. Longer axis N. and S. (Reporter—Wm. F. Swan.)

Crieff.—1. Conglomerate boulder, 16 × 10 × 5½ feet, about 64 tons, "Witches' Stone." 2. Conglomerate boulder, 19 × 10 × 5 feet, about 70 tons. 3. Red granite boulder, 8½ × 4½ × 4 feet, called "Cradle Stone." (Reporter—Rev. Dr Nisbet, Edinburgh.)

At Abercairney, dark grey granite boulder, about 20 tons. (Reporter—C. Home Drummond Moray; and Rev. Thomas Hardy, parish minister.)

In Glen Turret, appearances of ancient moraines, described in letter by Mr Sang, C.E., Kirkcaldy.

Doune (near Kilbride).—Conglomerate boulder, about 900 tons. (Described in Estuary of Forth, by Mr Milne Home.)

Dron.—Whinstone rocking stone, 10 × 7 feet. Stands on bare rock ("New Statistical Account," vol. x. 364).

Errol.—Several boulders, differing from adjacent rocks. Said to be indicated on Ordnance Survey maps.

Fortingall.—Gneiss boulder, 24 × 16 × 13 feet, called "Clach an Salaine," from people who brought trees out of Black Wood of Rannoch, resting them on it. Height above sea 2500 feet. Rocks *in situ* clay slate. Longer axis N.W. (Reporter—Mr Fletcher Menzies.)

Fowlis.—Two dark grey granite boulders, $10 \times 7 \times 4$ feet, and $12 \times 6 \times 4$ feet. Supposed to have been used as places of worship or sepulture, in very ancient times. (Reporter—Rev. Thomas Hardy.)

Killiecrankie (Tennandry Parish).—Blue limestone boulder, $6 \times 5\frac{1}{2} \times 4$ feet. Supposed to have come from "*Ben y Gloe*," a hill to N.N.E., across valley 500 feet deep; plan of district sent. Granite boulder, also mentioned; has come from North. (Reporter—Rev. Patrick Grant, Tennandry Manse.)

Kilspindie.—Seven granite boulders, from 5 to 6 tons weight. Five form a belt or row having N.W. direction. All differ from adjacent rocks. (Reporter—James M'Kerracher, schoolmaster, by Errol.)

Kirkmichael.—Rocking stone, $7 \times 5 \times 2\frac{1}{2}$ feet, about 3 tons, whinstone. (?) Several tall stones near it, called "Clachan Sleuchdaidh" (Stones of Worship).—("New Statistical Account," vol. x. p. 737.)

Logie Almond.—Whinstone boulder, 8 or 10 feet square, about 48 tons, called "The Ker Stone," about 600 feet above sea, on north bank of River Almond, opposite to Glenalmond College. Probably, as there is a great peat moss near, the name has reference to the moss, "char" being the Gaelic for peat.

There is another boulder called "Cul na Cloich," or *Stone Nook.* A stream forms a nook or angle with the drain or ridge on which the boulder stands. It is a conglomerate, and rests on Old Red Sandstone. Another conglomerate boulder occurs at S.E. corner of the farm of Risk. (Reporter—Rev. Patrick Macgregor, Logie Almond Manse.)

Methven (Auchtergavin Parish).—Whinstone boulder, about 10 feet high, oval shaped, standing on small end, called "Sack Stone." No rock of same kind near. 800 feet above sea. (Reporter—William Duff, schoolmaster.)

Monzie.—In Glen Almond, a large stone, 8 feet high, near side of river, nearly cubical, called *Clach-Ossian*, said to mark grave of that poet. ("New St. Acct." of parish, vol. x. 264.)

Pitlochrie.—1. On road to Straloch, mica slate boulder, called "*Gledstone*," about 1800 feet above sea. Lying on drift of gravel and stratified sand. Rocks adjoining clay slate.

About 8 tons weight. Legend, that this stone gave name to Gladstone family, an infant having been found at it by a shepherd, who took it home to his wife, who nursed it.

2. Near parish church of Straloch, a huge boulder of very coarse granite, called "*Clach m'hor*," or "*Big stone*," about 24 feet diameter, and about 20 feet high. Supposed to weigh about 800 tons. Adjoining rocks clay slate. Many other boulders of mica slate and quartzite beside it. Supposed to have come from north through a valley. (Reporter—Rev. Dr Robertson, Straloch.)

Rattray.—Mica schist boulder, 12 × 6 × 6 feet, about 25 tons, called "Glenballoch Stone." Has cup and groove markings on south side. There are other boulders in Druidical circles. They have all come from hills to N. or N.W. (Reporter—Rev. Mr Herdman, Rattray.)

Renfrew.

Kilbarchan.—Porphyry boulder, 22 × 17 × 12 feet, about 300 tons. Longer axis E. and W., called "Clach a Druidh" (Stone of Druid)? Legend. Boulder differs from adjacent rocks. Same rock seen in hills 2 or 3 miles to west and north. (Reporters,—Robert Graham, D.D.; and R. L. Jack (Geol. Survey).)

Ross and Cromarty.

Alness.—In forest of Gildermoy, a very large granite boulder reported by Earl of Selkirk.

Applecross.—Three large boulders, one near shore at Rassel, called "*Clach Oiu*," weighing about 60 tons, other two about 30 tons, each called respectively "*Clach Mhoir*" and "*Clach Van*." Used as landmarks from the sea. Kaims at Ardbain and Ardrishach, extending each more than two miles along coast. (Reporter—William Ross, schoolhouse, Applecross.)

Ben Wyvis.—N.W. shoulder of, presents whole acres of rock, swept bare of soil, rounded and polished. Boulders of a peculiar veined granite have come from the Derry More (tract situated to west of Ben Wyvis), and been carried eastward to Moray Frith. These boulders found half-way up Ben Wyvis, also in valleys of Alness and Ault Grand. In Strathgarve some of

the blocks are as big as cottages. Their size lessens towards E. No boulder of same kind seen on West Coast. (Nicol "Geol. of N. of Scot.," p. 70.)

Carnock.—Five large boulders, each weighing about 20 tons. Each has a Gaelic name. One, a boundary stone. (Reporter—James Watson, schoolhouse, Strathconon, Beauly.)

Edderton.—Granite boulder, 23 × 19 × 12 feet, weighs about 290 tons. Longer axis N.E. Two others, not quite so large. All differing from adjacent rocks. (Reporter—Rev. Ewen M'Ewen, parish minister.)

Rev. Mr Joass states that this word is derived from "*Garbh*"—"*rough*," the Gaelic for "*Hill of the Pitcher*," on account of shape, its sides being almost vertical. (Rev. Mr Joass.)

Rev. Mr Joass of Golspie states, that the boulders here referred to are on a shelf or terrace about 900 feet above sea, and that their parent rock is at Carn na Cuinnaig about 12 miles to N.W.

He adds, that the boulders specified, as in the parishes of Tain and Tarbat, are probably from same source. The granite is peculiar. (See Tain and Tarbat farther on.)

Fannich Mountains.—Boulder of grey gneiss, with garnets. 30 × 10 × 5 feet, described in letter to Convener by J. F. Campbell of Islay; 2700 feet above sea; angular. Situated on watershed. Called "Clach mhor na Biachdoil." A train of large boulders to be seen in a valley not far off. Rocks also smoothed and striated. Lines of striation parallel with valleys.

Foddarty.—Boulder, 14 × 8 × 5 feet, about 40 tons. About 6 feet above sea; shape, angular; Druidical. Another with inscription illegible. Supposed to commemorate a battle between two clans. (Reporter, parish schoolmaster.)

Lochalsh.—Gneiss boulder, 9 × 7 × 8 feet; longer axis E. and W., striated. Boulder differs from adjacent rocks. Same rock said to be at Glenelg, 5 or 6 miles to south.

Boulder called after Fingal. Quartz, 7½ × 7 × 5 feet. Longer axis, N.W.; striated. At Loch Carron, said to be a kaim or diluvial bank. (Reporter—Duncan Sinclair, parish school, Lochalsh.)

Lochgair.—One granite boulder, 28 × 17 × 16 feet, about 560 tons striated. Two granite boulders, 23 × 10½ × 7 feet, about 120 tons. One of these said to be on top of a hill, and called "Sandel Stone." Legend. There are three other boulders of smaller size. Rocks *in situ* are granite. (Reporter—John MacKillop, schoolmaster.)

Shieldag (Loch Carron).—Granite boulder, 16 × 10 × 10 feet, about 120 tons. Longer axis E. and W. There is another large boulder. Both said to be in precarious positions. (Reporter—Rev. Alex. C. M'Intyre, Shieldag Manse, Dingwall.)

Tain.—Granite boulder, 18 × 12 × 8½ feet, about 60 tons. Plan and section of boulder given. Rocks of district are Old Red Sandstone. South shore of Dornoch Frith said to be thickly strewed with granite blocks, whilst none on north shore. (Reporter—Robert Gordon.)

Tarbat.—Seven or eight large boulders of gneiss and granite. Places, dimensions, and names specified, with sketches of boulders. Also, kaims of clay running E. and W. in parallel lines. One a mile long. (Reporter—Rev. George Campbell, parish minister.)

West Coast.—Vestiges of moraines, lateral and terminal, from glacier generated in valley occupied by Loch Fuir, N. of Loch Maree. (Nicol "Geol. Soc. Jour.," xiv. p. 170.)

Roxburgh.

Eckford.—Two kaims, each from 100 to 300 yards long, from 50 to 60 feet high. (Reporter—Parish schoolmaster.)

Jedburgh.—Porphyry boulder, supposed to have come from Dunion Hill, which is 2 miles to west. Formerly granite boulder on Dunion. Supposed to have come from Galloway or Dumfries now destroyed. A whinstone boulder, above Bedrule Bridge. (Reporters—Rev. Archibald Craig and Rev. Dr Ritchie.)

Melrose.—Greywacke boulder, round shaped, called "Samson's Putting Stone." (Reporter—Parish schoolmaster.)

Stirling.

Alloa.—Basaltic boulder, 13 × 11½ × 11 feet. Longer axis N. and S. Called "Hair Stane." About 70 feet above sea. (Reporter—Parish minister.)

Campsie.—Rocks glaciated. Striations W.S.W. & W.N.W. (Reporter—Rev. Thomas Monro, D.D.)

Fintray.—Boulders in a group, called "Gowk Stones." Have apparently come down valley. (Reporter—R. L. Jack (Geol. Survey).)

Kilsyth.—Mica Slate boulder, 7 × 5 × $2\frac{1}{2}$ feet, about 6 tons. 1250 feet above sea. Parent rock supposed to be 15 miles to north. (Reporter—R. L. Jack (Geol. Survey).)

Ochils.—On watersheds of, at about 2000 feet, boulder of mica schist fall of garnets, apparently from Grampians to N.W. (Jamieson, "Geol. Soc. Jour.," xxii. p. 166.)

St Ninians.—Boulder about 200 tons, at height of 1250 feet above sea. (Reporter—R. L. Jack (Geol. Survey).)

Strathblane.—Conglomerate boulder, 8 × 4 × 3 feet, about 7 tons. Longer axis W. 20° N. 1803 feet above sea. Parent rock supposed to be to N.W. (Reporter—R. L. Jack (Geol. Survey).)

Sutherland.

Assynt.—Two arge boulders, one at Unapool, the other at Stronchrubie, called "Clach na Putain" (Stone of the Button). (Reporter—Angus M'Ewen, parochial schoolmaster.)

Clyne.—Remarkable kaims, apparently moraines (lateral and terminal) in valley of Brora. Also, rocks striated at Brora quarry. Striæ run N.W. (Reporter—M. Myron.)

Golspie.—Old Red Sandstone boulder, 16 × 10 × 4 feet, lying on Oolite rocks. Longer axis, N.N.W.; sub-angular. Sketch sent. About 248 feet above sea. Three smaller boulders of Old Red Sandstone lie about 100 yards to S.E. of the above. The Old Red Sandstone formation is situated to north and west, about 3 miles from boulder. Terminal and lateral moraines occur in Brora valley, broken up by diluvial action into ridges and hummocks. (Reporter—Rev. James Joass, minister of Golspie.)

On the whole N.W. coast from Cape Wrath southwards, numerous "Perched" boulders occur on summits and sides of hills, in the most exposed positions. Especially numerous around Loch Maree. (Nicol "Geol. Soc. Journal," xiii. pp. 29, 39.)

Boulders of large size on top of Applecross Hills. Rocks below, striated. Direction of striæ S. 20° W. (true.) (Reporter—Nicol of Aberdeen.)

WIGTOWNSHIRE.

Glasserton.—Granite boulder, 9 × 6 × 6 feet, about 24 tons. Longer axis N.E. Two small boulders to east of above, and in a line with it. These boulders supposed to have come from mountains to N.E., across arm of sea. Kaims in parish, full of granite pebbles. (Reporter—Archibald Stewart.)

The following Gentleman was elected a Fellow of the Society:—

THOMAS B. CHRISTIE, M.D., F.R.C.P.E.

Monday, 6th May 1872.

D. MILNE HOME, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. On the Chemical Efficiency of Sunlight. By James Dewar, Esq.

Of all the processes proposed to measure varying luminous intensities by means of chemical effects, not one has yet been expressed in strictly dynamical measure. This is owing to the very small amount of energy to be measured necessitating very peculiar processes for its recognition. The chemical actions generally induced by light are of the "Trigger" or "Relay" description; that is, bear no necessary relation to the power evolved by the transformation. There is one natural action of light continuously at work of a very different kind in the decomposition of carbonic acid by plants, necessitating a large absorption of energy, and thus enabling us to ascertain the proportion of the radiant power retained, through the chemical syntheses effected.

So far as I am aware, the following passage extracted from Helmholtz's Lectures "On the Conservation of Energy," delivered

at the Royal Institution in 1864, and published in the "Medical Times and Gazette," contains the first estimate of the chemical efficiency of sunlight. "Now, we have seen already, that by the life of plants great stores of energy are collected in the form of combustible matter, and that they are collected under the influence of solar light. I have shown you in the last lecture that some parts of solar light—the so called chemical rays, the blue and the violet which produce chemical action—are completely absorbed and taken away by the green leaves of plants; and we must suppose that these chemical rays afford that amount of energy which is necessary to decompose again the carbonic acid and water into its elements, to separate the oxygen, to give it back to the atmosphere, and to collect the carbon and hydrogen of the water and carbonic acid in the body of the plant itself. It is not yet possible to show that there exists an accurate equivalent proportion between the power or energy of the solar rays which are absorbed by the green leaves of plants, and the energy which is stored up in the form of chemical force in the interior of the plants. We are not yet able to make so accurate a measurement of both these stores of energy, as to be able to show that there is an equivalent proportion. We can only show that the amount of energy which the rays of the sun bring to the rank is completely sufficient to produce such an effect as this chemical effect going on in the plant. I will give you some figures in reference to this. It is found in a piece of cultivated land producing corn or trees, one may reckon per year and per square foot of land 0·036 lb. of carbon to be produced by vegetation. This is the amount of carbon, which during one year, on the surface of a square foot in our latitude, can be produced under the influence of solar rays. This quantity, when used as fuel and burnt to produce carbonic acid, gives so much heat that 291 lbs. of water could be heated 1° C. Now we know the whole quantity of solar light which comes down to one square foot of terrestrial surface during one second, or one minute, or one year. The whole amount which comes down during a year to one square foot is sufficient to raise the temperature of 430,000 lbs. of water 1° C. The amount of heat which can be produced by fuel growing upon one square foot during one year is, as you see from these figures, a very small fraction of the whole amount of solar

heat which can be produced by the solar rays. It is only the 1477th part of the whole energy of solar light. It is impossible to determine the quantity of solar heat so accurately that we could detect the loss of so small a fraction as is absorbed by plants and converted into other forms of energy. Therefore, at present, we can only show that the amount of solar heat is sufficient to produce the effects of vegetable life, but we cannot yet prove that this is a complete equivalent ratio." This estimate is, strictly speaking, the mean agricultural efficiency of a given area of land, cultivated as forest, and considering that active growth only takes place during five months in the year, we may safely adopt $\frac{1}{600}$th of the total energy of sunlight as a fair value of the conserved power, on a given area of the earth's surface in this latitude during the course of the summer. As chlorophyll in one or other of its forms is the substance through which light becomes absorbed, and chemical decomposition ensues, it would be interesting to acquire some idea of the storage of power, effected by a given area of leaf surface during the course of a day, and to compare this with the total available energy. Here we are dealing with strictly measurable quantities, provided we could determine the equation of chemical transformation.

Boussingault's recent observations on the amount of carbonic acid decomposed by a given area of green leaf seem to me to afford interesting data for a new determination of the efficiency of sunlight. In his experiments made between the months of January and October under the most favourable circumstances in atmospheres rich in CO_2 one square decimetre of leaf has decomposed in one hour, as a mean 5·28 cc of CO_2, and in darkness evolves in the same period of time 0·33 cc of CO_2. In other words, one square metre of green surface will decompose in twelve hours of the day, 6336 cc of CO_2, and produce in twelve hours of the night 396 cc of CO_2.

This quantity of carbonic acid decomposed does not represent the whole work of sunlight for the time, as water is simultaneously attacked in order to supply the hydrogen of the carbo-hydrates. Boussingault, in summing up the general results of his laborious researches on vegetable physiology, says, "Si l'on envisage la vie végétale dans son ensemble, on est convaincu que la feuille est la première étape des glucoses que, plus ou moins modifies, on trouve

répartis dans les diverses parties de l'organisme ; que c'est la feuille qui les élabore aux depens de l'acid carbonique et de l'eau."— P. 415, Am. de Chemie, tom xiii. The fundamental chemical re-action taking place in the leaf, may therefore be represented as follows :—

$$(1)\quad CO,O \quad + \quad H_2O \quad = \quad CO,H_2 \quad + \quad O,O$$
$$(2)\quad 6(CO.H_2) \quad = \quad C_6H_{12}O_6$$

In the first equation carbonic acid and water are simultaneously attacked with the liberation of a volume of oxygen equal to that of the original carbonic, together with the formation of a substance having the composition of methylic aldelyde. The second equation represents the condensation of this aldelyde into grape sugar. The transformation induced in (1) necessitates the absorption of a large amount of energy; and if we neglect the heat evolved in the combination of nascent CO and H_2, which can be shown to be very little, the calculated result is made a maximum : whereas the con densation of (2) being attended with an evolution of heat, diminishes considerably the amount of power required. Happily Frankland's direct determination of the thermal value of grape sugar leaves no doubt as to the true equivalent of work done in its formation. Taking the following thermal value CO,O = 68,000, H_2 O = 68,000, $C_6H_{12}O_6$ = 642,000, 1c centimetre of CO_2 decomposed as in (1) would require 6·06 gramme units of heat, or its light equivalent; whereas the complete change into grape sugar of the same amount of carbonic acid requires only 4·78 gramme units. But we have seen before 1 square decimetre of green leaf functions at the rate of 5·28cc of carbonic acid assimilated per hour, therefore (5·28) × (4·78) = 25·23 represents the number of gramme heat units conserved through the absorption of light in the above period of time. Pouillet estimates the mean total solar radiation per square decimetre exposed normally to the sun's rays in or near Paris per hour as 6000 gramme units, so that 6000 ÷ 25·23 = $\frac{1}{237}$ represents the fraction of the entire energy conserved. The estimate is by no means too little, as Boussingault has shown the leaf may function at twice the above rate for a limited time.

In connection with equation (1), above given, as representing the action of sunlight on the leaf, it is worthy of remark, that

supposing the carbonic acid and water equally efficient as absorbing agents of the vibratory energy (although each has a specific absorption for certain qualities of rays), then the decomposition of the two compound molecules may take place continuously side by side, owing to the equality of the thermal equivalents of carbonic oxide and hydrogen. We already know, from the laborious researches of Tyndall, how thoroughly aqueous vapour retains thermal radiations; and Janssen has further shown that the same substance has a strong absorptive action on the rays of light of low refrangibility (just those rays that are in part selected by chlorophyll), producing the well-known atmospheric lines of the solar spectrum. The presence, therefore, of varying quantities of aqueous vapour in the atmosphere in all probability produces a considerable difference of rate in the decomposition effected by the leaf, and may, in fact, end in carbonic acid and water being attacked in another ratio than that given as the fundamental equation of decomposition. Thus the same plant in different atmospheric conditions may elaborate different substances.

2. On the Rainfall of the Continents of the Globe. By Alexander Buchan, Secretary of the Scottish Meteorological Society.

This paper was illustrated by two large charts of the world showing, by ISOHYETAL LINES, the rainfall over the different continents in January and July; two large charts showing the months of least and greatest rainfall in Europe, north Africa, and west Asia; and by six sets of smaller charts of thirteen each, showing, by isohyetal lines, the monthly and annual rainfall of Europe, Asia, Australasia, North America, Africa, and parts of South America. The data laid down on these eighty-two charts were taken from a Table comprising about 2000 good averages of rainfall, calculated or collected by the author.

On comparing the results of the rainfall with the author's charts of Atmospheric Pressure and Prevailing Winds, published in the Society's Transactions,* the broad principles regulating aqueous precipitation are chiefly these:—

* Vol. xxv. p. 575, *et seq.*

1. When the prevailing wind has previously traversed a large extent of ocean, the rainfall is moderately large.

2. If the winds are at the same time advancing into colder regions, the rainfall is largely increased; and if a range of mountains lie across their onward path, the rainfall is also thereby largely increased on the side facing the prevailing winds, and reduced over the regions lying on the other side.

3. If the winds, though arriving from the ocean, have not traversed a considerable extent of it, the rainfall is not large.

4. If the winds, even though having traversed a considerable part of the ocean, yet on arriving at the land proceed into lower latitudes, or regions markedly warmer, the rainfall is small or *nil*.

3. On the Lunar Diurnal Variation of Magnetic Declination at Trevandrum, near the Magnetic Equator. By J. A. Broun, F.R.S.

The author gives the results derived from different discussions of nearly eighty thousand observations, made hourly during the eleven years 1854 to 1864. They are as follows:—

1. That the lunar diurnal variation consists of a double maximum and minimum in each month of the year.

2. That in December and January the *maxima* occur near the times of the moon's upper and lower passages of the meridian; while in June and July they occur six hours later, the *minima* then occurring near the times of the two passages.

3. The change of the law for December and January to that for June and July does not happen, as in the case of the solar diurnal variations, by leaps in the course of a month (those of March and October), but more or less gradually for the different maxima and minima.

4. While the lunar diurnal variation changes the hours of maxima and minima more gradually than the solar diurnal variation, it also makes the greatest change at different times; thus the solar diurnal variation changes completely during the month of March, or from February to April, while the lunar diurnal variation makes the greatest change from April to May The second

great change which happens for the sun, between September and November, occurs earlier, or between September and October for the moon.

5. The range of the variation is greatest in January, and is least in May and October; the arc, including the mean diurnal variation for January, from eleven years' observations, being nearly 0′·5, while in the latter months the ranges were nearly 0′·18 and 0′·14 respectively; the range for July being 0′·26.

The author states, that, in a paper already published,* he has shown that the range of the diurnal variation amounts sometimes to five minutes (5′·0), which, from the less value of the horizontal force, would be equivalent to about twelve minutes (12′·0) in England; and that the diminution of range appearing in the mean of many lunations is due to the combination of variations following different laws.

6. The ranges of the mean lunar and mean solar diurnal variations thus obey different laws with reference to the period of the year; the range of the former in January being nearly double that in any month from May to September, while the range of the latter in August is nearly double that in January.

In the discussion for the change of the law which might be due to the moon's passing from one hemisphere to the other, the author found different results for different months of the year; this led him to perform the calculations in a new way, described by him, in which the law derived from observations made during the day is separated from that obtained from observations made during the night. From this discussion it follows—

7. That the action of the moon on the declination needle is, in every month of the year, greater during the day than during the night; the range of the oscillation in January and June being nearly four times greater during the day than during the night, the ratio being less in the intermediate months.

When the results are derived from the forenoon hours only, or from the afternoon hours only, the range in January is six times greater than that derived from the night hours only.

It also appears that the law derived from the night hours varies little in the course of the year; it is only that derived from the

* Trans. Roy. Soc., Edin. vol. xxiv. p. 673

day hours which becomes inverted in passing from January to July. It follows—

8. That the principal, if not the only, cause of change in the amount of the lunar action at Trevandrum, near the magnetic equator, for the moon on different meridians, depends on whether the sun is shining on the place of the needle or not.

The author finds—

9. That the area of the curve representing the lunar diurnal variation in the mean of the group of months, October to April, for the half orbit about Perigee, is to that for the other half orbit as 1·18 : 1; while for the group of months, May to September, the ratio is 1·31 : 1; the moon's action appearing to diminish more rapidly with the distance from the earth, when both moon and earth are farthest from the sun. As the mean distances of the moon from the earth in the two half orbits are nearly as 1 to 1·07, it appears that the mean range for Perigee and for Apogee, derived from both groups, varies nearly as the inverse cube of the distance, as in the case of the tides.

Monday, 20th May 1872.

PROFESSOR SIR ROBERT CHRISTISON, Bart., President, in the Chair.

The following Communications were read :—

1. Some Helps to the Study of Scoto-Celtic Philology, by the Hon. Lord Neaves.

(*Abstract.*)

Lord Neaves read a paper entitled "Some Helps to the Study of Scoto-Celtic Philology," in which, after noticing the mistaken tendencies of the Celtic scholars of former times, both Irish and Scotch, as to the origin and affinities of Gaelic, and adverting to the fact now firmly fixed that it was an Aryan or Indo-Germanic tongue, he submitted a statement of some of the imitations or disguises which words underwent or assumed in passing into Gaelic. Thus it was a peculiarity of Gaelic to avoid the letter *p*, which it

did in various ways. Sometimes it dropped that letter, as when it changed the Latin *Pater* into *Athir*, the Latin *piscis* into *iasg*, *plenus* into *làn*, &c. Sometimes it changed the *p* into a gutteral c, g, or ch, as *seachd* for *septem*, *feasgar* for *vesper*. It did this even in borrowed words, as when the Church term *Pasch* for Easter was changed into *Caisg;* the Latin *purpur* into *corcur*. It was another peculiarity of Gaelic to omit the letter *n* before certain other consonants, so that *centum* became *cead*, *quinque* became *coig*, *mensis*, *mios ; infernum*, *ifrinn ; inter*, *eadar*. The Latin *v* or English *w* was generally represented in Gaelic at the beginning of words by *f:* thus *vir*, *fear ; verus*, *fior ; vinum*, *fion ; vates*, *faidh ;* &c. The old Irish word for a widow was *fedb*. Two remarkable prefixes occurring frequently in Gaelic, *do* and *so*, correspond to similar prefixes *du* and *su* in Sanscrit: *do* and *du* meaning "evil or difficulty," and *so* and *su* meaning "goodness or facility." These prefixes are very abundant in those two languages at the two extremes of the Aryan field, but though represented also in Greek, are scarcely or very slightly perceptible in the intermediate tongues.

An attention to these and other changes which words undergo in passing into Gaelic would greatly facilitate the study of this remarkable tongue, which it is not creditable to Scotchmen to neglect as they have done. The comparative forms of the inflections of words also deserve attention, and on this subject reference might be made to an interesting lecture on the Gaelic, by Professor Geddes of Aberdeen.

2. Some Observations on the Dentition of the Narwhal (*Monodon monoceros*). By Professor Turner.

The author expressed his concurrence with those anatomists who hold that the two tusks of the narwhal are situated in sockets in the superior maxillary bones, and not, as was stated by the Cuviers, in the premaxillæ, or partly in the pre- and partly in the superior maxillæ. He then proceeded to relate some further observations on the dentition of the narwhal, and pointed out, both in the skull of a young male and in those of three well grown fœtuses, an elongated canal on each side of the upper jaw, parallel and inferior to the tusk socket, which had the appearance of a socket

for a supplementary tooth, although none protruded from it. In the young male a minute denticle was seen at the bottom of this socket.

He then described a dissection he had made of the upper jaw of a male fœtus, 7¼ inches long, given him by Mr C. W. Peach, in which, imbedded in the gum on each side, were two well-formed dental papillæ, barely visible to the naked eye. Each papilla was contained in a well-defined tooth sac. Calcification of the papillæ or of the wall of the tooth sac had not commenced. The minute structure of these embryonic teeth was next described. The more anterior of the two papillæ was $\frac{3}{10}$ths inch behind the tip of the jaw, and the more posterior lay about $\frac{1}{10}$th inch behind the anterior.

No rudimentary teeth were found in the lower jaw.

The formation of bone had only just begun in the fibrous matrix of the maxillary bones; but in the lower jaw a very decided ossification of the fibrous membrane investing the cartilage of Meckel had commenced.

3. On the occurrence of *Ziphius cavirostris* in the Shetland Seas, and a comparison of its Skull with that of Sowerby's Whale (*Mesoplodon Sowerbyi*). By Professor Turner.

This paper contained a brief historical sketch of *Ziphius cavirostris*. The skull of a specimen caught at sea in 1870, off Hamna Voe, Northmaven, Shetland, was then described, and this skull was compared with previously recorded specimens. A brief historical sketch of Sowerby's whale was then given, a skull in the Edinburgh Museum of Science and Art was described, and reasons were advanced for associating it with the genus *Mesoplodon* rather than with *Ziphius*.

4. On the Maternal Sinus Vascular System of the Human Placenta. By Professor Turner.

The author gave a brief sketch of the various theories which have been advanced by Velpeau, R. Lee, Braxton Hicks, the Hunters, Owen, Weber, J. Reid, J. Goodsir, Virchow, Kölliker, Van

Der Kolk, Arthur Farre, and Ercolani regarding to the relations of the maternal blood-vessels to the placenta and chorionic villi. He then proceeded to state the results of his own observations on various specimens of placentæ, some of which had been separated at the full time, others prematurely, and on three specimens attached to the uterine wall. Two of these latter were from women at or about the full period of gestation, whilst the third was from a woman who died undelivered in the sixth month of pregnancy. In one of the attached specimens a pipe had been introduced into a uterine vein in the broad ligament, and a coloured gelatine injection had been passed along the venous sinuses in the muscular wall, and the utero-placental veins into the placenta. The utero-placental veins were followed through the decidua serotina, and were seen to pierce the uterine surface of the placenta. The walls of these veins were so delicate that they tore through on the application of very slight force. Thin sections made through the placenta and the adjacent part of the uterine wall permitted the author to trace a direct continuity of the injection within the placenta with that within the utero-placental veins and uterine sinuses, and showed the one to be continuous with the other. The injection also passed into veins of considerable size, situated within the decidua reflexa, near the attached border of the placenta. In another attached specimen, the intra-placental sinus system was injected with coloured gelatine from a pipe inserted into one of the uterine arteries, and the injection of the system of inter-communicating spaces within the placenta was as readily made as in the specimen where the injection was passed through the uterine vein. In the third attached specimen, the injecting pipe was introduced into the cut face of a section through the placenta itself, and the intra-placental sinus system was not only distended, but some of the injection had even entered the utero-placental veins.

Thin sections of the injected placentæ had been made and examined both with low and high powers of the microscope. Drawings, greatly enlarged, of the appearances seen on examining these sections were shown to the Society, and the author pointed out that these were to be regarded as actual representations of the objects, and not, as had previously been almost universally the case, mere diagrammatic conceptions of what the anatomist might consider to

be the character of the arrangement. The chorionic villi were seen in these sections to be cut across longitudinally, obliquely, and transversely, and the villi were not in contact with each other by their surfaces, but separated by intermediate freely-communicating spaces, filled with coloured gelatine. These spaces constituted the intra-placental maternal sinus vascular system. Thin sections examined with high powers showed multitudes of red-blood corpuscles lying in the coloured gelatine, which corpuscles had undoubtedly been in these sinuses before the injection had been passed into them, and from their position were the corpuscles of the maternal blood. The ready manner in which the injection flowed into the intra-placental sinuses, either when passed directly into the placenta, or through the artery, or through the vein, the regularity and uniformity of the pattern produced by the injection when set, and the abundance of blood corpuscles present in the sinuses, mingled with the injection, seemed to the author to substantiate the view that these sinuses are a natural system of intercommunicating spaces for the transmission of the maternal blood through the interior of the placenta; and not as some have maintained, artificially produced by the extravasation of injection from the uterine vessels into the placenta.

The author then proceeded to describe the structure of the chorionic villi, to show their relations to the decidua serotina and the decidual bars which pass into the interior of the placenta, and to discuss the views which have been advanced, whether the villi hang naked in the maternal blood, or whether they are invested either by a prolongation of the lining membrane of the maternal blood-vessels, or by the cells of the decidua, or by both.

The following Gentleman was admitted a Fellow of the Society:—

Rev. Hugh Macmillan, LL.D.

Monday, 3d June 1872.

Professor W. J. MACQUORN RANKINE, Vice-President, in the Chair.

The following Communications were read:—

1. On Dimorphic Flowers of *Cephaelis Ipecacuanha*, the Ipecacuan Plant. By Professor Balfour.

I have reported already to the Society (p. 688) the results of the cultivation of the Ipecacuan plant in the Botanic Garden, and its successful propagation by Mr M'Nab by root-cutting. By this means it has been sent in considerable quantity to Calcutta, under the direction of the Secretary of State for India. From the Garden at Kew, in 1863, a plant was sent out to Dr King, and of late he has been successful in propagating it by cuttings of the stem above ground. So that from both sources there seems to be every prospect of the plant being extensively cultivated in India, the climate of which in many places is favourable for its growth. The so-called root of the Ipecacuan may be said to be composed of a sort of underground stem capable of producing leaf-buds, as well as true roots.

I have already stated that the plants in the Botanic Garden have been derived from two sources,—one from a plant sent by Sir Wm. Hooker more than 40 years ago, and which he had procured from Mr M'Koy of Liege; the other is from plants sent from Rio Janeiro by Dr Gunning. There is an apparent difference in the characters of the plants from these two sources, but not such as to amount to a specific distinction. Hooker's plant has flowered pretty freely, but never produced fruit until last year, when the pollen was artificially applied from one flower to another. All the plants from this source have long stamens and short styles.

The plants sent by Dr Gunning have grown well, but it is only recently that they have flowered, and now there are several specimens in flower, and some are fruiting after artificial impregnation. In this series of plants there are evident dimorphic flowers. In some the stamens are long and the style is short; while in others the style is long, projecting much beyond the corolla, while the stamens are short.

It would appear that successful fertilisation may be effected by applying the pollen from the long stamens to the stigma of the long styles.

The partial fruiting which took place in the heads of flowers in the Hookerian plants may have depended on the fact that there were only produced flowers with long stamens and short styles, and although when pollen was applied from one flower to another fertilisation was effected, still it was by no means fully successful, only two or three of the flowers in the head producing fruit. The flowers are sweet-scented with a delicate odour.

One of the largest plants has the following dimensions:—

Height of plant, ...	$12\frac{1}{2}$	inches.
Length of leaves, . . .	5	,,
Breadth of leaves, . . .	2	,,
Peduncle (length), . . .	1	inch
Greatest circumference of stem, .	$\frac{1}{2}$	,,

2. On the Crinoids of the "Porcupine" Deep-Sea Dredging Expedition. By Professor Wyville Thomson.

Seven species belonging to the Echinoderm order Crinoidea, were procured during the "Porcupine" dredging expeditions of 1869 and 70. Four of these belong to the free section of the order, and are referred to the genus Antedon.

1. *A. escrichtii*, J. Müller.

This fine species is abundant off the coast of Greenland, but so far as I am aware, it does not occur in the seas of Scandinavia. Several hauls of the dredge in the cold area in the channel between Scotland and Faeroe, yielded many examples, the largest of which, however, fell somewhat short of the dimensions of the largest specimens from Greenland. *Antedon escrichtii* was associated in the Faeroe channel with *Ctenodiscus crispatus*, an Asteridean which had been met with previously only in the Greenland seas. A single example of a pentacrinoid in an early stage was found associated with *Antedon escrichtii*. It resembled closely the larva of *Antedon sarsii*, but the specimen was not sufficiently perfect for a critical examination.

2. *A. sarsii*, Duben and Koren.

More or less complete specimens or fragments of this widely distributed species came up in nearly every one of the deep hauls of the dredge, from the Faeroe Islands to Gibraltar. One or two small examples of the pentacrinoid were procured in the Faeroe Channel.

3. *A. rosaceus*, Linck.

Frequent in water of moderate depth. Many examples of the form known to continental naturalists under the name of *A. mediterraneus*, Lam. sp., were dredged in the Mediterranean off the coast of Africa. I do not feel satisfied that this is identical with *Antedon rosaceus* of the coast of Britain, although the two specific names are usually regarded as synonyms. There is a great difference between them in habit; a difference which it is difficult to define.

4. *A. celticus*, Barrett.

This species, which is at once distinguished by the extreme length of the dorsal cirri, is abundant at depths of 40 to 60 fathoms in the Minch, and we also met with it in local patches to 150 fathoms off the north coast of Scotland.

The remaining three Crinoids belong to the section of the Order which are permanently stalked. Two of the three are new to science, and the third was discovered in the year 1864 by G. O. Sars, in the deep water off the Loffoden Islands.

Up to the present time two recent species have been described belonging to the Family Pentacrinidæ. Both of these were known only from the deep water of the seas of the Antilles. Since the discovery of the first of these in the year 1755, they have been regarded with special interest, both on account of their great beauty, and of the singular relation which they bear to some of the most abundant and characteristic fossils of the palæozoic and mezozoic formations.

Pentacrinus asteria, L., the species first described by Guettard, and afterwards very carefully worked out by Johannes Müller, has a stem sometimes nearly a metre in length consisting of a multitude of discoidal joints about every seventeenth of which bears a circle of five long cirri which spread out rigidly and abruptly

from the joint, turning down hooklike towards the tips. Each cirrus consists of about 36 joints. The nodal joint, that is to say the joint modified for the insertion of the cirri, is single; but it is united to the joint beneath by a peculiar suture with much of the character of a syzygy. Most of the examples of *P. asteria* which have reached Europe have had the stem recently broken. In one however in my possession, the stem, which is unusually short, had evidently given way at one of these joints long before the death of the animal, for the surface of the terminal joint is smoothed and rounded, and the terminal row of cirri are curved over it. This example, at all events, must have lived for some time free.

In *Pentacrinus asteria*, the basal plates of the cup project like small round buttons over the ends of the salient angles of the first stem joint. The first radials are connected with the second radials by a true joint with muscles and ligaments, and the second radial is united to the radial axillary by a syzygy. There are from 70 to 120 pinnated arms. There is constantly a syzygy on each branch at the first joint beyond each bifurcation, but there are few syzygies on the arms after their last bifurcation, although in some specimens one is met with here and there.

All the examples of *P. asteria* in European museums have lost the soft parts and the disk; but I have one example which is complete. The mouth is central, and five radial grooves pass from the edge of the mouth-opening to the proximal ends of the arms, and become continuous with the brachial grooves, dividing with each bifurcation. The perisom of the disk is covered with irregular calcareous plates, and at the free inner angles of the interradial spaces these plates become closer, and form a solid kind of boss; but there are no distinct oral plates. A rather long anal tube occupies the centre of one of the interradial spaces.

Pentacrinus mülleri, Œrstedt, seems to be more common than *P. asteria* especially off the Danish West Indian Islands. The whole animal is more delicate in form. The stem attains nearly the same height, but is more slender. The nodes occur about every twelfth joint and at every node two stem-joints are modified. The upper joint bears the facets for the insertion of the cirri, and the second is grooved to receive the thick basal portions of the cirri, which bend downwards for a little way closely adpressed to the

stem before becoming free. The cirri are much shorter than in *P. asteria*. The syzygy is between the two modified joints. In all complete specimens which I have seen, the stem has evidently been separated for long at one of these syzygies. I described some years ago a specimen in which this was the case, and suggested that in that instance the animal had lived for some time free. I have since seen several other examples in the same condition, and I believe that the disengagement at a certain stage of growth is habitual. The arrangement of the joints and syzygies in the cup is the same in *P. mülleri* as in *P. asteria*, only the syzygy between the second radial and the radial axillary is not so complete. The arms are more delicate, and appear never to exceed thirty in number. The number of syzygies is very variable; sometimes they are confined, as in *P. asteria*, to the first joint after a bifurcation, and sometimes they occur at intervals all along the arms. The structure of the disk is the same as in *P. asteria*, but its texture is more delicate, and the calcareous pieces are smaller and more distant.

On the 21st of July 1870, Mr Gwyn Jeffreys, dredging from the "Porcupine," at a depth of 1095 fathoms, latitude 39° 42′ N. long. 9° 43′ W., with a bottom temperature of 4°·3 C., took about twenty specimens of a handsome Pentacrinus involved in the hempen tangles attached to the dredge.

1. *P. wyville-thomsoni*, Jeffreys.

This species is intermediate in some of its characters between *P. asteria* and *P. mülleri*, it approaches the latter however most nearly. In a mature specimen the stem is about 120 mm. in length and consists of five to six internodes. The whorls of cirri towards the lower part of the stem are 40 mm. apart, and the internodes consist of from thirty to thirty-five joints. The cirri are rather short, and stand out straight from the nodal joint or curve slightly downwards. There are usually eighteen joints in the cirri, the last forming a sharp claw. As in *P. asteria* the nodal joint is single, and a syzygy separates it from the joint immediately beneath it which does not differ materially in form from the ordinary internodal stem-joints. All the stems of mature examples of this species end inferiorly in a nodal joint surrounded by its whorl of cirri, which curve downwards into a

kind of grappling root. The lower surface of the terminal joint is in all smoothed and rounded, evidently by absorption, showing that the animal has long been free. This character I have already noted as occurring in some specimens of *P. mülleri* and in one at least of *P. asteria*. I have no doubt whatever that it is constant in the present species, and that the animal lives loosely rooted in the soft mud, and may change its place at pleasure by swimming with its pinnated arms: that it is, in fact, intermediate in this respect between the free species of *Antedon* and the permanently rooted fossil crinoids.

A young specimen of *P. wyville-thomsoni* gives the mode in which this freedom is acquired. The total length of this specimen is 95 mm., of which the head occupies 35 mm. The stem is broken off in the middle of the eighth internode from the head. The lowest complete internode consists of 14 joints, the next of 18, the next of 20, and the next of 26 joints. There are 8 joints in the cirri of the lowest whorl, 10 in those of the second; 12 in those of the third, and 14 in those of the fourth. This is the reverse of the condition in adult specimens, in all of which the numbers of joints in the internodes, and of joints in the cirri, decrease regularly from below upwards. The broken internode in the young example and the three internodes above it are atrophied and undeveloped; and suddenly at the third node from the head the stem increases in thickness and looks as if it were fully nourished. There can be no doubt that in early life the Crinoid is attached, and that it becomes disengaged by the withering of the lower part of the stem.

The structure of the cup is the same as in *P. asteria* and *P. mülleri*. The basals appear in the form of shield-like projections crowning the salient angles of the stem. Alternating with these we have well-developed first radials forming a closed ring and articulating to free second radials by muscular joints. The second radials are united by a syzygy to the radial axillaries, which as usual give off each two first brachials from their bevelled sides. A second brachial is united by syzygy to the first, and normally this second brachial is an axillary, and gives off two simple arms; sometimes, however, the radial axillary originates a simple arm only from one or both of its sides, thus reducing the

total number of the arms, and sometimes one of the four arms given off from the brachial axillaries again divides, in which case the total number of arms is increased. The structure of the disk is much the same as in the species of the genus previously known.

The Apiocrinidæ to which the remaining two fixed Crinoids must be referred, differ from all other sections of the order in the structure of the upper part of the stem. At a certain point considerably below the crown of arms the joints of the stem widen by the greater development of the calcified ring, the central cavity scarcely increasing in width. The widening of the stem-joint increases upwards until a pyriform body is produced, usually very elegant in form, in which one would suppose looking at the outside that the viscera were lodged. It is, however, nothing more than a symmetrical thickening of the stem, and the body cavity occupies a shallow depression in the top of it inclosed within the plates of the cup; the basals and radials are much thicker and more fully calcified than in other crinoids, but they are normally arranged.

The stem is usually long and simple, until near the base, where it forms some means of attachment; either as in the celebrated pear encrinites of the forest-marble, a complicated arrangement of concentric layers of cement which fix it firmly to some foreign body; or as in the chalk *Bourguetticrinus* and in the recent *Rhizocrinus*, an irregular series of jointed branching cirri.

The Apiocrinidæ attained their maximum during the Jurassic period, where they are represented by numerous and fine species of the genera *Apiocrinus* and *Millericrinus*. The chalk genus *Bourguetticrinus* shows many symptoms of degeneracy. The head is small, and the arms are small and short. The arm joints are so minute that it is difficult to make up anything like a complete series from the separate fragments scattered through the chalk in the neighbourhood of a cluster of heads. The stem, on the other hand, is disproportionately large and long, and one is led to suspect that the animal was nourished chiefly by the general surface absorption of organic matter, and that the head and special assimilative organs are principally concerned in the function of reproduction. The genus Rhizocrinus possesses all the essential characters of the family.

1. *R. lofotensis*, M. Sars.

This species was discovered in the year 1864, at a depth of about 300 fathoms, off the Loffoden Islands, by G. O. Sars, a son of the celebrated Professor of Natural History in the Uuiversity of Christiania; and it was described in detail by the latter in the year 1868. It is evidently a form of the Apiocrinidæ still more degraded than *Bourguetticrinus*, which it closely resembles. The stem is long and of considerable thickness in proportion to the size of the head. The joints of the stem are individually long and dice-box shaped, and between the joints spaces are left on either side of the stem alternately, as in *Bourguetticrinus*, and in the pentacrinoid of *Antedon* for the insertion of fascicles of contractile fibres. Towards the base of the stem branches spring from the upper part of the joints; and these, each composed of a succession of gradually diminishing joints, divide and re-divide into a bunch of fibres which expand at the ends into thin calcareous laminæ, clinging to small pieces of shell, grains of sand—anything which may improve the anchorage of the crinoid in the soft mud which is nearly universal at great depths.

In *Rhizocrinus* the basal series of plates of the cup are not distinguishable. They are masked in a closed ring at the top of the stem, and whether the ring be composed of the fused basals alone, or of an upper stem-joint with the basals within it forming a "rosette" as in the calyx of *Antedon*, is a question which can only be solved by a careful tracing of successive stages of development. The first radials are likewise fused, and form the upper wider portion of the funnel-shaped calyx The first radials are deeply excavated above for the insertion of the muscles and ligaments which unite them to the second radials by a true (or moveable) joint. One of the most remarkable points in connection with this species is, that the first radials, the first joints of the arm, are variable in number, some examples having four rays, some five, some six, and a very small number seven in the following proportions. Out of seventy-five specimens examined by Sars, there were—

15	with	4 arms.
43	,,	5 ,,
15	,,	6 ,,
2	,,	7 ,,

This variability in so important a character, particularly when associated with so great a preponderance in bulk of the vegetative over the more specially animal parts of the organism, must undoubtedly be accepted as indicating a deterioration from the symmetry and compactness of the Apiocrinidæ of the Jurassic period.

The anchylosed ring of first radials is succeeded by a tier of free second radials, which are united by a straight syzygial suture to the next series—the radial axillaries. The surface of the funnel-shaped dilation of the stem, headed by the ring of first radials, is smooth and uniform, and the second radials and radial axillaries present a smooth regularly arched outer surface. The radial axillaries differ from the corresponding joints in most other known crinoids in contracting slightly above, presenting only one articulating facet, and giving origin to a single arm. The arms, which in the larger specimens are from 10 to 12 mm. in length, consist of a series of from about twenty-eight to thirty-four joints, uniformly transversely arched externally, and deeply grooved within to receive the soft parts. Each alternate joint bears a pinnule alternating on either side of the axis of the arm, and the joint which does not bear a pinnule is united to the pinnule-bearing joint above it by a syzygy: thus joints with muscular connections and syzygies alternate throughout the whole length of the arm.

The pinnules, twelve to fourteen in number, consist of a uniform series of minute joints united by muscular connections. The grooves of the arm and of the pinnules are bordered by a double series of delicate round fenestrated calcareous plates, which, when the animal is contracted and at rest, form a closely imbricated covering to the nerve and the radial vessel with its delicate cæcal tentacles. The mouth is placed in the centre of the disk, and radial canals, equal in number to the number of arms, pass across the disk, and are continuous with the arm grooves. The mouth is surrounded by a row of flexible cirri arranged nearly as in the pentacrinoid of *Antedon*, and is provided with five oval calcareous valve-like plates occupying the interradial angles, and closing over the mouth at will. A low papilla in one of the interradial species indicates the position of the minute excretory orifice.

Rhizocrinus lofotensis is a very interesting addition to the British

Fauna. We met with it in the Faeroe Channel in the year 1869,—three examples, greatly mutilated, at a depth of 530 feet, with a bottom temperature of 6°·4 C. Station 12 (1868) —Several occurred attached to the beards of *Holteniæ* off the Butt of the Lews, and specimens of considerably greater size were dredged in 862 fathoms off Cape Clear. The range of this species is evidently very wide. It has been dredged by G. O. Sars off the north of Norway; by Count Pourtales, in the Gulf-stream off the coast of Florida; by the naturalists on board the "Josephine" on the "Josephine Bank" near the entrance of the Strait of Gibralter; and by ourselves between Shetland and Faeroe, and off Ushant and Cape Clear.

The Genus Bathycrinus (n. g.) must also apparently be referred to the Apiocrinidæ, since the lower portion of the head consists of a gradually expanding funnel-shaped piece, which seems to be composed of coalesced upper stem-joints.

1. *B. gracilis* (n. sp.).

The stem is long and delicate, in one example of a stem alone, which came up in the same haul with the one perfect example which was procured, it was 90 mm. in length. The joints are dice-box shaped as in *Rhizocrinus*, long and delicate, towards the lower part of the stem 3·0 mm. in length by 0·5 mm. in width in the centre, the ends expanding to a width of 1·0 mm. As in *Rhizocrinus*, the joints of the stem diminish in length towards the head, and additions are made in the form of calcareous laminæ beneath the coalesced joints which form the base of the cup.

The first radials are five in number. They are closely opposed, but they do not seem to be fused as in *Rhizocrinus*, as the sutures show quite distinctly. The centre of each of the first radials rises into a sharp keel, while the sides are slightly depressed towards the sutures, which gives the calyx a fluted appearance, like a folded filter paper. The second radials are long and free from one another, joining the radial axillaries by a straight syzygial union. They are most peculiar in form. A strong plate-like keel runs down the centre of the outer surface, and the joint is deeply excavated on either side, rising again slightly towards the edges. The radial axillary shows a continuation of the same keel through its lower half, and midway up the joint the

keel bifurcates, leaving a very characteristic diamond-shaped space in the centre towards the top of the joint. Two facets are thus formed for the insertion of two first radials. The number of arms is therefore ten. The arms are perfectly simple, and in our single specimen consist of twelve joints each. There is no trace of pinnules, and the arms resemble in character the pinnules of *Rhizocrinus.* The first brachial is united to the second by a syzygial joint, but after that the syzygies are not repeated, so that there is only one of these peculiar junctions in each arm. The arm-grooves are bordered by circular fenestrated plates as in *Rhizocrinus.*

Certain marked resemblances in the structure of the stem, in the structure of the base of the cup, and in the form and arrangement of the ultimate parts of the arms, evidently associate *Bathycrinus* with *Rhizocrinus;* but the differences are very wide. Five free keeled and sculptured first radials replace the uniform smooth ring formed by these plates in *Rhizocrinus.* The radial axillaries give off each two arms, thus recurring to the more usual arrangement in the order, and the alternate syzygies on the arms, which form so remarkable a character in *Rhizocrinus*, are absent.

Only one nearly complete specimen and a detached stem of this very remarkable species were met with, and they were both brought up from the very greatest depth which has as yet been reached with the dredge, 2435 fathoms, at the mouth of the Bay of Biscay, 200 miles south of Cape Clear.

3. Laboratory Notes. By Professor Tait.

1. On Thermo-electricity: Circuits with more than one Neutral Point. (With a Plate.)

Having lately obtained from Messrs Johnson & Matthey some wires of platinum, and of alloys of platinum and iridium, I formed them into circuits with iron wire of commerce; and noticed that with all, excepting what is called "soft" platinum, there is more than one neutral point situated below the temperature of low white heat, and that at higher temperatures other neutral points occur. This observation is, in itself, highly interesting; but my first impression was one of disappointment, as I imagined it depended on some peculiarity of the platinum metals, which I had hoped would

furnish me with the means of accurately measuring high temperatures (by a process described in previous notes of this series). As this hope may possibly not be realised, I can as yet make only rough approximations to an estimation of the temperatures of these neutral points.

So far as I am aware, the phenomenon discovered by Cumming and analysed by Thomson has hitherto been described thus: When the temperature of the cold junction is below the neutral point, the gradual raising of the temperature of the other produces a current which increases in intensity till the neutral point is reached, thenceforth diminishes; vanishes when one junction is about as much above the neutral point as the other is below it, and is *reversed* with gradually increasing intensity as the hot junction is farther heated. To discover how my recent observation affects this statement, I first simply heated one junction of a circuit of iron and (hard) platinum gradually to whiteness, by means of a blowpipe, and observed the indications of a galvanometer—both during the heating and during the subsequent cooling when the flame was withdrawn. The heating could obviously not be effected at all so uniformly as the cooling; but, making allowance for this, the effects occurred in the opposite order, and very nearly at the same points of the scale in the descent and in the ascent. [I have noticed a gradual displacement of the neutral points when the junction was heated and cooled several times in rapid succession; but as my galvanometer, though it comes very quickly to rest, is not quite a *dead-beat* instrument, I shall not farther advert to this point till I have made experiments with an instrument of this more perfect kind, which is now being constructed for me.] The observed effect of heating, then, was a rise from zero to 110 scale divisions when the higher temperature was that of the first neutral point, then descent to 95 at a second neutral point, then ascent to a third, descent to a fourth, neither of which could be at all accurately observed, and finally ascent until the junction was fused.

With an alloy of 15 per cent. iridium and 85 per cent. platinum, the galvanometer rose to 53·5 at a neutral point, then fell to − 50 at a second, then rose to a third at − 39·5, and thence fell, but I could not observe a possible fourth neutral point on account of the

fusion of the iron. As shown on the plate, the first of these occurs at about 240° C. of a mercurial thermometer.

With another alloy supposed to be of the same metals, but of which I do not yet know the composition, also made into a junction with iron, the behaviour was nearly the same, but the readings at the successive neutral points were 28, – 137, – 132. The temperature of the first is about 200° C. by mercurial thermometer.

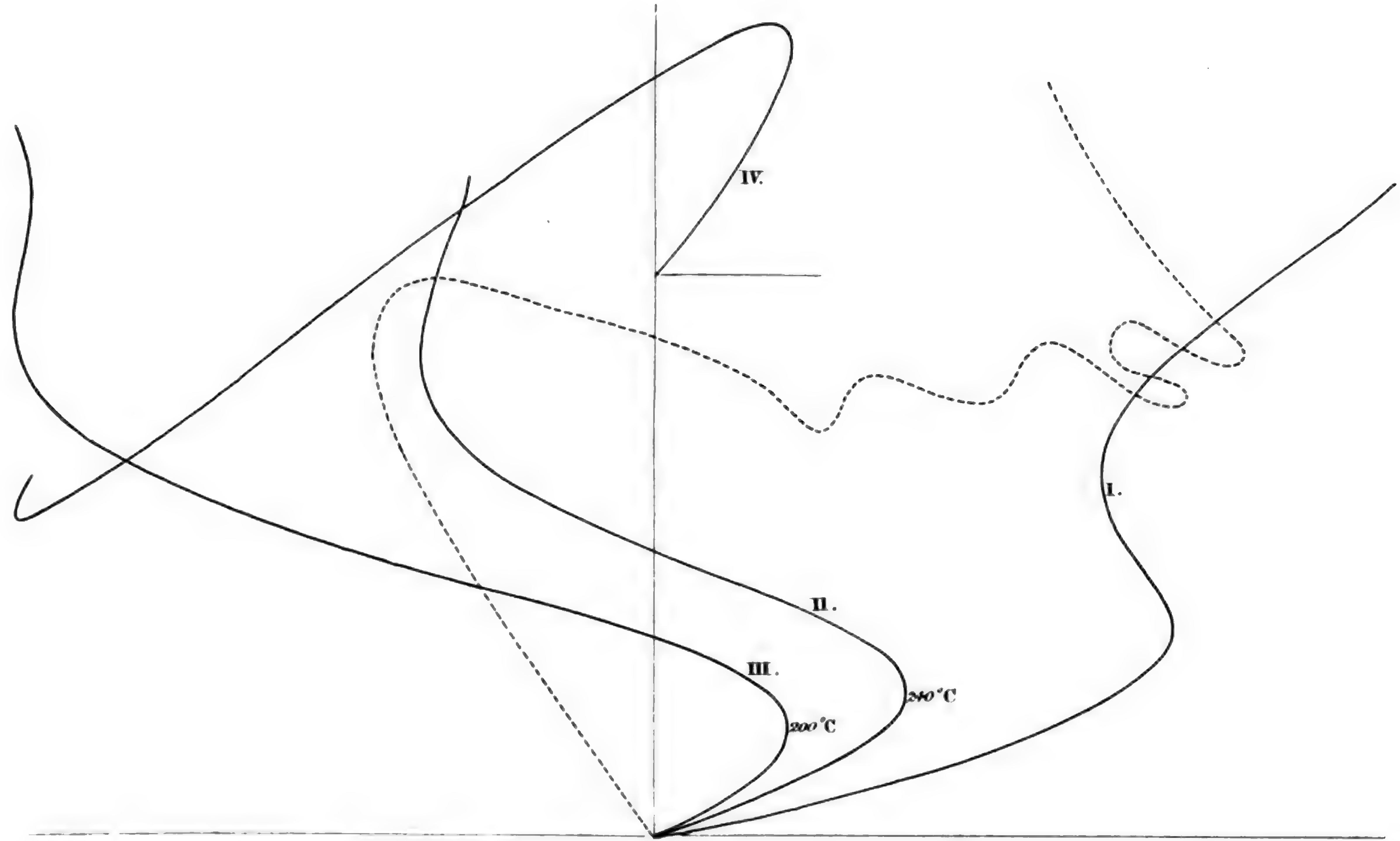
An iron-palladium circuit showed no neutral points within the great range of temperatures mentioned above; though it showed a remarkable peculiarity which must be more closely studied, as it appears to point to the cause of the above effects in a property of iron. It was therefore employed to give (very roughly) an indication of the actual temperatures in these experiments. But as for this purpose it is necessary to measure the simultaneous indications of two circuits whose hot and whose cold junctions are respectively at the same temperatures, I was obliged to employ a steadier source of heat than the naked flame. I therefore immersed the hot junctions in an iron crucible containing borax glass, subsequently exchanged for a mixture of fused carbonate of soda and carbonate of potash; but, to my surprise, the former of these substances at a red heat disintegrated both the platinum and the alloy, and thus broke both circuits without sensibly acting on the iron, while the mixture (evidently by the powerful currents discovered by Andrews, *Phil. Mag.* 1837) interfered greatly with the indications of the thermo-electric circuit, as will be seen by the dotted curve in the plate. [I may remark here that the deviations of this curve from its form when these currents are prevented are quite easily observed and plotted by the process next to be mentioned, so that the study of the Andrews' effect may be carried out with great accuracy by my method.] Finally, determining to dispense altogether with fused salts, which conduct too well besides acting on the metals, I simply suspended a red-hot bombshell, vent downwards, in such a way that the hot junction was near its centre. This arrangement worked admirably, until a white heat was required, for this melted the shell. In its place a wrought iron tube (an inch in bore, four inches long, half an inch thick, and closed at the upper end) has been substituted and answers excellently. It does not cool too fast for accurate reading at the higher temperatures, and by elevating

it by degrees from over the hot junction we can make the cooling fast enough at the lower ranges. In fact, I believe that if I do not succeed in getting a sufficient number of practically infusible metals to construct my proposed thermometric arrangement, I may be able to make a fair approximation to temperatures by simple time observations made with the hot tube, surrounded by some very bad conductor, such as sand, where the surface in contact with the air is always comparatively cool, and where therefore we can accurately calculate the rate of cooling.

Curves I., II., III., in the plate were drawn by means of this apparatus. The hot junction consisted of an iron wire, a palladium wire, and (for the several curves in order)—I. Hard platinum; II. Pt 85, Ir 15; III. The other alloy of Pt and Ir. The free ends of the palladium wire, and of the platinum or alloy, were joined to iron wires, and the junctions immersed in test-tubes filled with water resting side by side in a large vessel of cold water. The other ends of these three iron wires, and the wires of the galvanometer, were led to a sort of switch, by means of which either circuit could be instantly made to include the galvanometer. Readings were taken of each circuit as fast after one another as possible (with the galvanometer I employed about 6·5 seconds was the *necessary* interval), and the mean of two successive readings of one circuit was taken as being at the same temperature as that of the intermediate reading of the other.

The indications of these curves are very curious as regards the effect of even small impurities on the thermo-electric relations of some metals. It is probable, from analogy, that the curve for iron and *pure* platinum, in terms of temperature, would be (approximately, at least; even if it should be the iron, and not the platinum metal, which is represented by a broken or curved line) a parabola with a very distant vertex. And it appears probable that when the wire of curve III. is analysed it will be found to contain even a larger percentage of iridium (?) than that of curve II.

I find, by tracing these curves on ground glass, allowing for the difference between temperatures and the indications of an Fe-Pd circuit, and superposing them on a nest of parabolas with a common vertex and axis, that they can be closely represented by successive portions of different parabolas (with parallel axes) whose


IV.
I.
II.
III.
240°C
200°C

gents coincide at the points of junction, though the *curvature* necessarily not continuous from one to the other. Hence, as at st a fair approximation to the electro-motive force in terms of 'erence of temperature in the junctions, we may assume a para-ic function, which up to a certain temperature belongs to one abola, then changes to another without discontinuity of direc-n, and so on.

Hence either the iron, or the hard platinum and the platinum-lium alloys, will be (approximately, at least) represented on my m of Thomson's thermo-electric diagram (*anté* p. 601) by *broken* es, of which the successive parts are straight. This, contrasted th the (at least nearly) straight lines for pure metals, seems show that some bodies take successively different states (*i.e.*, come *different substances*) at certain "critical" temperatures, re-ining their thermo-electric properties nearly unchanged from one those critical points to another.

The curve marked IV. in the figure was obtained by plotting ;ainst each other the simultaneous indications of the alloy of curve I. and iron, and of the alloy of curve II. and iron, so as to avoid y disturbance from possible peculiarities of palladium. Then, to tain an idea of the share taken by iron in the results, it was found at the electro-motive force in a circuit formed by the two alloys, by either with hard Pt, is (for a very great range of temperature) nsibly proportional to the temperature difference of the junctions. The same result is easily seen from the plate, if we notice that e difference of corresponding ordinates in any two of curves I., I., III., is nearly proportional to the corresponding abscissa. Now, seems a less harsh supposition that the lines representing pla-num and its alloys are nearly straight and parallel, while that of on is a broken line, than that the latter should be straight and e former all broken at the same temperatures. On the other and, this latter hypothesis would make k alternately negative and ositive in iron, while the former would only require the platinum etals to have values of k alternately less and more negative than hat of iron.

I may add that none of the above-mentioned effects can be due o altered electric resistance of the heated junctions, because the ;alvanometer resistance was about 23 B. A. units, while that of the

iron and platinum wires together was in each case not more than one such unit. The palladium-iron circuit was so much more powerful than the others that a resistance coil of about 146 B.A. units had to be inserted in its course.

Assuming, for a moment, that, as above suggested as at least approximately true, in one of the wires we have $\sigma = k_1 t$ up to the temperature t_1, $\sigma = k_2 t$ up to temperature t_2, &c., we have by the two equations of thermo-dynamics—

$$E = J\left(\Sigma\Pi + \Sigma_1^n \int_{t_r}^{t_{r+1}} \sigma_r dt\right)$$

$$0 = \Sigma\frac{\Pi}{t} + \Sigma_1^n \int_{t_r}^{t_{r+1}} \frac{\sigma_r}{t} dt.$$

Now, if both junctions be under t_1, and if $\sigma = kt$ for the other wire,

$$\delta E = J(\delta\Pi + \overline{k_1 - k}t\delta t)$$

$$0 = \delta\frac{\Pi}{t} + (k_1 - k)\delta t,$$

and we have as before, t_0 being temperature of cold junction,

$$\frac{\Pi}{t} = -(k_1 - k)(T - t)$$

$$E = -(k_1 - k)(t - t_0)\left(T - \frac{t + t_0}{2}\right).$$

But from t_1 to t_2 we have

$$\frac{\Pi}{t} = -(k_2 - k)(T_1 - t)$$

$$E = C - (k_2 - k)(t - t_0)\left(T_1 - \frac{t + t_0}{2}\right).$$

Now, at $t = t_1$ these formulæ must agree, so that

$$(k_2 - k)(T_1 - t_1) = (k_1 - k)(T - t_1)$$

$$C = (t_1 - t_0)\left\{(k_2 - k)T_1 - (k_1 - k)T - (k_2 - k_1)\frac{t_1 + t_0}{2}\right\},$$

whence

$$T_1 = \frac{(k_2 - k_1)t_1 + (k_1 - k)T}{k_2 - k},$$

and

$$C = (t_1 - t_0)(k_2 - k_1)\left(t_1 - \frac{t_1 + t_0}{2}\right) = \frac{1}{2}(k_2 - k_1)(t_1 - t_0)^2.$$

I reserve farther developments of this subject until I have made a sufficient number of experiments with *both* junctions at high temperatures, particularly when these are two of the series of neutral points; and especially until I manage to settle, by one at least of several processes which have occurred to me, whether the multiple neutral points depend upon peculiarities in the behaviour of the iron, or of the platinum, or of both.

[*Added during printing.*—I have since made out that the lines of the diagram are approximately straight, and parallel to the lead line, for the platinum metals, that of hard platinum being below the lead line, while those of most of the other alloys are above it, and that the multiple neutral points depend upon the peculiar sinuosity of the line for iron. I have also obtained curious results of a somewhat similar kind with steel wire. The method I employed was to explore the part of the thermo-electric diagram included between the lines of gold and palladium, by making a multiple arc of these two metals, and varying the ratio of their separate resistances. But I reserve details until I have carefully examined the behaviour of nearly pure iron.]

2. On a Method of Exhibiting the Sympathy of Pendulums.

While making some magnetic experiments lately with Mr Fox Talbot, I happened to notice that two equal rectangular pieces of tin plate, when standing nearly parallel to one another on the pole of a large electromagnet, acted on one another so that a vibration communicated to either was in a few seconds handed over to the other, and *vice versâ*.

The definiteness of the result led me to try the experiment with ordinary bar magnets. Taking two large magnetised bars of almost exactly equal mass, I suspended them with their axes in the same horizontal line, so that their (small) vibrations were executed in that line, their undisturbed periods being very nearly equal, and the distance between them (when at rest) so small compared with their lengths, that we need consider only the magnetic action of the two poles nearest together. With this apparatus the transfer of energy from one pendulum to the other is most beautifully exhibited, for if one only be in motion at starting, the magnets

alternately come sharply to rest at successive equal intervals of time. This arrangement makes an excellent and instructive class experiment, and its value may be greatly increased by placing round the exterior end of one of the magnets a vertical coil of copper-wire connected with a distant galvanometer. The nature of the motion of this magnet at any instant is readily deciphered from the signals given by the reflected light on the galvanometer scale, which is also visible to the whole class. A more complex, but with practice easily intelligible, signal is given by placing the coil round the contiguous ends of the magnets.

The extension of this arrangement to three, four, and more equal magnets, all vibrating in one line, and of nearly equal mass, magnetic power, and (independent) period is of course obvious, and forms a beautiful mechanical illustration of the solution of a differential equation.

In thinking how most simply to explain such results to an elementary class, I was led to the following, which can hardly be new, though I have never met with it, but which is certainly not as well known as it ought to be. Take first the case of the two equal magnets.

Since there are but two moving parts of the system, and each has but *one* degree of freedom, it is obvious that if we can find *two* different forms of motion of the system which, once established, will persist for ever, any motion whatever of the system must be a mere superposition of these two modes with arbitrary amplitudes and epochs. Now, one such mode is obviously the motion of the pendulums *as one piece* at their equilibrium distance from one another. As the magnetic force does not vary during this motion, the time of vibration is that of either pendulum when left to itself. The other fundamental mode is that in which the centre of inertia of the two remains fixed, *i.e.*, the simultaneous displacements of the two magnets are equal and in opposite directions. The time of small oscillations now will evidently be the same as if one of the magnets were held fixed and its magnetic strength doubled. It will, therefore, be shorter or longer than the former period, according as the poles presented to one another attract or repel, and its actual value is easily calculated. Hence, as these small motions separately can be represented by expressions such as $\cos(mt + \epsilon)$,

$\cos(m't + \epsilon')$; the period of any complex vibration is $\frac{2\pi}{m - m'}$, and therefore at intervals of $\frac{\pi}{m - m}$ the configuration of the magnets will be the same to a spectator who changes the side from which he regards them in successive such intervals. Thus, if one magnet was originally at rest, the two will alternately be reduced to rest.

When there are three equal magnets, it is easy to see that one fundamental mode is a swing of the whole as one piece, a second (if we suppose like or unlike poles adjacent to each other at each gap) is the middle magnet and the centre of inertia of the other two fixed, and the third has also the centre of inertia fixed, but the two extreme magnets are at each instant equally deflected in the same direction, while the middle one has a double deflection to the opposite side. It is troublesome, but not difficult, to think out the fundamental modes for four and even for five magnets; but it would be a waste of time to try it in that way for more.

Generally if x_r denote the displacement at time t of the rth magnet, and if we assume the masses, magnetisation, and gaps to be equal, we have

$$\ddot{x}_r + n^2 x_r = \mu\left(\frac{1}{(a + x_r - x_{r-1})^2} - \frac{1}{(a + x_{r+1} - x_r)^2}\right)$$

$$= \frac{\mu}{a^3}(x_{r-1} + x_{r+1} - 2x_r),$$

except for the ends of the series where $r = 1$, and $r = m$, the number of magnets.

Hence, multiplying by λ_r and adding, we have

$$\ddot{\xi} + p^2\xi = 0,$$

where

$$\xi = \Sigma\lambda_r x_r$$

$$p^2 = \frac{\lambda_1\left(n^2 + \frac{\mu}{a^3}\right) - \lambda_2\frac{\mu}{a^3}}{\lambda_1} = \frac{-\lambda_1\frac{\mu}{a^3} + \lambda_2\left(n^2 + \frac{2\mu}{a^3}\right) - \lambda_3\frac{\mu}{a^3}}{\lambda_2} = \&c.$$

It will be sufficient to work this out for three magnets. Here, if we put $\frac{\mu}{n^2a^3} = e$, we have

$$\frac{p^2}{n^2} = 1 + e - e\frac{\lambda_2}{\lambda_1} = -e\frac{\lambda_1}{\lambda_2} + 1 + 2e - \frac{\lambda_3}{\lambda_2}e = -\frac{\lambda_2}{\lambda_3}e + 1 + e.$$

$$\therefore \frac{\lambda_2}{\lambda_1} = \frac{\lambda_2}{\lambda_3}, \text{ or } \lambda_1 = \lambda_3, \text{ besides } \lambda_2 = 0;$$

whence

$$-\frac{\lambda_2}{\lambda_1}e = -e\frac{\lambda_1}{\lambda_2} + e - \frac{\lambda_1}{\lambda_2}e$$

$$\left(\frac{\lambda_2}{\lambda_1}\right)^2 + \frac{\lambda_2}{\lambda_1} - 2 = 0,$$

$$i.e., \frac{\lambda_2}{\lambda_1} = 1, \text{ or } -2, \text{ or } 0.$$

Thus $p^2 = n^2$, or $n^2(1 + 3e)$, or $n^2(1 + e)$. There is no farther difficulty in applying the method to magnets of different masses or magnetic strengths; but it is interesting to observe that, by properly adjusting the gaps in terms of the masses and magnetisation of the bars, any set of magnets whatever can be brought to behave (for small oscillations) as if they were in all respects equal to each other and arranged at equal distances.

When there is an infinite series of magnets arranged in this way the equation above may be written

$$\left[\left(\frac{d}{dt}\right)^2 + n^2 + \frac{\mu}{a^3}\frac{(\mathrm{D}-1)^2}{\mathrm{D}}\right]x_r = 0,$$

where

$$\mathrm{D}x_r = x_{r+1},$$

of which the general integral is easily found.

When the number of magnets (m) is finite, and they are arranged in a closed curve, we have the conditional equation

$$(\mathrm{D}^m - 1)x_r = 0.$$

In this case the general solution may be elegantly expressed in terms of the m^{th} roots of unity. It leads to some curious properties of determinants, whose development will form an excellent exercise for the student. Thus, writing in succession $1, 2, \ldots, m$ for r; and putting

$$l = \frac{a^3}{\mu}\left(\left(\frac{d}{dt}\right)^2 + n^2\right),$$

the first of the above equations gives, by the help of the second, after the elimination of the displacements

$$\begin{vmatrix} l-2 & 1 & & & & & 1 \\ 1 & l-2 & 1 & & & & \\ & 1 & l-2 & 1 & & & \\ & & & \&c. & & & \\ & & & & 1 & l-2 & 1 \\ 1 & & & & & 1 & l-2 \end{vmatrix} = 0.$$

This is a particular case of the determinant,

$$\begin{vmatrix} p & q & r & s & \ldots\ldots \\ z & p & q & r & \ldots\ldots \\ y & z & p & q & \ldots\ldots \\ \ldots & \ldots & \ldots & \ldots & \ldots \\ \ldots\ldots & \ldots & z & p & q \\ \ldots\ldots & \ldots & y & z & p \end{vmatrix}$$

which, equated to zero, gives the result of elimination of θ between the equations

$$p + q\theta + r\theta^2 + \ldots\ldots\ldots + z\theta^{m-1} = 0\,,$$

$$\theta^m - 1 = 0\,.$$

Its factors are obviously to be found by substituting in succession the several m^{th} roots of unity in the expression

$$p + q\theta + \ldots\ldots\ldots + z\theta^{m-1}\,.$$

The form of its minors, on which depends the solution of the pendulum question, follows easily from these properties; and from them we in turn easily obtain the value of the same determinant when bordered, as it will be in the pendulum case if the series of magnets be finite and *not* closed. The question forms a very interesting illustration of the linear propagation of disturbances in a medium consisting of discrete, massive, particles—when only con tiguous ones act on one another. For, if we put

$$D = \epsilon^{a\frac{d}{dy}}\,,$$

and alter the value of μ, we have by taking a small,

$$\left[\left(\frac{d}{dt}\right)^2 + n^2 + \mu\left(\frac{d}{dy}\right)^2\right]x = 0;$$

which, with $n = 0$, is the usual equation for sound, provided the particles repel one another. Of course we can easily extend the investigation so as to include the more complex cases where the mutual actions of all the poles are taken into account. The result is not altered in form; but it might be curious to inquire whether the retention of n^2 in the equation might not give some hints as to the formation of a dynamical hypothesis of the action of transparent solids on the luminiferous ether. This, however, I cannot enter upon at present.

4. On Some Quaternion Integrals. Part II. By Professor Tait.

(*Abstract.*)

Commencing afresh with the fundamental integral

$$\iiint S \,.\, \nabla\sigma \, d\varsigma = \iint S \,.\, U\nu\sigma \, ds\,,$$

put

$$\sigma = u\beta$$

and we have

$$\iiint (S \,.\, \beta\nabla) u \, d\varsigma = \iint u \, S \,.\, \beta U\nu \, ds\,;$$

from which at once

$$\iiint \nabla u \, d\varsigma = \iint u U\nu \, ds\,, \qquad . \quad . \quad . \quad (a),$$

or

$$\iiint \nabla\tau \, d\varsigma = \iint U\nu \,.\, \tau ds\,. \qquad . \quad . \quad . \quad (b).$$

Putting $u_1\tau$ for τ, and taking the scalar, we have

$$\iiint (S(\tau\nabla) \,.\, u_1 + u_1 S \,.\, \nabla\tau) \, d\varsigma = \iint u_1 S \,.\, U\nu\tau \, ds$$

whence

$$\iiint (S(\tau\nabla)\sigma + \sigma S \,.\, \nabla\tau) \, d\varsigma = \iint \sigma \, S \,.\, U\nu\tau \, ds \qquad . \quad . \quad (c).$$

As one example of the important results derived from these simple formulæ, I take in this abstract the following, viz.:—

$$\iint V \,.\, (V \,.\, \sigma \, U\nu) \tau ds = \iint \sigma S \,.\, U\nu\tau \, ds - \iint U\nu S \,.\, \sigma\tau ds\,,$$

where by (*c*) and (*a*) we see that the right hand member may be written

$$= \iiint (S.(\tau\nabla)\sigma + \sigma S.\nabla\tau - \nabla S.\sigma\tau)\, ds$$
$$= -\iiint V.V(\nabla\sigma)\tau ds. \quad . \quad . \quad . \quad . \quad . \quad (d).$$

This, and similar formulæ, are applied in the paper to find the potential and vector-force due to various distributions of magnetism. To show how this is introduced, I briefly sketch the mode of expressing the potential of a distribution.

Let σ be the vector expressing the direction and intensity of magnetisation, per unit of volume, at the element ds. Then if the magnet be placed in a field of magnetic force whose potential is u, we have for its potential energy

$$E = -\iiint S(\sigma\nabla)u ds$$
$$= \iiint uS(\nabla\sigma)ds - \iint uS.U\nu\sigma ds.$$

This shows at once that the magnetism may be resolved into a volume-density $S(\nabla\sigma)$, and a surface-density $-S.U\nu\sigma$. Hence, for a solenoidal distribution,

$$S.\nabla\sigma = 0.$$

What Thomson has called a lamellar distribution (*Phil. Trans.* 1852), obviously requires that

$$S.\sigma d\rho\,,$$

be integrable without a factor; *i.e.*, that

$$V.\nabla\sigma = 0.$$

A complex lamellar distribution requires that the same expression be integrable by the aid of a factor. If this be u, we have at once

$$V.\nabla(u\sigma) = 0,$$

or

$$S.\sigma\nabla\sigma = 0.$$

With these preliminaries we see at once that (d) may be written

$$\iint \mathrm{V}.(\mathrm{V}.\sigma \mathrm{U}\nu)\tau ds = -\iiint \mathrm{V}.\tau \mathrm{V}.\nabla\sigma d\varsigma - \iiint \mathrm{V}.\sigma\nabla\tau d\varsigma + \iiint \mathrm{S}\alpha\nabla.\tau d\varsigma.$$

Now, if $\tau = \nabla\left(\frac{1}{r}\right)$, where r is the distance between any external point and the element $d\varsigma$, the last term on the right is the vector-force exerted by the magnet on a unit pole placed at the point. The second term on the right vanishes by Laplace's equation, and the first vanishes as above if the distribution of magnetism be lamellar, thus giving Thomson's result in the form of a surface integral.

Another of the applications made is to Ampère's *Directrice de l'action électrodynamique*, which (*Quarterly Math. Journal*, Jan. 1860) is the vector-integral

$$\int \frac{\mathrm{V}\rho d\rho}{\mathrm{T}\rho^3},$$

where $d\rho$ is an element of a closed circuit, and the integration extends round the circuit. This leads again to the consideration of relations between single and double integrals.

[Here it may be well to note that, by inadvertence, I wrote σ for τ towards the end of the abstract of the former part of this paper, thus giving the result a false generalisation depending on the fact that τ had been made subject to the condition

$$\mathrm{S}.\nabla\tau = 0,$$

while no such restriction was imposed on σ. With this restriction most of the results already given (*Proc. ante* p. 320) are correct, but the general forms in the paper itself are as follows, being deducible at once from the first expression in the abstract :—

$$\iint \mathrm{S}.\mathrm{U}\nu\nabla^2\sigma ds - \iint \mathrm{S}.\mathrm{U}\nu\nabla \mathrm{S}.\nabla\sigma ds = \int \mathrm{S}.\nabla\sigma d\rho,$$

and

$$\iint \mathrm{U}\nu\nabla^2 \mathrm{P} ds - \iint \mathrm{S}.\mathrm{U}\nu\nabla.\nabla \mathrm{P} ds = \int \mathrm{V}(d\rho\nabla)\mathrm{P};$$

giving finally

$$\iint \mathrm{V}.\mathrm{U}\nu\nabla^2\sigma ds - \iint \mathrm{S}.\mathrm{U}\nu\nabla.\mathrm{V}\nabla\sigma ds = \int \mathrm{V}.\mathrm{V}(d\rho\nabla)\sigma.]$$

Returning to the electrodynamic integral, note that it may be written

$$-\int \mathrm{V}\,.\,(d\rho\nabla)\,\frac{1}{r}\,,$$

so that, by the corrected formula just quoted, its value as a surface integral is

$$\iint \mathrm{S}\,.\,\mathrm{U}\nu\nabla\,.\,\nabla\,\frac{1}{r}\,ds - \iint \mathrm{U}\nu\nabla^2\,\frac{1}{r}\;ds.$$

Of this the last term vanishes, unless the origin is in, or infinitely near to, the surface over which the double integration extends. The value of the first term is seen (by what precedes) to be the vector-force due to uniform normal magnetisation of the same surface.

Also, since

$$\nabla\mathrm{U}\rho = -\,\frac{2}{\mathrm{T}\rho}\,,$$

we obtain at once

$$-\,2\iiint\frac{d\varsigma}{\mathrm{T}\rho} = \iint \mathrm{S}\,.\,\mathrm{U}\rho\mathrm{U}\nu ds\,,$$

whence, by differentiation, or by putting $\rho + \alpha$ for ρ, and expanding in ascending powers of $\mathrm{T}\alpha$ (both of which tacitly assume that the origin is external to the space integrated through, *i.e.*, that $\mathrm{T}\rho$ nowhere vanishes), we have

$$-\,2\iiint\frac{d\varsigma\mathrm{U}\rho}{\mathrm{T}\rho^2} = \iint\frac{\mathrm{V}.\mathrm{U}\rho\mathrm{V}.\,\mathrm{U}\nu\mathrm{U}\rho}{\mathrm{T}\rho}\;ds = 2\iint\frac{\mathrm{U}\nu ds}{\mathrm{T}\rho}\,;$$

and this, again, involves

$$\iint\frac{\mathrm{U}\nu ds}{\mathrm{T}\rho} = \iint\frac{\mathrm{U}\rho}{\mathrm{T}\rho}\,\mathrm{S}\,.\,\mathrm{U}\nu\mathrm{U}\rho ds\,.$$

The interpretation of these, and of more complex formulæ of a similar kind, leads to many curious theorems in attraction and in potentials. Thus, from (a) we have

$$\iiint\frac{\nabla t}{\mathrm{T}\rho}d\varsigma - \iiint\frac{t\mathrm{U}\rho}{\mathrm{T}\rho^2}d\varsigma = \iint\frac{t\mathrm{U}\nu}{\mathrm{T}\rho}ds\,,$$

which gives the attraction of a mass of density t in terms of the potentials of volume distributions and surface distributions. Putting

$$\sigma = it + jt_2 + kt_3,$$

this becomes

$$\iiint \frac{\nabla\sigma d\varsigma}{T\rho} - \iiint \frac{U\rho \,.\, \sigma d\varsigma}{T\rho^2} = \iint \frac{U\nu \,.\, \sigma ds}{T\rho}.$$

By putting $\sigma = \rho$, and taking the scalar, we recover a formula given above; and by taking the vector we have

$$V\iint U\nu U\rho ds = 0.$$

This may be easily verified from the formula

$$\int P d\rho = V\iint U\nu \,.\, \nabla P ds,$$

by remembering that

$$\nabla T\rho = U\rho.$$

Again if, in the fundamental integral, we put

$$\sigma = tU\rho,$$

we have

$$\iiint \frac{S(\rho\nabla)t}{T\rho} d\varsigma - 2\iiint \frac{t d\varsigma}{T\rho} = \iint tS \,.\, U\nu U\rho ds.$$

5. On the Currents produced by Contact of Wires of the same Metal at different Temperatures. By W. Durham, Esq. Communicated by Professor Tait.

At the suggestion of Professor Tait, I undertook the investigation of the momentary thermo-electric current developed when two conductors or wires of the *same* metal are brought into contact, the one being at a different temperature from the other.

Platinum was chosen as the most suitable metal to experiment with, in the first instance, as it is free from the interfering action of oxidation at high temperatures.

The following arrangement of apparatus was employed :—

1. A long iron bar, one of those used by the late Principal Forbes in his experiments on the conduction of heat, was heated at one end in the usual manner. This formed the source of heat at once steady and graduated, so that, by contact with it at various parts, the platinum wire experimented with could be kept at any required temperature.

2. Small glass tubes were fitted into holes in the bar at regular intervals, and turned over a little at the edge in the form of a lip. These served the double purpose of preventing metallic contact with the bar (and thus introducing ordinary thermo-electric currents), and also served as guides to the same point of contact in each experiment.

3. A small iron bar kept at the temperature of the room.

4. A reflecting galvanometer (with somewhat massive mirror and magnet, so as to "integrate"), with a scale placed at the distance of six feet, so that the smallest deflection of the needle could be readily observed and measured.

5. Two pieces of the same platinum wire connected with the galvanometer in the usual manner.

The mode of working was as follows :—The free end of one of the platinum wires rested on the small bar, and was thus kept at the temperature of the room. The free end of the other wire was placed in one of the glass tubes on the heated bar, and, while in that position, and after it had attained the temperature of the bar at that particular spot, the wire from the small bar was brought into contact with it, and the sudden deflection of the galvanometer needle noted.

With this arrangement very good and steady results were obtained when care was taken to keep the wires perfectly clean, and to apply the same amount of pressure in making contact in every experiment, because any deficiency of contact increased the resistance so as greatly to affect the currents.

The results show that for platinum wire the current, as indicated by the deflection of the galvanometer needle, is exactly as the difference of temperature between the two wires.

To show the steadiness of the results, I give the details of one experiment—

	Temperature of Hole.	Difference of Temperature.	Galvanometer Deflection.	Mean.
No. 1.	325°C.?	310° ?	215, 220, 225, 220, 225, 235, 240, 230, 240, 240, 237, 245, 235, 220, 250, 230, . .	= 231·7
2.	208°	193°	140, 140, 135, 130, 142, 130, 130, 130, 132, 128, 132, 130, 130, 135, 130, 132, 135, 140, 140, 140, 130, 135, 135, . .	= 134·
3.	144°	129°	90, 90, 90, 92, 90, 85, 85, 90, 85, 87, 85, 85, 90, 85, 80, 80, 90, 85, 90, 90, . . .	= 85·
4.	103°	88°	62, 60, 60, 60, 55, 60, 55, 60, 60, 60, 60, . . .	= 59·27
5.	78°	63°	42, 42, 44, 44, 44, 40, 50, 47, 50, 47, 50, . . .	= 45·5
6.	56°	41°	38, 35, 32, 30, 30, 32, 35, 35, 33, 35, 35, 35, 35, 35, 35, 38, 38, 35, 35, 38, . . .	= 34·7

The following are the means of a great number of experiments, the mean values of the current being all multiplied by a common factor :—

No. 1.		No. 2.		No. 3.	
Difference of Temperature in Degrees Cent.	Current.	Difference of Temperature in Degrees Cent.	Current.	Difference of Temperature in Degrees Cent.	Current.
21°	19·	50°	55·5	9°	9·6
30°	30·	53°	54·5	14°	13·
42°	33·3	63°	68·	20°	19·
60°	59·	68°	70·	28°	26·
88°	89·	74°	73·	39°	34·
92°	90·	88°	89·	61°	65·
134°	132·5	105°	101·	84°	76·
136°	135·	109°	105·	124°	120·
139°	138·	129°	127·	131°	120· ?
140°	142·	152°	120· ?	196°	192·
		167°	161·5	?	314·
		193°	201·		
		?	266·		
		?	347·		

With the same apparatus as in the foregoing, I next tried heating *both* wires considerably above the temperature of the room,

till, however, keeping one wire at a higher temperature than the other. The result in this case was as in the former. The current was exactly as the difference of temperature. The following are the means of the experiment:—

Temperatures in Degrees Cent.	Current.
203° — 142° = 61°	64·5
142° — 100° = 42°	48·
100° — 76° = 24°	30·

With more sensitive galvanometer,—

320°? — 205° = 115°?	120· *
205° — 143° = 62°	64·5
143° — 102° = 41°	42·
102° — 76° = 26°	28·5

6. Remarks on the Deep-Water Temperature of Lochs Lomond, Katrine, and Tay. By Alexander Buchan.

In the communications made by Sir Robert Christison to the Society in December and April last on the deep-water temperature of Loch Lomond, from observations made by him with a Miller-Casilla thermometer, these important facts were stated:—

(1.) On 12th October 1871, the temperature at the surface was 52·°0, from which it fell, on descending, till at 300 feet below the surface it stood at 42°·0, and this temperature of 42°·0 was uniformly maintained at greater depths or to 518 feet, the depth of the loch at the place of observation.

(2). On 18th November following, the surface temperature was 46°·0; at depth of 250 feet, 42°·25; at 270 feet and lower depths, 42°·0.

(3.) On the 10th April 1872, the temperature at the surface was 43°·0; at 150 feet, 42°·1; and from 200 to 594 feet, 42°·0.

Hence it appears that there is a stratum of water of considerable thickness at the bottom of this loch of uniform temperature; that the upper surface of this stratum of deep water of uniform temperature was about 100 higher on the 10th of April than it was in the

* Results varied considerably owing to working so near the flame—varying from 104° to 126°·

beginning of winter, or on the 18th November; and that this deep water temperature probably remains constantly at, or very near, 42° 0.

Sir Robert asked me for a statement of the temperature of the air at Loch Lomond from 18th November 1871 to 10th April 1872, or during the time that the cold stratum of water of the uniform temperature of 42°·0 had increased about 100 feet in thickness. This I have prepared from the observations made at Balloch Castle, by Mr David Hill, the observer of the Scottish Meteorological Society at that place. Balloch Castle is at the foot of the loch, and 72 feet above its surface. The table showed the mean temperature of each day during the time,—the mean of the maximum and minimum temperatures of each day being assumed as the mean temperature of that day. Of this table an abstract is given below, from which it appears that the mean temperature, from

November	18	to	30	was	38°·0,	or	2°·5	below	the average,
December	1	,,	31	,,	39°·4,	,,	0°·4	,,	,,
January	1	,,	31	,,	40°·8,	,,	2°·3	above	,,
February	1	,,	29	,,	43°·3,	,,	3°·3	,,	,,
March	1	,,	31	,,	43°·6,	,,	2°·1	,,	,,
April	1	,,	10	,,	45°·6,	,,	1°·4	,,	,,

The average temperature of the 145 days was 41·7, which 1°·4 above the average of past years.

Taking the observed mean temperature of each day for Edinburgh as calculated by the late Principal Forbes,* and applying to these the differences observed between Balloch Castle and Edinburgh, the normal temperature of each day at Balloch Castle was calculated. In this way the divergence of the temperature of each of the 145 days from its normal was ascertained. The amount for each day was given in a table,—temperatures above the average being given in red ink, under the average in blue. An abstract of this table is given below, from which it appears that there were four cold, and four mild periods, as under:—

* Trans. of the Society, vol. xxii. p. 351.

Cold Periods.

November 18	to	December 10,	or	23 days,	4°·6	under average,
December 20	„	„ 23,	„	4 „	3°·9	„
January 5	„	January 10,	„	6 „	1°·0	„
March 20	„	April 6,	„	18 „	3°·0	„
		Average,		51 days,	3°·4	„

Mild Periods.

December 11	to	December 19,	or	9 days,	4°·1	above average,
„ 24	„	January 4,	„	12 „	3°·5	„
January 11	„	March 19,	„	69 „	3°·9	„
April 7	„	April 10,	„	4 „	6°·0	„
		Average,		94 days,	4°·0	„

Hence during this period the temperature was under the average of the season on 51 days, the deficiency amounting to a mean of 3°·4; and above the average on 94 days, the excess amounting to a mean of 4°·0. The most markedly mild period extended over 69 days, viz., from 11th January to 19th March, during which the temperature was on an average of 3°·9 above that of the season; and as already stated, the temperature was, for the whole period of 145 days, 1°·4 above the average.

It may be concluded that in ordinary winters the stratum of water of uniform temperature will be thicker than Sir Robert Christison found it to be this year in the beginning of spring; in other words, that it will be nearer the surface than 170 feet.

In the end of last week, Mr James Leslie, C.E., kindly sent me some highly interesting and valuable observations on the deep-water temperature of Lochs Tay, Katrine, and Lomond, made by the late Mr James Jardine, C.E., in 1812 and 1814. These I have now very great pleasure in laying before the Society. They were taken in fathoms, and the temperature in degrees centigrade which are here reduced to Eng. feet, and degrees Fah.

* The general results of these observations were given by Sir John Leslie in his "Treatises on Various Subjects of Natural and Chemical Philosophy," Edinburgh 1838, p. 281.

Observations of the Deep-Water Temperature of Lochs Tay, Katrine, and Lomond, by the late James Jardine, Esq., C.E.

Depth.	Loch Tay. Aug. 12, 1812.	Loch Katrine. Sept. 3, 1814.	Loch Katrine. Sept. 7, 1812.	Loch Lomond Sept. 8, 1812.
Surface	57°·2	56°·8	57°·9	59°·5
30 feet	...	56°·7	...	...
60 "	...	49°·6	50°·9	...
90 "	...	45°·5	...	44°·1
120 "	...	44°·4	43°·5	...
150 "	...	43°·3	...	...
180 "	...	42°·3	...	...
210 "	43°·2	...	41°·5	...
240 "	...	...	...	41°·7
300 "	...	...	41°·5	...
360 "	...	...	41°·5	...
420 "	41°·9	...	...	...
480 "	...	41°·7	41°·4	41°·7
540 "	...	...	...	41°·5
600 "	...	...	...	41°·5

These results are strikingly accordant with those obtained by Sir Robert Christison, The difference as regards the deep-water temperature of Loch Lomond may be, and probably is, only instrumental.

These observations were made in the summer and early autumn, or when the temperature of the sea and of lakes is about the annual maximum. Taken in connection with Sir Robert's observations, they warrant the conclusion that the deep-water temperature of Loch Lomond remains during the whole year either absolutely at, or very nearly at, the low figure of 42°·0.

The observations also show that this is not a peculiarity of Loch Lomond, but that it is also a characteristic of Lochs Katrine and Tay, and most probably of other deep waters.

The mean annual temperature of the air at Loch Lomond, from the mean at Balloch Castle, calculated on the 13 years' average, ending 1869, is 48°·0,* which is 6°·0 higher than the uniform deep-water temperature of the loch. The deep-water temperature

* In this and following temperatures 0°·2 has been added, in order to bring them to the level of the loch, which is 72 feet lower than the thermometers at Balloch Castle.

is, therefore, not determined by the mean annual temperature of air over this part of the earth's surface.

From Forbes' "Climate of Edinburgh," it is seen that the temperature there is under the annual mean from the 21st October to the 26th April. Assuming that this holds good for Balloch Castle, then the mean temperature for the cold half of the year will be, from—

October	21 to 31, . . .	46°·0
November	1 to 30, . . .	41°·7
December	1 to 31, . . .	40°·9
January	1 to 31, . . .	38°·6
February	1 to 28, . . .	39°·8
March	1 to 31, . . .	40°·5
April	1 to 26, . . .	45°·8

The mean of these 188 days is therefore 41°·4.

The close approximation of this temperature of 41°·4 to 42°·0, the deep-water temperature of the loch, is such as to suggest *that it is the mean temperature of the cold half of the year which determines the temperature of the lowest stratum of water at the bottom of deep lakes*, so long as the deep-water temperature does not fall below that of the maximum density of the water. As this principle, if established, would be of great importance in many questions of physical research, such as the deep-water temperature of the Mediterranean Sea, which Dr Carpenter has very accurately ascertained, in its connection with the larger question of general oceanic circulation, it well deserves further investigation.

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